Take-up and Targeting: Experimental Evidence from SNAP

Amy Finkelstein and Matthew J. Notowidigdo*

April 2019

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 have higher net income and are less sick than the average enrollee in the control group. We present evidence consistent with the existence of optimization frictions that are greater for needier individuals, which suggests that the poor targeting properties of the interventions reduce their welfare benefits.

JEL codes: C93; H53; I38

Keywords: SNAP; Food Stamps; Take-Up; Targeting; Welfare.

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

I Introduction

Enrollment in U.S. social safety net programs is not automatic: individuals must apply and demonstrate eligibility. Often, eligibility rules are complicated, application forms long, and documentation requirements substantial. Perhaps as a result, incomplete take-up is pervasive (Currie, 2006). Two typical explanations 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 for a given amount of redistribution to occur at lower public cost (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 2018, Bhargava and Manoli 2015, Guyton et al. 2016, Manoli and Turner 2016), Social Security Disability Insurance (Armour, 2018), post-secondary enrollment (Barr and Turner 2018, Bettinger at al. 2012, Dynarski et al. 2018), energy efficiency audits (Alcott and Greestone 2017) and SNAP (Daponte et al. 1999). Studies of assistance interventions

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

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 model 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 allow the government to redistribute to a given amount to lower ability types at lower public cost . Our key extension is - in the spirit of the behavioral literature - to allow for the possibility that individuals may not make privately optimal application decisions. The private welfare gains for marginal enrollees therefore depend on the size of their behavioral biases, which may vary (with unknown sign) by type. In addition, we allow for a flexible relationship between the individual's type and the fiscal externality from her enrollment on the government budget. Thus, for a given enrollment response to an intervention, the welfare implications of its targeting properties depend on the relative behavioral biases across types and the relative fiscal externalities across types. These are empirical questions.

To explore these issues empirically, we examine the impact of various interventions on the number and type of eligible elderly individuals who enroll in SNAP, one of the most important social safety net programs in the United States. It is the only benefit that is virtually universally available to low-income households. During the Great Recession, as many as one in seven individuals received SNAP (Ganong and Liebman, 2018). In 2015, public expenditures on SNAP were about $70 billion, roughly the same amount as the Earned Income Tax Credit (EITC) and higher than the $60 billion spent on SSI or the $30 billion spent on cash welfare (TANF).[2] Although the elderly, who are the focus of our study, are only 10 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 transforming how individuals in need access public benefits. We constructed a study population of

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

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 results. First, information alone increases enrollment, while information combined with assistance increases enrollment even more, but at a higher cost per enrollee. Nine months after the intervention - at which point the initial impact appears to be fully in place - enrollment is 6 percentage points in the control arm compared to 11 percentage points in the Information Only arm and 18 percentage points in the Information Plus Assistance arm; these enrollment rates are all statistically distinguishable ($p < 0.001$). 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 - as well as social - welfare gains from each intervention. Our estimates also suggest that underestimation of expected benefits is greater for needier individuals, again consistent with leading behavioral theories (e.g., Bertrand 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 II presents our framework. Section III provides background information on SNAP. Section IV describes our experimental design and data. Section V presents the experimental results. Section VI uses the results to calibrate the model from Section II and perform welfare analysis of the interventions. All appendix material is presented in the on-line appendices.

II Framework

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

II.A Model setup

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

Individuals choose hours of work h_j (which produces labor income $\theta_j h_j$) and whether 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 ϵ_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 κ_j is how the utility cost varies with $\bar{\Lambda}$ for individuals of type j. This formulation nests ordeals that impose a greater utility cost on H types ($\kappa_H > \kappa_L$) or on L types ($\kappa_L > \kappa_H$). The former includes utility costs $\kappa_j = \theta_j$, which might correspond to a common time cost that has higher utility costs for H types due to higher wages (e.g. Nichols and Zeckhauser 1982). The latter includes L types having more difficulties filling out forms (e.g. Bertrand 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 θ_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.

II.B Social Welfare Effects of Interventions and Targeting

We model two alternative interventions corresponding to the two main treatment arms in the experiment. In the **Information Only treatment**, the treatment increases the perceived benefits of applying ($d\epsilon$). 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 μ_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 ξ_j term denotes the marginal utility of consumption for type j individuals who choose to apply.

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

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

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

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

Note that we abstract away from potential income effects of the interventions on inframarginal applicants. These generate additional terms without qualitatively changing the main insights of the model; for completeness, we provide the terms in Appendix A.1.1. We also implicitly assume in our discussion that $u'(x_j) > 1$ for $j \in \{H, L\}$, so that the marginal utility of consumption from B exceeds the mechanical program cost for both types.[4]

For the Information Only intervention, the above expressions indicate that in the "neoclassical benchmark" ($\epsilon_j = 0$), the intervention has no effect on private welfare, since $\mu_L = \mu_H = 0$. Intuitively, since individual decisions are already privately optimal, the marginal individual is indifferent between applying and not applying, and therefore a change in behavior has no first order impact on their private welfare. If, however, individuals under-estimate the benefit of applying (i.e., $\epsilon_j < 0$), the intervention increases private welfare for marginal applicants of each type by μ_j, with the increase in private welfare increasing in the amount of 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 ϵ_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 $de/dT > 0$. 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 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

[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.

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 (forthcoming).

However, our framework shows that once we depart from the neoclassical benchmark, the relationship between the targeting properties of the intervention and the social welfare impact of the intervention breaks down. With misperceptions ($\epsilon_j \neq 0$), the change in social welfare from a change in targeting is increasing in $(\mu_L - \mu_H)$; μ_L enters positively while μ_H enters negatively because the thought experiment of increasing targeting "swaps" an H applicant for an L applicant. With $\epsilon_j < 0$, μ_j is increasing in two type-specific factors: the marginal utility of consumption (ξ_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.[5] As a result, even in the absence of behavioral frictions, interventions that improve targeting are not necessarily better for social welfare - it depends on the relative magnitudes of the fiscal externalities generated from enrollment by different types.

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

Extensions

In Appendix A.2 we show that the core propositions are robust to alternative modeling choices about the nature of misperceptions and "mistakes". We also consider non-marginal changes, similar in spirit to Kleven (2018). 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 enrollees who would not have enrolled absent the intervention can no longer be zeroed out by the envelope theorem. Insteaad, their increase in private welfare now depends on the shape of the type-specific cost distribution $f_j(c)$; thus, the cost distribution functions introduce another "free parameter" that can affect the relationship between improvements in targeting and changes in social welfare, much as the misperception terms do when we depart from the neoclassical benchmark.

III Setting and Background

SNAP is the second-largest means-tested program in the United States (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 in 2016. Take-up of SNAP in Pennsylvania is similar to the nationwide estimates (Cunnyngham 2015). Appendix B provides more details on the program for our study participants; we summarize a few key features here.

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

To enroll in SNAP, an individual must complete an application, provide the necessary documents verifying household circumstances, and participate in an interview (phone or in person). The applicant must provide identifying information about herself and each household member, information on resources and income, and information on various household expenses such as medical expenses, rent and utilities. She must provide documentation verifying identity, proof of residency, and proof of earnings, income, resources and expenses. Applications can be submitted by mail, fax, in person at the County Assistance Office, or 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.

IV Empirical Design and Data

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

IV.A 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 $119 a month to buy healthy food"), and provide information on how to apply ("We want to help you apply for SNAP!"), offering a number at BDT to call ("Please call the PA Benefits Center today. It could save you hundreds of dollars each year"). These materials are written in simple, clear language for a 4th to 6th grade reading level and are sent from the Secretary of the Pennsylvania Department of Human Services. Appendix Figure A1 shows these standard outreach materials. In the framework of Section II, we think of this intervention as increasing the perceived benefits from applying ($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). In the framework of Section II, we think of this intervention as reducing the private costs of applying ($-d\bar{\Lambda}$).

Data from our intervention indicate that BDT submitted about 70 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.[6] For callers who end up applying, BDT spends on average 47 minutes on the phone with them; for callers who end up not applying, the average phone time is about 30 minutes.

[6] 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.

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 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.

IV.B Study Population

Our study population consists of individuals aged 60 and older who are enrolled in Medicaid but not SNAP. They are considered likely income eligible for SNAP based on their enrollment (and hence eligibility) for Medicaid. This is, of course, an imperfect proxy of SNAP eligibility. This is by necessity; as described in detail in Appendix B, 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 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. 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 I 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), and excluded all individuals to whom BDT had previously sent any outreach materials (N=26,155).

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

IV.C Randomization

We randomly assigned the 31,888 individuals in our study population to one of three equally-sized groups: Information Only treatment, Information Plus Assistance treatment, and control (no intervention). There were separate 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").

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.[7] 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 Department of Human Services (DHS) before the study was launched. MIT's Institutional review board (IRB) approved this research (Protocol: 1506106206; FWA: 00004881).[8] The trial was registered on the AEA RCT Registry (AEA RCTR -0000902) in October 2015, prior to our launch - at which point we pre-specified our primary and secondary outcomes. 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.[9]

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

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

[9] Our analysis hews closely to the analysis plan in terms of the take-up outcomes analyzed (calls, applications, and enrollment) and the analysis of enrollee benefits and enrollee and applicant demographic and health characteristics. The exact analysis of study participant characteristics was not fully specified at that point due to uncertainty on

IV.D 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.

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 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.

data availability. We were unable to execute on our aspirations to analyze additional characteristics like earnings and credit report outcomes due to our inability to obtain the relevant data.

V 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.

V.A Behavioral Responses to Intervention

Enrollment, applications, and calls

Table II 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 ($p<0.001$).[10]

Figure I 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; Appendix Figure A7 shows similar monthly patterns for applications and calls.

The next two rows of Table II 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. This suggests that assistance affects enrollment (over and above information alone) primarily by affecting individuals' willingness to apply, rather than by increasing the success (i.e., approval) rate of a given application. This is consistent with other studies that have found that changes in transaction costs

[10] For some perspective on these numbers, we considered how they compared to other take-up interventions, bearing in mind that these were different interventions conducted on different programs and populations. In the context of encouraging low-income high school seniors to apply for aid and attend college, Bettinger et al. (2012) found that providing information about aid eligibility and nearby colleges had no detectable effect, but combining the information with assistance in completing a streamlined application process increased college enrollment by 8 percentage points or about 25 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).

have no or small effects on rejection rates of applicants (Deshpande and Li forthcoming, Alatas et al. 2016). 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 II examine call-in rates. A caller is defined as someone calling the number provided on the outreach material; the caller rate is therefore mechanically zero for those in the control arm.[11] 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 IV.D above) is 29 percent, and statistically indistinguishable from the call-in rate for Information Plus Assistance. The similar call-in rate is not surprising given the (deliberate) similarity of the outreach materials (see Appendix Figures A1 and A2). It suggests that any difference in applications and enrollment between the Information Only and Information Plus Assistance interventions is attributable to the assistance itself, rather than to the expectation of assistance. Conditional on calling, we find the average caller made 1.8 calls in the Information Plus Assistance arm and 1.6 calls in the Information Only arm (results not shown); these differences are statistically distinguishable ($p<0.001$). BDT employees are assigned to callers using a rotation system which is plausibly quasi-random. Using a non-parametric Empirical Bayes approach, we found no evidence of statistically significant differences in treatment effects on applications or enrollment across employees. We also found no evidence of differential impact based on employee observable characteristics (see Appendix Tables A18 and A19).

The table also shows that the the share of people who apply or enroll without calling is the same in all three arms. This suggests that all marginal applicants affected by the interventions call in response to the outreach materials: Such individuals presumably call the state directly (without being routed through BDT or our tracking service), 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

[11] 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.

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

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.[12] 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

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

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 III 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.[13] Reminders matter: all behavioral responses decrease by about 20 percent without the reminder postcard. Specifically, the "standard" Information Only 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 II, this is modeled as under-estimating the benefits of applying (i.e., $\epsilon < 0$).

V.B Characteristics of Marginal Applicants and Enrollees

To examine the characteristics of the marginal applicant or enrollee whose behavior is affected by the intervention, we define the outcome in each arm to be the average of a specific characteristic among those who apply or enroll. For example, we compare the average monthly benefits among those who enroll in each arm. Differences in the average characteristics of enrollees or applicants in a given treatment arm relative to the control group 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

[13] The results for the Information Only treatment results shown in Table II pool the results from the standard treatment and a "marketing" sub-treatment that varied the content of the outreach letters (see 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 II.

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 IV 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 ($p = 0.013$).[14]

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 IV shows that the interventions increased the share of enrollees receiving the minimum benefit and decreased the share of enrollees receiving the maximum benefit.

We explored two potential concerns with these results. First, we are missing benefit information for about 4 percent of enrollees, presumably due to data errors. Importantly, Table IV 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 E that the differences in benefits across the arms is robust to using the fairly conservative procedure of Lee (2009) to bound the potential bias arising from differential missing benefit rates. We also generated a predicted benefit measure in which we predict the benefit amounts based on the relationship between benefits and the pre-randomization demographic and health characteristics shown in Table I; Appendix E provides

[14]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). 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.

more detail on the prediction algorithm which follows a standard algorithm in machine learning (Rifkin and Klautau 2004). Table IV shows that predicted benefits show the same pattern across arms as actual benefits, 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 IV shows that the interventions increase the share of enrollees who are in a household size of 1. However, the bottom row of Table IV shows that if we limit our analysis to households of size 1, average benefits for these households are still statistically significantly lower in each intervention arm relative to the control. An additional attraction of limiting to households with only a single individual is that we have essentially no missing benefits for such households.

Demographics and health of applicants and enrollees

Table V shows the demographic and health characteristics of applicants and enrollees. On a variety of dimensions, marginal applicants and enrollees from the intervention appear less needy than the average applicant or enrollee in the control group. Panel A shows that applicants and enrollees in either intervention have lower predicted benefits (i.e., have higher predicted net resources) than applicants in the control arm ($p < 0.001$).

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.[15] Total medical spending is noisy - due to the well-known high variance of medical spending - and conflates variation in utilization with variation in recorded prices. Our total number of days or visits measures attempt to circumvent 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.[16]

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

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

[16] 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).

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 II, the observable socio-economic characteristics of those targeted by the intervention are neither necessary nor sufficient for normative analysis, a point we return to when we explore the normative implications of our findings in Section VI below.

Out-of-sample implications: non-caller characteristics

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

Table VI 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 more needy on all dimensions: they have higher predicted benefits, higher health care spending and use, and more chronic conditions.[17] 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. Our interventions do not, however, appear to affect their behavior. An open question is whether there are other interventions that would.

VI Normative implications

As noted in the Introduction, there is an active empirical literature studying take up and targeting in other programs. We discuss this literaure in more detail in Appendix F. Importantly, it has been primarily descriptive, examining whether interventions designed to increase enrollment tend

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

to attract individuals who are observably worse off than those who would enroll in the absence of the intervention.

However, our framework in Section II emphasized that there is no general relationship between targeting on observables and the normative implications of the interventions. It also provided additional conditions that need to be examined empirically 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).

VI.A Conceptual framework mapped to our context

We tailor the framework from Section II 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 π_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 IV, a mass of individuals with sufficiently high net resources receive the minimum benefit B_{min}, and others with lower net resources receive higher benefits (which for simplicity we average together).

In addition, given the partial equilibrium nature of the intervention and the elderly study population, we assume that earnings do not respond endogenously to our intervention, although of course in 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 II, the framework and propositions developed apply generally to any fiscal externality. Importantly, however, without endogenous earnings, the level of benefits that individuals receive is determined by their type, with low-ability types receiving higher benefits $\overline{B}$ and high-ability types receiving the minimum level of benefits B_{min}. This suggests a natural empirical definition of targeting based on the level of benefits received: $e = E_L/(E_L + E_H)$. 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*:

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

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

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

Proof: See Appendix A.3

Proposition 2a. *Holding constant the change in applications due to an intervention, the change in social welfare in response to an improvement in targeting* ($de/dT > 0$) *from an Information Only (or Information Plus Assistant) treatment is given by the following expression*:

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

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

Proof: See Appendix A.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.

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

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

[18]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.

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.

VI.C 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 ϵ (which we will also relax below) that allows us to approximate μ_H as $\xi_H \pi_H \epsilon_H B_{min}$, and μ_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 μ_j into a change in dollars of surplus for each type (rather than the dollar surplus for a typical individual in the population - see 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. 2019 for more discussion). Given our normalization, the MVPF represents the dollars of surplus transferred to each type (measured in that type's own money metric), divided by the total fiscal cost (in dollars) of the intervention. With these changes, we can write:

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

Appendix Section A.3.1 provides the derivation.

We previously parameterized $g \sim \$267$, $\pi_L\overline{B} \sim \$4,806$, $\pi_H B_{min} \sim \$432$, $\epsilon_L \sim -0.98$, 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 II shows directly the increase in applications - for the Information Only intervention, $\frac{dA}{dT} = 0.07$ and for the Information Plus Assistance intervention, $\frac{dA}{dT} = 0.16$. 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 II 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.[19] An MVPF below 1 is to be expected for a redistributive policy such as SNAP; redistribution inevitably involves some resource cost (Okun 1975).

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

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

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

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.

Appendix A.3.2 describes an analagous calculation for the Information Plus Assistance Intervention. 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; indeed, we calculate that costlessly eliminating private application costs (i.e., reducing them from $75 per application to zero) would increase the MVPF from 0.89 in the Information Only intervention to 0.93. Naturally, the specific numbers we calculate will be sensitive to the assumptions we have made. Appendix A.3.3 therefore briefly explores sensitivity of our results to some key alternative assumptions, with the goal of providing insight into the determinants of the welfare impacts of the intervention.

VII Conclusion

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 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.

MIT, NBER, AND J-PAL NORTH AMERICA
NORTHWESTERN, NBER, AND J-PAL NORTH AMERICA

VIII References

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

Abadie, Alberto, "Semiparametric instrumental variable estimation of treatment response models", *Journal of Econometrics,* 113 (2003), 231-263.

Aaronson, Becca, “Number of Texans Receiving Food Stamps Up Sharply Amid Recession,” *Texas Tribune*, 11/10/2011. URL http://www.texastribune.org/library/data/texas-supplemental-nutrition-assistance-program/ (accessed 2.3.15).

Alatas Vivi, Abhijit Banerjee, Rema Hanna, Benjamin Olken, Matthew Wai-Poi, and Ririn

Pernamasari, “Self-Targeting: Evidence from a Field Experiment in Indonesia,” *Journal of Political Economy.* 124 (2016), 371-427.

Almond, Douglas, Hilary W. Hoynes, and Diane Whitmore Schanzenbach, “Inside the War on Poverty: The Impact of Food Stamps on Birth Outcomes,” *The Review of Economics and Statistics*, 93 (2011), 387-403.

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

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

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

Benefits Data Trust, 2016. About: Our Model.

Bartlett, Susan, Nancy Burstein, and William Hamilton, “Food Stamp Access Study: Final Report,” United States Department of Agriculture, Economic Research Service, November, 2004. https://www.ers.usda.gov/publications/pub-details/?pubid=43407 (last accessed July 31, 2017).

Berkowitz Seth A., Hilary K. Seligman, Joseph Rigdon, James B. Meigs, and Sanjay Basu, “Supplemental Nutrition Assistance Program (SNAP) Participation and Health Care Expenditures Among Low-Income Adults,” *JAMA Internal Medicine*, 177 (2017), 1642-1649.

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

Besley, Timothy, and Stephen Coate, “Workfare versus Welfare: Incentive Arguments for Work Requirements in Poverty-Alleviation Programs,” *American Economic Review*, 82 (1992), 249–61.

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

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

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

Cutler, David, and Jonathan Gruber, “Does Public Insurance Crowd out Private Insurance?” *Quarterly Journal of Economics,* 111 (1996), 391-430.

Center on Budget and Policy Priorities, “SNAP Online: A Review of State Government SNAP Websites,” 2016. http://www.cbpp.org/research/food-assistance/snap-online-a-review-of-state-government-snap-websites. Last accessed, June 29 2017.

Center on Budget and Policy Priorities, “SNAP Helps Millions of Low-Income Seniors,” (2017)

https://www.cbpp.org/research/food-assistance/snap-helps-millions-of-low-income-seniors

Cunnyngham, Karen, "Estimates of State Supplemental Nutrition Assistance Program Participation Rates in 2012," *Mathematical Policy Research*, 2015.https://fns-prod.azureedge.net/sites/default/files/ops/Reaching2012.pdf.LastaccessedFebruary6,2019

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

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

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

Deshpande, Manasi, and Yue Li, "Who Is Screened Out? Application Costs and the Targeting of Disability Programs," *American Economic Journal: Economic Policy*, forthcoming.

Dynarski Susan, C. J. Libassi, Katherine Michelmore, and Stephanie Owen, "Closing the Gap: The Effect of a Targeted, Tuition-Free Promise on College Choices of High-Achieving, Low-Income Students," NBER Working Paper 25349, 2018.

Einav, Liran, Amy Finkelstein, and Mark R. Cullen, "Estimating Welfare in Insurance Markets using Variation in Prices," *Quarterly Journal of Economics*, 125 (2010), 877-921.

Eslami, Esa, "Characteristics of Elderly Individuals Participating in and Eligible for SNAP," *Mathematica Policy Research*, 2016. https://www.mathematica-mpr.com/download-media?MediaItemId={DB17EFFA-E155-48C0-9E2D-CF90C59BCDDD}.LastaccessedFebruary6,2019

Finkelstein Amy, Matthew Gentzkow, and Heidi Williams, "Sources of Geographic Variation in Health Care: Evidence from Patient Migration," *Quarterly Journal of Economics*, 131(2016), 1681-1726.

Finkelstein, Amy, Nathaniel Hendren, and Mark Shepard, "Subsidizing Health Insurance for Low-Income Adults: Evidence from Massachusetts," *American Economic Review*, 109 (2019), 1530-1567.

Ganong, Peter, and Jeffrey B. Liebman, "The Decline, Rebound, and Further Rise in SNAP Enrollment: Disentangling Business Cycle Fluctuations and Policy Changes," *American Economic Journal: Economic Policy*, 10 (2018), 153-176.

Gray, Colin, "Why Leave Benefits on the Table? Evidence from SNAP," Upjohn Institute Working Paper, 2018.

Gruber, Jonathan, Philip B. Levine, and Douglas Staiger, "Abortion Legalization and Child Living Circumstances: Who is the Marginal Child?" *Quarterly Journal of Economics*, 114 (1999), 263–292.

Guyton, John, Dayanand Manoli, Brenda Schafer, and Michael Sebastiani, "Reminders & Recidivism: Evidence from Tax Filing & EITC Participation Among Low-Income Non-Filers," NBER Working Paper 21904, 2016.

Hanratty, Maria J., "Has the Food Stamp Program become more accessible? Impacts of recent

changes in reporting requirements and asset eligibility limits," *Journal of Policy Analysis and Management*, 25 (2016), 603-621.

Hastings, Justine, and Jesse Shapiro, "How Are SNAP Benefits Spent? Evidence from a Retail Panel," *American Economic Review,* 108 (2018), 3493-3540.

Hendren, Nathaniel, "The Policy Elasticity," *Tax Policy and the Economy,* 30 (2016), 51-89.

Hoynes, Hilary W., and Diane Whitmore Schanzenbach, "Work Incentives and the Food Stamp Program," *Journal of Public Economics,* 96 (2012), 151–162.

Hoynes, Hilary W., and Diane Whitmore Schanzenbach. 2016. U.S. Food and Nutrition Programs. in Robert Moffitt (ed) "Means Tested Transfer Programs in the United States, Volume II." University of Chicago Press.

Hu, Winnie, "Rations Reduced as Demand Grows for Soup Kitchens," *New York Times*, 6/27/2014.

Isaacs, Julia, "The Costs of Benefit Delivery in the Food Stamp Program." USDA Contractor and Cooperator Report No. 39., 2008. `https://www.brookings.edu/wp-content/uploads/2016/06/03_food_stamp_isaacs.pdf.LastaccessedJune29,2017`

Kabbani, Nader S. and Parke E. Wilde, "Short Recertification Periods in the U.S. Food Stamp Program," *Journal of Human Resources*, 38 (2003), 1112-1138.

Kauff, Jaqueline, Lisa Dragoset, Elizabeth Clary, Elizabeth Laird, Libby Makowsky, and Emma Samaa-Miller, "Reaching the Underserved Elderly and Working Poor in SNAP: Evaluation Findings from the Fiscal Year 2009 Pilots," Mathematica Policy Research, 2014.

Klerman, Jacob A., and Caroline Danielson, "The Transformation of the Supplemental Nutrition Assistance Program," *Journal of Policy Analysis and Management*, 30 (2011), 863-888.

Kleven, Henrik J., and Wojiciech Kopczuk, "Transfer Program Complexity and the Take Up of Social Benefits." *American Economic Journal: Economic Policy,* 3 (2011), 54-90.

Kleven, Henrik J, "Sufficeint Statistics Revisited," Working Paper, 2018. `https://www.henrikkleven.com/uploads/3/7/3/1/37310663/kleven_sufficientstats_march2018.pdf`

Lee, David, "Training, Wages, and Sample Selection: Estimating Sharp Bounds on Treatment Effects," *Review of Economic Studies*, 76 (2009), 1071-1102.

Madrian, Brigitte, and Dennis Shea, "The Power of Suggestion: Inertia in 401(k) Participation and Savings Behavior" *Quarterly Journal of Economics*, 116 (2001), 1149-1187.

Manchester, Colleen F., and Kevin J. Mumford, "How Costly is Welfare Stigma? Separating Psychological Costs from Time Costs in Food Assistance Programs," Working Paper, 2012. `https://krannert.purdue.edu/faculty/kjmumfor/papers/stigma.pdf`

Mani, Anandi, Sendhil Mullainathan, Eldar Shafir, and Jiaying Zhao, "Poverty Impedes Cognitive Function," *Science,* 341 (2013), 976–980.

Manoli, Dayand, and Nicholas Turner, "Nudges and Learning: Evidence from Informational Interventions for Low-Income Taxpayers," NBER Working Paper 20718, 2016.

Moffitt, Robert, "An Economic Model of Welfare Stigma," *American Economic Review,* 73 (1983), 1023-1035.

Mullainathan, Sendhil, and Eldar Shafir, Scarcity: Why Having Too Little Means So Much, (New York, NY: Henry Hold and Company, 2013).

Nichols, Albert L., and Richard J. Zeckhauser, "Targeting Transfers through Restrictions on Recipients," *American Economic Review*, 72 (1982), 372-377.

Nichols, Donald A., Eugene Smolensky, T. Nicolaus Tideman, "Discrimination by Waiting Time in Merit Goods*,"* *American Economic Review*, 61 (1971), 312–323.

Okun, Arthur M., Equality and Efficiency, The Big Tradeoff, (Washington D.C., Brookings Institution Press, 1975).

Pennsylvania Department of Human Services. Online. "Supplemental Nutrition Assistance Program Policy Manual," http://services.dpw.state.pa.us/oimpolicymanuals/snap/ (last accessed June 28, 2017).

Ponza, Michael, James C. Ohls, Lorenzo Moreno, Amy Zambrowski, and Rhoda Cohen, "Customer Service in the Food Stamp Program," Mathematica Policy Research Inc., Reference Number 8243-140, 1999.

Ratcliffe, Caroline, Signe-Mary McKernan, and Kenneth Finegold, "Effects of Food Stamp and TANF Policies on Food Stamp Receipt," *Social Service Review*, 82 (2008), 291-334.

Rifkin, Ryan and Aldebaro Klautau, "In Defense of One-Vs-All Classification," *Journal of Machine Learning Research*, 5 (2004), 101-141.

Rossin-Slater, Maya, "WIC in Your Neighborhood: New Evidence on the Impacts of Geographic Access to Clinics," *Journal of Public Economics*, 102 (2013), 51-69.

Schanzenbach, Diane, "Experimental Estimates of the Barriers to Food Stamp Enrollment," Institute for Research on Poverty, University of Wisconsin-Madison, Discussion Paper 1367-09, 2009.

Song Yunjie, Jonathan Skinner, Julie Bynum, Jason Sutherland, John E. Wennberg, and Elliott S. Fisher, "Regional Variations in Diagnostic Practices," *New England Journal of Medicine*, 363 (2010), 45-53.

U.S. Congressional Budget Office, "Growth in Means-Tested Programs and Tax Credits for Low-Income Households," 2013.

U.S. Department of Agriculture: Food and Nutrition Service, "Supplemental Nutrition Assistance Program (SNAP): Eligibility," 2015. http://www.fns.usda.gov/snap/eligibility (accessed 2.1.15).

U.S. Department of Health and Human Services, "Fiscal year 2011 TANF Financial Data," 2012.

U.S. Department of Agriculture, "The Food Assistance Landscape: FY 2015 Annual Report," 2016.

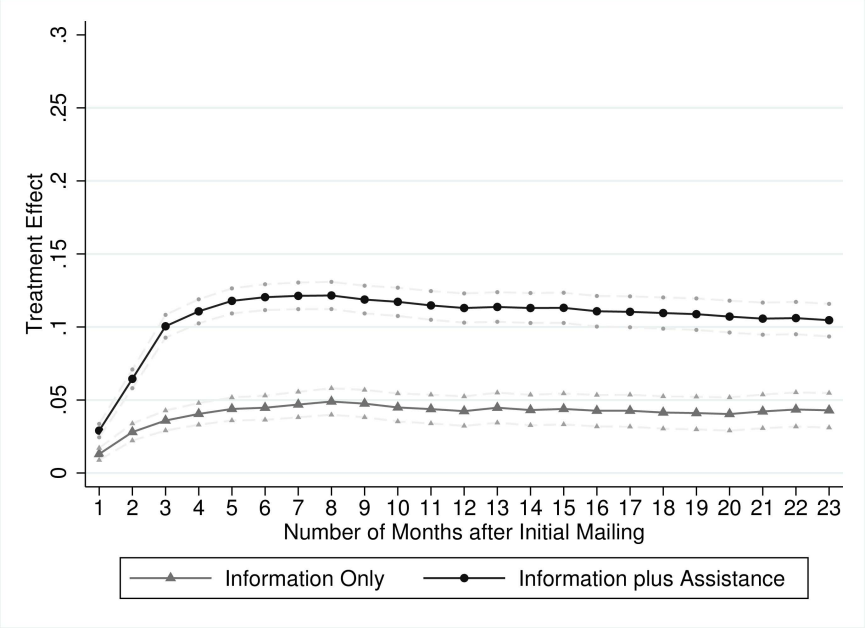
U.S. Department of Health and Human Services, "FY 2015 Federal TANF & State MOE financial data," 2016.

U.S. Internal Revenue Service, "EITC Calendar Year Report," 2016.

U.S. Social Security Administration, "Social Security Income Program 2016 Technical Materi-

als," February 2016.

FIGURE I
TIME PATTERN OF ENROLLMENT RESPONSES



Notes: 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 I
DESCRIPTION OF STUDY POPULATION

	(1)	(2)	(3)	(4)	(5)
	Original Outreach List	After Exclusions			
		List, After Exclusions	Receving SNAP	Not Receiving SNAP	Study Population
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 D for more details.
[a]Omitted category is other or missing race.
[b]Total spending is truncated at twice 99.5th percentile of study population, which is 371,620 (99.5th percentile in study population is 185,810). Amounts greater than the threshold are set to missing.

TABLE II

BEHAVIORAL RESPONSES TO "INFORMATION ONLY" AND "INFORMATION PLUS ASSISTANCE"

	(1) Control	(2) Information Only	(3) Information Plus Assistance	(4) P Value of Difference (Column 2 vs 3)
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 III

BEHAVIORAL RESPONSES TO "INFORMATION ONLY" INTERVENTION WITH AND WITHOUT REMINDERS

	(1) Control	(2) Information Only Standard	(3) Information Only No-Postcard	(4) P Value of Difference (Column 2 vs 3)
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 II column (3) for the pooled Information Only analysis). Column 3 shows the results of the Information Only intervention without the reminder postcard; the outreach materials are otherwise identical to those in 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 IV
ENROLLEE MONTHLY BENEFITS AND PREDICTED BENEFITS

	(1) Control	(2) Information Only	(3) Information Plus Assistance	(4) P Value of Difference (Column 2 vs 3)
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 V
Demographic and Health Characteristics: Applicants and Enrollees

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	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
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 VI

DEMOGRAPHIC AND HEALTH CHARACTERISTICS: CALLERS AND NON-CALLERS

	(1) Callers	(2) Non-callers	(3) P Value of Difference
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 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 II. 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.

Online Appendix to

"Take-up and Targeting: Experimental Evidence from SNAP"

Amy Finkelstein and Matthew J. Notowidigdo

April 2019

A: Conceptual Framework - Details and Extensions

This section provides detailed proofs of the main propositions and additional extended results summarized in the main text.

A.1 More detail on Conceptual Framework from Section II

This subsection goes through the model setup from the main text in more detail, and includes proofs of the Propositions in the main text.

The model features two types of individuals $j \in \{L, H\}$. Each type has an unobserved wage of θ_j, with $\theta_H > \theta_L$. This is the key source of heterogeneity in the model; it can be interpreted as heterogeneity in ability or labor market productivity, and it creates a potential motive for redistribution. We assume throughout that there is a unit mass of each type of individual.

Individuals choose hours of work h_j (which produces labor income $\theta_j h_j$) and whether or not to apply to the supplemental income program, which provides benefits B if income is below an earnings cutoff we denote by r^*. We allow each type to misperceive the benefit amount by ϵ_j, so that the *perceived* benefit from applying is $(1 + \epsilon_j)B$. We refer to the special case of no misperceptions – i.e., $\epsilon_j = 0$ for $j \in \{L, H\}$ – as the "neoclassical" benchmark case. With $\epsilon_j < 0$, misperceptions reduce the perceived benefit from applying.

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 $y_j \equiv \theta_j h_j - \tau(\theta_j h_j)$.

Individuals share a common utility function: $u(x_j) - v(h_j)$ if they don't apply and $u(x_j) - v(h_j) - (\bar{\Lambda}\kappa_j + c)$ if they apply. Individuals get utilty from consumption (x), 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 κ_j is how

the utility cost varies with $\bar{\Lambda}$ for individuals of type j. This formulation nests ordeals that impose a greater utility cost on H types ($\kappa_H > \kappa_L$), or on L types ($\kappa_L > \kappa_H$). The former case 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 (see, e.g., Nichols and Zeckhauser 1982). The latter case includes the possibility that L types having greater difficulty filling out forms (see, e.g., Bertrand et al. 2004).

Individual choices and private welfare

Individuals make application and labor supply choices to maximize private utility, given their (possibly incorrect) perceptions. We denote the optimal hours choice for type j individuals who apply as h_j^A and the optimal hours choice for individuals who do not apply as $h_j^{\neg A}$. For low-ability individuals, we assume that either hours choice would leave them with labor earnings below the income eligibility threshold r^* needed to qualify for the supplemental income program, with optimal solutions given by the following maximization problems:

$$\begin{aligned} h_L^A &= \arg\max_{h_L} u(h_L\theta_L - \tau(h_L\theta_L) + (1+\epsilon_L)B) - v(h_L) - (\bar{\Lambda}\kappa_L + c) \\ h_L^{\neg A} &= \arg\max_{h_L} u(h_L\theta_L - \tau(h_L\theta_L)) - v(h_L) \end{aligned} \tag{7}$$

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. Their hours choice when not applying is given by the same maximization problem as above; i.e,

$$h_H^{\neg A} = \arg\max_{h_H} u(h_H\theta_H - \tau(h_H\theta_H)) - v(h_H) \tag{8}$$

Given these optimal hours choices, expected utility from applying (A) is:

$$u(h_j^A\theta_j - \tau(h_j^A\theta_j) + (1+\epsilon_j)B) - v(h_j^A) - (\bar{\Lambda}\kappa_j + c)$$

and the utility of not applying ($\neg A$) is given by:

$$u(h_j^{\neg A}\theta_j - \tau(h_j^{\neg A}\theta_j)) - v(h_j^{\neg A})$$

Individuals of type j apply if the expected utility of applying is greater than the utility of not applying. We define c_j^* to be the threshold level of c such that a type j individual is indifferent between applying and not applying. For $c < c_j^*$, the individual chooses to apply. This threshold is defined as follows:

$$\begin{aligned} c_j^* &= u(h_j^A\theta_j - \tau(h_j^A\theta_j) + (1+\epsilon_j)B) - v(h_j^A) - \bar{\Lambda}\kappa_j \\ &\quad -u(h_j^{\neg A}\theta_j - \tau(h_j^{\neg A}\theta_j)) + v(h_j^{\neg A}) \end{aligned} \tag{9}$$

Note that an intervention that reduces transaction costs of applying (i.e., decreases $\bar{\Lambda}$) will increase c_j^* more for types with higher utility costs of applying (i.e., larger κ_j). This will lead to greater increases in applications if $f_j(c)$ is the same across types.

We use this application decision rule in equation (9) and integrate across the distribution of private costs to get the total private welfare of type j individuals:

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

Note that ϵ_j affects the individual application decision but not realized utility, since the ϵ_j term only changes the perceived benefits, not the actual benefits.

Social welfare

Because of the fiscal externality on the government budget, privately optimal application decisions may not be socially optimal. We consider a redistributive social welfare function, which is natural given the redistributive purpose of the transfer program. Specifically, we consider a utilitarian social welfare function, although we could easily accommodate alternative individualistic social welfare functions at the cost of some additional notation.

The social welfare function W is therefore the sum of total private welfare of both types of individuals, minus public expenditures on benefits, plus net government revenue:

$$\begin{aligned} W \quad = \quad & \underbrace{V_L + V_H}_{\text{Private Welfare}} \\ & - \underbrace{[B(A_L + A_H)]}_{\text{Mechanical Program Costs}} \\ & + \underbrace{[A_L\tau(h_L^A\theta_L) + (1 - A_L)\tau(h_L^{\neg A}\theta_L) + A_H\tau(h_H^A\theta_H) + (1 - A_H)\tau(h_H^{\neg A}\theta_H)]}_{\text{Fiscal Externality from Program}} \end{aligned} \tag{10}$$

where A_j is the expected number of applications from type j individuals; this is equal to $A_j = F_j(c_j^*)$ from the take-up decision in equation (9). Note that in the main text we denoted net government revenue from applicants (i.e., tax revenue $\tau(h_j^A\theta_j)$) by G_j^A and equivalently for non applicants. Note also that rather than add net government revenue to the social welfare function, we could instead "close" the government budget by having net government revenue and program expenditures "paid for" out of individual consumption. Our approach assumes that the costs of the government budget are born by someone with the average marginal utility of consumption in society; implicitly, our W

expression in equation (10) is thus a "money metric" social welfare expression, normalized by the average marginal utility of consumption in the population

The social planner chooses the income tax system 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 θ_j and endogenous hours choices, the government is not able to achieve the first best desired amount of redistribution (equal consumption across types); redistribution to low ability types is limited by the binding incentive compatibility constraint that high ability types not want to "mimic" the hours choice of low ability 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 may affect information about eligibility and the private cost of application.

Welfare Effects of Interventions

Proposition 1. *Let $\mu_j \equiv u(h_j^A\theta_j - \tau(h_j^A\theta_j) + B) - u(h_j^A\theta_j - \tau(h_j^A\theta_j) + (1+\epsilon_j)B)$, which equals the difference between the actual and perceived utility when applying. The effect of the Information Only treatment on welfare is given by the following expression:*

$$\begin{aligned}\frac{dW}{dT}^{Info\,Only} = & \underbrace{\mu_L\frac{dA_L}{dT} + \mu_H\frac{dA_H}{dT}}_{\textit{Change in Private Welfare}} \\ & - \underbrace{\left[B(\frac{dA_L}{dT} + \frac{dA_H}{dT})\right]}_{\textit{Change in Public Expenditure on Benefits}} \\ & + \underbrace{\left[(\tau(\theta_L h_L^A) - \tau(\theta_L h_L^{\neg A}))\frac{dA_L}{dT} + (\tau(\theta_H h_H^A) - \tau(\theta_H h_H^{\neg A}))\frac{dA_H}{dT}\right]}_{\textit{Change in Net Government Revenue}}\end{aligned}$$

and the effect of the Information Plus Assistance treatment on welfare is given by the following expression:

$$
\begin{aligned}
\frac{dW}{dT}^{Info+Assistance} &= \underbrace{\mu_l \frac{dA_l}{dT} + \mu_h \frac{dA_h}{dT} + \kappa_L A_L + \kappa_H A_H}_{\text{Change in Private Welfare}} \\
&- \underbrace{\left[B(\frac{dA_L}{dT} + \frac{dA_H}{dT})\right]}_{\text{Change in Program Costs}} \\
&+ \underbrace{\left[(\tau(\theta_L h_L^A) - \tau(\theta_L h_L^{\neg A}))\frac{dA_L}{dT} + (\tau(\theta_H h_H^A) - \tau(\theta_H h_H^{\neg A}))\frac{dA_H}{dT}\right]}_{\text{Change in Net Government Revenue}}
\end{aligned}
$$

Proof: Welfare is given by

$$
\begin{aligned}
W =& V_L + V_H \\
& - [B(A_L + A_H)] \\
& + \left[A_L \tau(h_L^A \theta_L) + (1 - A_L)\tau(h_L^{\neg A}\theta_L) + A_H \tau(h_H^A \theta_H) + (1 - A_H)\tau(h_H^{\neg A}\theta_H)\right] \\
=& u(h_L^{\neg A}\theta_L - \tau(h_L^{\neg A}\theta_L)) - v(h_L^{\neg A}) \\
& + \int_0^{c_L^*} \left[(u(h_L^A \theta_L - \tau(h_L^A \theta_L) + B) - v(h_L^A) - (\bar{\Lambda}\kappa_L + c)) - (u(h_L^{\neg A}\theta_L - \tau(h_L^{\neg A}\theta_L)) - v(h_L^{\neg A}))\right] dF_L(c) \\
& + u(h_H^{\neg A}\theta_H - \tau(h_H^{\neg A}\theta_H)) - v(h_H^{\neg A}) \\
& + \int_0^{c_H^*} \left[(u(h_H^A \theta_H - \tau(h_H^A \theta_H) + B) - v(h_H^A) - (\bar{\Lambda}\kappa_H + c)) - (u(h_H^{\neg A}\theta_H - \tau(h_H^{\neg A}\theta_H)) - v(h_H^{\neg A}))\right] dF_H(c) \\
& - [B(A_L + A_H)] \\
& + \left[A_L \tau(h_L^A \theta_L) + (1 - A_L)\tau(h_L^{\neg A}\theta_L) + A_H \tau(h_H^A \theta_H) + (1 - A_H)\tau(h_H^{\neg A}\theta_H)\right],
\end{aligned}
$$

where $c_j^* = u(h_j^A \theta_j - \tau(h_j^A \theta_j) + (1 + \epsilon_j)B) - v(h_j^A) - \bar{\Lambda}\kappa_j - (u(h_j^{\neg A}\theta_j - \tau(h_j^{\neg A}\theta_j)) - v(h_j^{\neg A}))$.

Information Only ($dT = d\epsilon$): Taking the derivative with respect to to ϵ yields

$$\begin{aligned}\frac{dW}{d\epsilon} =& \frac{d}{d\epsilon}\int_0^{c_L^*}\Big[u(h_L^A\theta_L - \tau(h_L^A\theta_L) + B) - v(h_L^A) - (\bar{\Lambda}\kappa_L + c) - u(h_L^{\neg A}\theta_L - \tau(h_L^{\neg A}\theta_L)) + v(h_L^{\neg A})\Big] f_L(c)dc \\ &+ \frac{d}{d\epsilon}\int_0^{c_H^*}\Big[u(h_H^A\theta_H - \tau(h_H^A\theta_H) + B) - v(h_H^A) - (\bar{\Lambda}\kappa_H + c) - u(h_H^{\neg A}\theta_H - \tau(h_H^{\neg A}\theta_H)) + v(h_H^{\neg A})\Big] f_H(c)dc \\ &- \Big[B(\frac{dA_L}{d\epsilon} + \frac{dA_H}{d\epsilon})\Big] \\ &+ \Big[(\tau(h_L^A\theta_L) - \tau(h_L^{\neg A}\theta_L))\frac{dA_L}{d\epsilon} + (\tau(h_H^A\theta_H) - \tau(h_H^{\neg A}\theta_H))\frac{dA_H}{d\epsilon}\Big]\end{aligned}$$

Applying Leibniz's Rule, we get

$$\begin{aligned}&\frac{d}{d\epsilon}\int_0^{c_L^*}\Big[(u(h_L^A\theta_L - \tau(h_L^A\theta_L) + B) - v(h_L^A) - (\bar{\Lambda}\kappa_L + c)) - u(h_L^{\neg A}\theta_L - \tau(h_L^{\neg A}\theta_L)) + v(h_L^{\neg A})\Big] f_L(c)dc \\ =& \Big[(u(h_L^A\theta_L - \tau(h_L^A\theta_L) + B) - v(h_L^A) - (\bar{\Lambda}\kappa_L + c_L^*)) - (u(h_L^{\neg A}\theta_L - \tau(h_L^{\neg A}\theta_L)) - v(h_L^{\neg A}))\Big] f_L(c_L^*)\frac{dc_L^*}{d\epsilon} \\ =&(u(h_L^A\theta_L - \tau(h_L^A\theta_L) + B) - u(h_L^A\theta_L - \tau(h_L^A\theta_L) + (1+\epsilon_L)B))f_L(c_L^*)\frac{dc_L^*}{d\epsilon}\end{aligned}$$

Similarly,

$$\begin{aligned}&\frac{d}{d\epsilon}\int_0^{c_H^*}\Big[(u(h_H^A\theta_H - \tau(h_H^A\theta_H) + B) - v(h_H^A) - (\bar{\Lambda}\kappa_H + c)) - u(h_H^{\neg A}\theta_H - \tau(h_H^{\neg A}\theta_H)) + v(h_H^{\neg A})\Big] f_H(c)dc \\ =&(u(h_H^A\theta_H - \tau(h_H^A\theta_H) + B) - u(h_H^A\theta_H - \tau(h_H^A\theta_H) + (1+\epsilon_H)B))f_H(c_H^*)\frac{dc_H^*}{d\epsilon}\end{aligned}$$

Since the number of applicants is given by $A_H = F_H(c_H^*)$ and $A_L = F_L(c_L^*)$,

$$\frac{dA_H}{d\epsilon} = f_H(c_H^*)\frac{dc_H^*}{d\epsilon}$$

and

$$\frac{dA_L}{d\epsilon} = f_L(c_L^*)\frac{dc_L^*}{d\epsilon}$$

Therefore, we can re-write

$$\begin{aligned}&\frac{d}{d\epsilon}\int_0^{c_L^*}\Big[(u(h_L^A\theta_L - \tau(h_L^A\theta_L) + B) - v(h_L^A) - (\bar{\Lambda}\kappa_L + c)) - (u(h_L^{\neg A}\theta_L - \tau(h_L^{\neg A}\theta_L)) - v(h_L^{\neg A}))\Big] f_L(c)dc \\ =&(u(h_L^A\theta_L - \tau(h_L^A\theta_L) + B) - u(h_L^A\theta_L - \tau(h_L^A\theta_L) + (1+\epsilon_L)B))\frac{dA_L}{d\epsilon}\end{aligned}$$

and

$$\frac{d}{d\epsilon}\int_0^{c_H^*}\Big[(u(h_H^A\theta_H-\tau(h_H^A\theta_H)+B)-v(h_H^A)-(\bar{\Lambda}\kappa_H+c))-(u(h_H^{\neg A}\theta_H-\tau(h_H^{\neg A}\theta_H))-v(h_H^{\neg A})))\Big]f_H(c)dc$$

$$=(u(h_H^A\theta_H-\tau(h_H^A\theta_H)+B)-u(h_H^A\theta_H-\tau(h_H^A\theta_H)+(1+\epsilon_H)B))\frac{dA_H}{d\epsilon}$$

Putting all this together (and noting that $dT=d\epsilon$), we have

$$\begin{aligned}\frac{dW}{dT}=&\mu_L\frac{dA_L}{dT}+\mu_H\frac{dA_H}{dT}\\&-\Big[B(\frac{dA_L}{dT}+\frac{dA_H}{dT})\Big]\\&+\Big[(\tau(h_L^A\theta_L)-\tau(h_L^{\neg A}\theta_L))\frac{dA_L}{dT}+(\tau(h_H^A\theta_H)-\tau(h_H^{\neg A}\theta_H))\frac{dA_H}{dT}\Big].\end{aligned}$$

This completes the first part of the proposition.

Assistance Only ($dT=-d\bar{\Lambda}$): Recall that the total private cost of applying for type j individual is $\bar{\Lambda}\kappa_j+c$. Thus, a change $-d\bar{\Lambda}$ is a downward shift in every applicant's total private cost of applying. As in previous derivation, we can differentiate social welfare, W, with respect to $\bar{\Lambda}$:

$$\begin{aligned}\frac{dW}{d\bar{\Lambda}}=&\frac{d}{d\bar{\Lambda}}\int_0^{c_L^*}\Big[u(h_L^A\theta_L-\tau(h_L^A\theta_L)+B)-v(h_L^A)-(\bar{\Lambda}\kappa_L+c)-u(h_L^{\neg A}\theta_L-\tau(h_L^{\neg A}\theta_L))+v(h_L^{\neg A})\Big]f_L(c)dc\\&+\frac{d}{d\bar{\Lambda}}\int_0^{c_H^*}\Big[u(h_H^A\theta_H-\tau(h_H^A\theta_H)+B)-v(h_H^A)-(\bar{\Lambda}\kappa_H+c)-u(h_H^{\neg A}\theta_H-\tau(h_H^{\neg A}\theta_H))+v(h_H^{\neg A})\Big]f_H(c)dc\\&-\Big[B(\frac{dA_L}{d\bar{\Lambda}}+\frac{dA_H}{d\bar{\Lambda}})\Big]\\&+\Big[(\tau(h_L^A\theta_L)-\tau(h_L^{\neg A}\theta_L))\frac{dA_L}{d\bar{\Lambda}}+(\tau(h_H^A\theta_H)-\tau(h_H^{\neg A}\theta_H))\frac{dA_H}{d\bar{\Lambda}}\Big]\end{aligned}$$

Applying Leibniz's Rule, we get

$$\frac{d}{d\bar{\Lambda}}\int_0^{c_L^*}\left[(u(h_L^A\theta_L-\tau(h_L^A\theta_L)+B)-v(h_L^A)-(\bar{\Lambda}\kappa_L+c))-u(h_L^{\neg A}\theta_L-\tau(h_L^{\neg A}\theta_L))+v(h_L^{\neg A})\right]f_L(c)dc$$

$$=\left[(u(h_L^A\theta_L-\tau(h_L^A\theta_L)+B)-v(h_L^A)-(\bar{\Lambda}\kappa_L+c_L^*))-(u(h_L^{\neg A}\theta_L-\tau(h_L^{\neg A}\theta_L))-v(h_L^{\neg A}))\right]f_L(c_L^*)\frac{dc_L^*}{d\bar{\Lambda}}$$

$$+\int_0^{c_L^*}[-(\kappa_L)]\,f_L(c)dc$$

$$=(u(h_L^A\theta_L-\tau(h_L^A\theta_L)+B)-u(h_L^A\theta_L-\tau(h_L^A\theta_L)+(1+\epsilon_L)B))f_L(c_L^*)\frac{dc_L^*}{d\epsilon}-\kappa_L A_L$$

Similarly,

$$\frac{d}{d\bar{\Lambda}}\int_0^{c_H^*}\left[(u(h_H^A\theta_H-\tau(h_H^A\theta_H)+B)-v(h_H^A)-(\bar{\Lambda}\kappa_H+c))-u(h_H^{\neg A}\theta_H-\tau(h_H^{\neg A}\theta_H))+v(h_H^{\neg A})\right]f_H(c)dc$$

$$=(u(h_H^A\theta_H-\tau(h_H^A\theta_H)+B)-u(h_H^A\theta_H-\tau(h_H^A\theta_H)+(1+\epsilon_H)B))f_H(c_H^*)\frac{dc_H^*}{d\bar{\Lambda}}-\kappa_H A_H$$

Note that the two above expressions are similar to the Information Only change in private welfare for each type with additional term $(\kappa_j A_j)$ that reflects the change in welfare for infra-marginal applicants.Noting that $\frac{dA_j}{d\bar{\Lambda}}=f_j(c_j^*)\frac{dc_j^*}{d\bar{\Lambda}}$, and putting this together implies

$$\begin{aligned}\frac{dW}{dT}&=-\frac{dW}{d\bar{\Lambda}}\\&=\kappa_L F_L(c_L^*)+\mu_L f_L(c_L^*)+(\tau(h_L^A\theta_L)-\tau(h_L^{\neg A}\theta_L)-B)f_L(c_L^*)\\&+\kappa_H F_H(c_H^*)+\mu_H f_H(c_H^*)+(\tau(h_H^A\theta_H)-\tau(h_H^{\neg A}\theta_H)-B)f_H(c_H^*)\\&=\mu_L\frac{dA_L}{dT}+\kappa_L A_L+\mu_H\frac{dA_H}{dT}+\kappa_H A_H-B(\frac{dA_L}{dT}+\frac{dA_H}{dT})\\&+(\tau(h_L^A\theta_L)-\tau(h_L^{\neg A}\theta_L))\frac{dA_L}{dT}+(\tau(h_H^A\theta_H)-\tau(h_H^{\neg A}\theta_H))\frac{dA_H}{dT}.\end{aligned}$$

Information + Assistance ($dT=d\epsilon,-d\bar{\Lambda}$):

Combining information and assistance yields

$$\begin{aligned}\frac{dW}{dT}&=\mu_L\frac{dA_L}{dT}+A_L+\mu_H\frac{dA_H}{dT}+A_H-B(\frac{dA_L}{dT}+\frac{dA_H}{dT})\\&+(\tau(h_L^A\theta_L)-\tau(h_L^{\neg A}\theta_L))\frac{dA_L}{dT}+(\tau(h_H^A\theta_H)-\tau(h_H^{\neg A}\theta_H))\frac{dA_H}{dT}\end{aligned}$$

where $\frac{dA_j}{dT}$ is the change in the number of applications from both de and $-dc$. This is the second part of the proposition, and completes the proof.

Relationship Between Targeting Impacts and Changes in Welfare

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:*

$$\frac{\partial}{\partial(de/dT)}\left(\frac{dW}{dT}\right)\Bigg|_{\frac{dA}{dT}} = \left[(\mu_L - \mu_H) + (\tau(\theta_L h_L^A) - \tau(\theta_L h_L^{\neg A})) - (\tau(\theta_H h_H^A) - \tau(\theta_H h_H^{\neg A}))\right](E_L + E_H).$$

Proof: Note that $\mu_j \equiv u(h_j^A\theta_j - \tau(h_j^A\theta_j) + B) - u(h_j^A\theta_j - \tau(h_j^A\theta_j) + (1+\epsilon_j)B)$, as defined above. We assume applications are all accepted, so $E_j = A_j$. We extend model to allow for uncertainty in application process and prove analogous propositions below (see A.3).

Since $e = \frac{E_L}{E_H+E_L} = \frac{E_L}{A_L+A_H} = \frac{E_L}{A}$, can solve for de/dT as follows:

$$\begin{aligned}\frac{de}{dT} &= \frac{d\frac{E_L}{A}}{dT} \\ &= \frac{1}{A}\cdot\frac{dE_L}{dT} - \frac{dA}{dT}\cdot\frac{E_L}{A^2}\end{aligned}$$

From Proposition 1, we know that the change in welfare from Information Only is the following:

$$\begin{aligned}\frac{dW}{dT} &= \mu_L\frac{dA_L}{dT} + \mu_L\frac{dA_L}{dT} - \left[B(\frac{dA_L}{dT} + \frac{dA_H}{dT})\right] \\ &+ \left[(\tau(\theta_L h_L^A) - \tau(\theta_L h_L^{\neg A}))\frac{dA_L}{dT} + (\tau(\theta_H h_H^A) - \tau(\theta_H h_H^{\neg A}))\frac{dA_H}{dT}\right] \\ &= \left[\mu_L - B + \tau(\theta_L h_L^A) - \tau(\theta_L h_L^{\neg A})\right](\frac{dA_L}{dT} + \frac{dA_H}{dT}) \\ &+ \left[(\mu_H - B + \tau(\theta_H h_H^A) - \tau(\theta_H h_H^{\neg A})) - (\mu_L - B + \tau(\theta_L h_L^A) - \tau(\theta_L h_L^{\neg A}))\right]\frac{dA_H}{dT} \\ &= \left[\mu_L - B + \tau(\theta_L h_L^A) - \tau(\theta_L h_L^{\neg A})\right]\frac{dA}{dT} \\ &+ \left[(\mu_H - B + \tau(\theta_H h_H^A) - \tau(\theta_H h_H^{\neg A})) - (\mu_L - B + \tau(\theta_L h_L^A) - \tau(\theta_L h_L^{\neg A}))\right]\frac{dA_H}{dT}.\end{aligned}$$

Now, since dA/dT is held constant, and $\frac{dh_L^A}{dT}$ only depends on the private utility function, we just need to determine how the right-hand term varies with de/dT. We begin with the following derivative given model assumptions :

$$-\frac{dA_H}{dT} = -\frac{dE_H}{dT} = \frac{dE_L}{dT} - \frac{dA}{dT}$$

Thus, we can solve for $-\frac{dA_H}{dT}$ in terms of de/dT:

$$\begin{aligned}
-\frac{dA_H}{dT} + \frac{dA}{dT} &= A \cdot \frac{de}{dT} + \frac{dA}{dT} \cdot \frac{E_L}{A} \\
&= A \cdot \frac{de}{dT} + e\frac{dA}{dT} \\
-\frac{dA_H}{dT} &= A \cdot \frac{de}{dT} + (e-1)\frac{dA}{dT}
\end{aligned}$$

which can then be substituted back into the dW/dT expression above. Then, taking the partial derivative with respect to de/dT gives the expression in Proposition 2:

$$\begin{aligned}
&\frac{\partial}{\partial(de/dT)} \left(\frac{dW}{dT}\right)\Big|_{\frac{dA}{dT}} \\
=&\frac{\partial}{\partial(de/dT)} \left[(\mu_L - \mu_H) + (\tau(\theta_L h_L^A) - \tau(\theta_L h_L^{\neg A})) - (\tau(\theta_H h_H^A) - \tau(\theta_H h_H^{\neg A})\right] (A \cdot \frac{de}{dT} + (e-1)\frac{dA}{dT}) \\
=&\left[(\mu_L - \mu_H) + (\tau(\theta_L h_L^A) - \tau(\theta_L h_L^{\neg A})) - (\tau(\theta_H h_H^A) - \tau(\theta_H h_H^{\neg A})\right] \frac{\partial}{\partial(de/dT)}(A \cdot \frac{de}{dT} + (e-1)\frac{dA}{dT}) \\
=&\left[(\mu_L - \mu_H) + (\tau(\theta_L h_L^A) - \tau(\theta_L h_L^{\neg A})) - (\tau(\theta_H h_H^A) - \tau(\theta_H h_H^{\neg A})\right] \cdot A \\
=&\left[(\mu_L - \mu_H) + (\tau(\theta_L h_L^A) - \tau(\theta_L h_L^{\neg A})) - (\tau(\theta_H h_H^A) - \tau(\theta_H h_H^{\neg A})\right] (E_L + E_H)
\end{aligned}$$

A.1.1 Welfare effects from infra-marginal applicants

As described in the main text, Proposition 1 abstracts from income effects on infra-marginal applicants. These income effects can affect social welfare through an additional fiscal externality and – if there are pre-existing misperceptions – then there will also be effects of the interventions on the private welfare of infra-marginal applicants. Intuitively, for individuals already applying, changes in their beliefs can cause them to change their labor supply, and this can affect both private welfare and also generate an additional fiscal externality from the labor supply response. Proposition 1 abstracts from these income effects on infra-marginal applicants. We ignore these additional terms in the main text because they scale with the magnitude of the change in beliefs from intervention, the magnitude of the misperceptions, and the magnitude of the income effect in labor supply. Unless these terms are large, this term is likely to be small.

However, for completeness we go through these additional terms in this sub-section, focusing on the Information Only intervention.

Information Only ($dT = d\epsilon$): In our baseline setting, the private welfare of type j applicants is given by

$$V_{j,\,apply} = \int_0^{c_j^*} \Big[u(h_j^A\theta_j - \tau(h_j^A\theta_j) + B) - v(h_j^A) - (\bar{\Lambda}\kappa_j + c)\Big]\, dF_j(c)$$

Since the welfare gain for infra-marginal applicants is through the adjustment of their labor supply, type H infra-marginal applicants will not experience this gain since their labor supply is fixed

by $h_H^A = \frac{r*}{\theta_H}$. For type L infra-marginal applicants, however, the change in private welfare of infra-marginal applicants can be derived as follows:

$$\begin{aligned}
\frac{dV_{L,\,apply}}{d\epsilon} &= \int_0^{c_L^*} \frac{d}{d\epsilon}\left[u(h_L^A\theta_L - \tau(h_L^A\theta_L) + B) - v(h_L^A) - (\bar{\Lambda}\kappa_L + c)\right] dF_L(c) \\
&= \frac{d}{d\epsilon}\left[u(h_L^A\theta_L - \tau(h_L^A\theta_L) + B) - v(h_L^A)\right] \int_0^{c_L^*} f_L(c)dc \\
&= \frac{d}{d\epsilon}\left[u(h_L^A\theta_L - \tau(h_L^A\theta_L) + B) - v(h_L^A)\right] A_L
\end{aligned}$$

From line 2 to line 3, the marginal effect is taken out of the integral, because it does not depend on cost as discussed above. Notice that the first-order condition applicants use to make choices remains the same, so $d\epsilon$ does not have first-order effect on $u(h_L^A\theta_L - \tau(h_L^A\theta_L) + (1+\epsilon_L)B) - v(h_L^A) - (\bar{\Lambda}\kappa_L + c)$. But this is the *perceived* utility of infra-marginal applicants, not the actual utility, and we can take a first-order approximation of the difference between this level of utility and the (actual) utility level in the private welfare expression. The change of this difference is approximately equal to the change of the private welfare.

$$\begin{aligned}
\triangle u^A &= u(h_L^A\theta_L - \tau(h_L^A\theta_L) + B) - v(h_L^A) - (\bar{\Lambda}\kappa_L + c) - \left[u(h_L^A\theta_L - \tau(h_L^A\theta_L) + (1+\epsilon_L)B) - v(h_L^A) - (\bar{\Lambda}\kappa_L + c)\right] \\
&\approx -u'(h_L^A\theta_L - \tau(h_L^A\theta_L) + B)\epsilon_L B
\end{aligned}$$

This expression shows that when there is no misperception ($\epsilon_j = 0$), this term is zero, and there is no first-order effect on private welfare of infra-marginal applicants. If individuals under-estimate benefits from applying, however, then individuals gain from Information Only treatment, and this benefit scales with $d\epsilon$, ϵ, and the marginal utility of consumption.

The second term coming from infra-marginal applicants is the additional fiscal externality. This is a straightforward term to characterize and it does not depend on whether or not there are pre-existing misperceptions. Any change in labor earnings from infra-marginal applicants' changing labor supply from Information Only intervention will (in turn) affect government revenue. If the change in hours for infra-marginal type L applicants from Information Only treatment is $\frac{dh_L^A}{dT}$, then the additional fiscal externality is given by $\tau'(\theta_L h_L^A)\theta_L A_L \frac{dh_L^A}{dT}$. This term will be small if either the change in beliefs is small, or the income effect in labor supply is small.

In the remainder of the Appendix we will exclude this additional fiscal externality term as well as the change in private welfare for infra-marginal applicants from all extensions of the baseline model. These terms will be similar in the model extensions to the terms in this sub-section.

A.2 Extensions

This section goes through several extensions of the main model presented in the main text. These extensions cover non-marginal changes in beliefs and alternative ways of modeling "mistakes" –

specifically, misperceptions of the private costs of applying and modeling inattention instead of misperceptions in the benefits from applying. We also refer readers to the NBER Working Paper version of this paper (#24652, www.nber.org/papers/w24652) for a number of additional model extensions, including allowing for heterogeneity in beliefs and modelling the information interventions as reducing the variance in beliefs.

In each of these extensions, we focus on deriving results that are analogous to Proposition 1 on the effect of the interventions on social welfare, and we focus on the Information Only treatment, since the derivations for Information Plus Assistance are often very similar and follow many of the same steps as in the proof to Proposition 1 above.

A.2.1 Non-Marginal Changes in Beliefs (Away From Envelope Theorem)

Our main analysis focuses on the marginal welfare gain (or loss) from "small" interventions, allowing us to make heavy use of the envelope theorem. We explore here an extension where we consider non-marginal changes, similar in spirit to Kleven (2018) who extends sufficient statistics analysis to discrete policy changes. We describe how this addition to the "standard" setting (no mistakes and the only fiscal externality occurs via labor supply) can also undo the "standard" relationship between changes in targeting and changes in welfare.

With non-marginal changes, the envelope theorem will no longer apply, and so changes in private welfare can be meaningful, even in the neoclassical benchmark case with accurate beliefs. As a result, we will still have the same lack of a general relationship between targeting properties of the intervention and changes in welfare. This is illustrated in the following result, which focuses on the case of accurate beliefs for simplicity:

Proposition 3. *Let c_j' be the threshold application cost after the non-marginal change. Define $h_L^{\triangle\epsilon} = \arg\max_{h_L} u(h_L\theta_L - \tau(h_L\theta_L) + (1+\epsilon_L+\triangle\epsilon)B) - v(h_L) - (\bar{\Lambda}\kappa_L + c))$, and define $c_L' = u(h_L^{\triangle\epsilon}\theta_L - \tau(h_L^{\triangle\epsilon}\theta_L) + (1+\triangle\epsilon)B) - v(h_L^{\triangle\epsilon}) - \bar{\Lambda}\kappa_L - u(h_L^{\neg A}\theta_L - \tau(h_L^{\neg A}\theta_L)) + v(h_L^{\neg A})$ and $c_H' = u(h_H^A\theta_H - \tau(h_H^A\theta_H) + (1+\triangle\epsilon)B) - v(h_H^A) - \bar{\Lambda}\kappa_H - u(h_H^{\neg A}\theta_H - \tau(h_H^{\neg A}\theta_H)) + v(h_H^{\neg A})$.*

If $\epsilon_L = \epsilon_H = 0$, then the effect of a non-marginal Information Only treatment ($\Delta T = \Delta\epsilon$) on welfare is given by:

$$\begin{aligned}
\Delta W = &\int_{c_L^*}^{c_L'} \left[(u(h_L^{\triangle\epsilon}\theta_L - \tau(h_L^{\triangle\epsilon}\theta_L) + B) - v(h_L^{\triangle\epsilon}) - (\bar{\Lambda}\kappa_L + c)) - u(h_L^{\neg A}\theta_L - \tau(h_L^{\neg A}\theta_L)) + v(h_L^{\neg A})\right] f_L(c)dc \\
&+ \int_{c_H^*}^{c_H'} \left[(u(h_H^A\theta_H - \tau(h_H^A\theta_H) + B) - v(h_H^A) - (\bar{\Lambda}\kappa_H + c)) - u(h_H^{\neg A}\theta_H - \tau(h_H^{\neg A}\theta_H)) + v(h_H^{\neg A})\right] f_H(c)dc \\
&- \left[(B - \tau(h_L^{\triangle\epsilon}\theta_L) + \tau(h_L^{\neg A}\theta_L))\Delta A_L + (B - \tau(h_H^A\theta_H) + \tau(h_H^{\neg A}\theta_H))\Delta A_H\right]
\end{aligned}$$

In the expression above, $c_j^* = u(h_j^A\theta_j - \tau(h_j^A\theta_j) + B) - v(h_j^A) - \bar{\Lambda}\kappa_j - u(h_j^{\neg A}\theta_j - \tau(h_j^{\neg A}\theta_j)) + v(h_j^{\neg A})$ is the threshold cost prior to the non-marginal change in beliefs. Type H applicants do not adjust their labor supply, which is given by $h_H^A = \frac{r^*}{\theta_H}$; thus the expression above already substitutes h_H^A for $h_H^{\triangle\epsilon}$.

This expression shows that the change in social welfare can be decomposed into three parts. The first line is the change of private welfare from new type L applicants; the second line is the change in private welfare from new type H applicants. The third line represents the change of program cost and government expenditure brought by new applicants of each type.

To see why these terms are not obviously signed in the case of an intervention that increases targeting, note that in the non-marginal case the increase in private welfare from a given change in applicants depends on the shape of the type-specific cost distribution $f_j(c)$. If most of the individuals induced to apply were close to indifferent before the non-marginal change in costs, then a non-marginal change in costs can have a non-marginal change in private welfare; however, if most of the individuals induced to apply were close to indifferent to applying *after* the non-marginal change, then the non-marginal change in costs will have a negligible effect on their utility, since they are close to indifferent to applying after the intervention. Thus, the cost distribution functions – much as the misperception terms did away from the neoclassic benchmark – provide another factor that potentially breaks the relationship between improvements in targeting and changes in social welfare.

Proof:

Information Only ($\Delta T = \Delta\epsilon$): Let c_j' be the threshold in cost after the non-marginal change. Let $h_L^{\triangle\epsilon} = \arg\max_{h_L} u(h_L\theta_L - \tau(h_L\theta_L) + (1+\triangle\epsilon)B) - v(h_L) - (\bar{\Lambda}\kappa_L + c))$, and define $c_L' = u(h_L^{\triangle\epsilon}\theta_L - \tau(h_L^{\triangle\epsilon}\theta_L) + (1+\triangle\epsilon)B) - v(h_L^{\triangle\epsilon}) - \bar{\Lambda}\kappa_L - u(h_L^{\neg A}\theta_L - \tau(h_L^{\neg A}\theta_L)) + v(h_L^{\neg A})$ and $c_H' = u(h_H^A\theta_H - \tau(h_H^A\theta_H) + (1+\triangle\epsilon)B) - v(h_H^A) - \bar{\Lambda}\kappa_H - u(h_H^{\neg A}\theta_H - \tau(h_H^{\neg A}\theta_H)) + v(h_H^{\neg A})$.

If $\epsilon_L = \epsilon_H = 0$, then the effect of the non-marginal Information Only treatment ($\Delta T = \Delta\epsilon$) can be derived as follows:

$$\Delta W = \int_{c_L^*}^{c_L'} \left[(u(h_L^{\triangle\epsilon}\theta_L - \tau(h_L^{\triangle\epsilon}\theta_L) + B) - v(h_L^{\triangle\epsilon}) - (\bar{\Lambda}\kappa_L + c)) - u(h_L^{\neg A}\theta_L - \tau(h_L^{\neg A}\theta_L)) + v(h_L^{\neg A})\right] f_L(c)dc$$
$$+ \int_{c_H^*}^{c_H'} \left[(u(h_H^A\theta_H - \tau(h_H^A\theta_H) + B) - v(h_H^A) - (\bar{\Lambda}\kappa_H + c)) - u(h_H^{\neg A}\theta_H - \tau(h_H^{\neg A}\theta_H)) + v(h_H^{\neg A})\right] f_H(c)dc$$
$$- \left[\int_{c_L^*}^{c_L'} (B - \tau(h_L^{\triangle\epsilon}\theta_L) + \tau(h_L^{\neg A}\theta_L)) f_L(c)dc + \int_{c_H^*}^{c_H'} (B - \tau(h_H^{\triangle\epsilon}\theta_H) + \tau(h_H^{\neg A}\theta_H)) f_H(c)dc\right]$$

$$
\begin{aligned}
\Delta W = & \int_{c_L^*}^{c_L'} \Big[(u(h_L^{\triangle\epsilon}\theta_L - \tau(h_L^{\triangle\epsilon}\theta_L) + B) - v(h_L^{\triangle\epsilon}) - (\bar{\Lambda}\kappa_L + c)) - u(h_L^{\neg A}\theta_L - \tau(h_L^{\neg A}\theta_L)) + v(h_L^{\neg A}) \Big] f_L(c)dc \\
& + \int_{c_H^*}^{c_H'} \Big[(u(h_H^{A}\theta_H - \tau(h_H^{A}\theta_H) + B) - v(h_H^{A}) - (\bar{\Lambda}\kappa_H + c)) - u(h_H^{\neg A}\theta_H - \tau(h_H^{\neg A}\theta_H)) + v(h_H^{\neg A}) \Big] f_H(c)dc \\
& - \left[(B - \tau(h_L^{\triangle\epsilon}\theta_L) + \tau(h_L^{\neg A}\theta_L)) \int_{c_L^*}^{c_L'} f_L(c)dc + (B - \tau(h_H^{A}\theta_H) + \tau(h_H^{\neg A}\theta_H)) \int_{c_H^*}^{c_H'} f_H(c)dc \right] \\
= & \int_{c_L^*}^{c_L'} \Big[(u(h_L^{\triangle\epsilon}\theta_L - \tau(h_L^{\triangle\epsilon}\theta_L) + B) - v(h_L^{\triangle\epsilon}) - (\bar{\Lambda}\kappa_L + c)) - u(h_L^{\neg A}\theta_L - \tau(h_L^{\neg A}\theta_L)) + v(h_L^{\neg A}) \Big] f_L(c)dc \\
& + \int_{c_H^*}^{c_H'} \Big[(u(h_H^{A}\theta_H - \tau(h_H^{A}\theta_H) + B) - v(h_H^{A}) - (\bar{\Lambda}\kappa_H + c)) - u(h_H^{\neg A}\theta_H - \tau(h_H^{\neg A}\theta_H)) + v(h_H^{\neg A}) \Big] f_H(c)dc \\
& - \Big[(B - \tau(h_L^{\triangle\epsilon}\theta_L) + \tau(h_L^{\neg A}\theta_L))\Delta A_L + (B - \tau(h_H^{A}\theta_H) + \tau(h_H^{\neg A}\theta_H))\Delta A_H \Big]
\end{aligned}
$$

A.2.2 Alternative Models of "Mistakes": Misperceptions in Costs of Applying and Inattention

Misperceptions in Private Utility Cost of Applying (Instead of Misperceptions in Benefits from Applying) Next, instead of misperceiving the benefits from applying by misperceiving B, now assume that individuals misperceive private utility cost of applying. Specifically, each type may also misperceive true cost of applying by δ_j, which raises the perceived cost of applying for type j.

So it is still the case that individual of type j applies if perceived expected utility of applying is greater than (certain) utility of not applying. For individual with private cost c_i this can be defined as follows (assuming there is no misperception of benefits from applying; i.e., $\epsilon_j = 0$)

$$
\begin{aligned}
& u(h_j^{A}\theta_j - \tau(h_j^{A}\theta_j) + (1+\epsilon_j)B) - v(h_j^{A}) - (\bar{\Lambda}\kappa_j + c_j + \delta_j) > u(h_j^{\neg A}\theta_j - \tau(h_j^{\neg A}\theta_j)) + v(h_j^{\neg A}) \\
\Rightarrow\ & c_j + \delta_j < u(h_j^{A}\theta_j - \tau(h_j^{A}\theta_j) + (1+\epsilon_j)B) - v(h_j^{A}) - \bar{\Lambda}\kappa_j - u(h_j^{\neg A}\theta_j - \tau(h_j^{\neg A}\theta_j)) + v(h_j^{\neg A}) \\
\Rightarrow\ & c_j < u(h_j^{A}\theta_j - \tau(h_j^{A}\theta_j) + B) - v(h_j^{A}) - \bar{\Lambda}\kappa_j - u(h_j^{\neg A}\theta_j - \tau(h_j^{\neg A}\theta_j)) + v(h_j^{\neg A}) - \delta_j = c_j^* - \delta_j
\end{aligned}
$$

As a result, the share of individuals of type H applying is $F_H(c_H^* - \delta_H)$. We can also define the (private + public) welfare coming from type H individuals.

$$
\begin{aligned}
W_H =& u(h_H^{\neg A}\theta_H - \tau(h_H^{\neg A}\theta_H)) - v(h_H^{\neg A}) \\
&+ \int_0^{c_H^*-\delta_H} \Big[(u(h_H^A\theta_H - \tau(h_H^A\theta_H) + B) - v(h_H^A) - (\bar{\Lambda}\kappa_H + c)) - (u(h_H^{\neg A}\theta_H - \tau(h_H^{\neg A}\theta_H)) - v(h_H^{\neg A}))\Big]\, dF_H(c) \\
&- BF_H(c_H^* - \delta_H) \\
&+ \Big[F_H(c_H^* - \delta_H)\tau(h_H^A\theta_H) + (1 - F_H(c_H^* - \delta_H))\tau(h_H^{\neg A}\theta_H)\Big].
\end{aligned}
$$

Note in above expression that the δ affects application decision but not realized utility (it's perceived cost, not an actual cost). All of these results are identical for the type L individuals, simply replace the H subscripts with L subscripts.

Proposition 4. *The effect of the Information Only treatment on welfare is given by the following expression:*

$$
\frac{dW}{dT} = \delta_L \frac{dA_L}{dT} + \delta_H \frac{dA_H}{dT} - B\left(\frac{dA_L}{dT} + \frac{dA_H}{dT}\right) + \Big[\tau(h_L^A\theta_L) - \tau(h_L^{\neg A}\theta_L)\Big]\frac{dA_L}{dT} + \Big[\tau(h_H^A\theta_L) - \tau(h_H^{\neg A}\theta_H)\Big]\frac{dA_H}{dT}
$$

Proof: Welfare is given by:

$$
\begin{aligned}
W =& u(h_H^{\neg A}\theta_H - \tau(h_H^{\neg A}\theta_H)) - v(h_H^{\neg A}) \\
&+ \int_0^{c_H^*-\delta_H} \Big[(u(h_H^A\theta_H - \tau(h_H^A\theta_H) + B) - v(h_H^A) - (\bar{\Lambda}\kappa_H + c)) - (u(h_H^{\neg A}\theta_H - \tau(h_H^{\neg A}\theta_H)) - v(h_H^{\neg A}))\Big]\, dF_H(c) \\
&+ u(h_L^{\neg A}\theta_L - \tau(h_L^{\neg A}\theta_L)) - v(h_L^{\neg A}) \\
&+ \int_0^{c_L^*-\delta_L} \Big[(u(h_L^A\theta_L - \tau(h_L^A\theta_L) + B) - v(h_L^A) - (\bar{\Lambda}\kappa_L + c)) - (u(h_L^{\neg A}\theta_L - \tau(h_L^{\neg A}\theta_L)) - v(h_L^{\neg A}))\Big]\, dF_L(c) \\
&- B\left[F_H(c_H^* - \delta_H) + F_L(c_L^* - \delta_L)\right] \\
&+ \Big[F_H(c_H^* - \delta_H)\tau(h_H^A\theta_H) + (1 - F_H(c_H^* - \delta_H))\tau(h_H^{\neg A}\theta_H)\Big] \\
&+ \Big[F_L(c_L^* - \delta_L)\tau(h_L^A\theta_L) + (1 - F_L(c_L^* - \delta_L))\tau(h_L^{\neg A}\theta_L)\Big].
\end{aligned}
$$

Information Only ($dT = -d\delta$): Taking the derivative with respect to to δ (i.e., both δ_H and δ_L change by $d\delta$) yields the following:

$$
\begin{aligned}
\frac{dW}{d\delta} =& \frac{d}{d\delta} \int_0^{c_H^*-\delta_H} \left[u(h_H^A\theta_H - \tau(h_H^A\theta_H) + B) - v(h_H^A) - (\bar{\Lambda}\kappa_H + c) - u(h_H^{\neg A}\theta_H - \tau(h_H^{\neg A}\theta_H)) + v(h_H^{\neg A})\right] dF_H(c) \\
&+ \frac{d}{d\delta} \int_0^{c_L^*-\delta_L} \left[u(h_L^A\theta_L - \tau(h_L^A\theta_L) + B) - v(h_L^A) - (\bar{\Lambda}\kappa_L + c) - u(h_L^{\neg A}\theta_L - \tau(h_L^{\neg A}\theta_L)) + v(h_L^{\neg A})\right] dF_L(c) \\
&+ B\left[f_H(c_H^* - \delta_H) + f_L(c_L^* - \delta_L)\right] \\
&- \left[f_H(c_H^* - \delta_H)\tau(h_H^A\theta_H) - f_H(c_H^* - \delta_H)\tau(h_H^{\neg A}\theta_H) + f_L(c_L^* - \delta_L)\tau(h_:^A\theta_L) - f_L(c_L^* - \delta_L)\tau(h_L^{\neg A}\theta_L)\right]
\end{aligned}
$$

Applying Leibniz's Rule, we get

$$
\begin{aligned}
&\frac{d}{d\delta} \int_0^{c_H^*-\delta_H} \left[u(h_H^A\theta_H - \tau(h_H^A\theta_H) + B) - v(h_H^A) - (\bar{\Lambda}\kappa_H + c) - u(h_H^{\neg A}\theta_H - \tau(h_H^{\neg A}\theta_H)) + v(h_H^{\neg A})\right] dF_H(c) \\
&= -\delta f_H(c_H^* - \delta_H)
\end{aligned}
$$

Since the number of applicants is given by $A_L = F_L(c_L^* - \delta)L)$ and $A_H = F_H(c_H^* - \delta_H)$,

$$\frac{dA_L}{d\delta} = -f_L(c_L^* - \delta_L)$$

and

$$\frac{dA_H}{d\delta} = -f_H(c_H^* - \delta_H).$$

Therefore, we can re-write

$$
\begin{aligned}
&B\left[f_H(c_H^* - \delta_H) + f_L(c_L^* - \delta_L)\right] \\
&- \left[f_H(c_H^* - \delta_H)\tau(h_H^A\theta_H) - f_H(c_H^* - \delta_H)\tau(h_H^{\neg A}\theta_H) + f_L(c_L^* - \delta_L)\tau(h_L^A\theta_L) - f_L(c_L^* - \delta_L)\tau(h_L^{\neg A}\theta_L)\right] \\
=& - B(\frac{dA_L}{d\delta} + \frac{dA_H}{d\delta}) + \left[\frac{dA_L}{d\delta}\tau(h_L^A\theta_L) - \frac{dA_L}{d\delta}\tau(h_L^{\neg A}\theta_L) + \frac{dA_H}{d\delta}\tau(h_H^A\theta_H) - \frac{dA_H}{d\delta}\tau(h_H^{\neg A}\theta_L)\right]
\end{aligned}
$$

Putting all this together (since $dT = -d\delta$), we have

$$\frac{dW}{dT} = \delta_L\frac{dA_L}{dT} + \delta_H\frac{dA_H}{dT} - B(\frac{dA_L}{dT} + \frac{dA_H}{dT}) + \left[\tau(h_L^A\theta_L) - \tau(h_L^{\neg A}\theta_L)\right]\frac{dA_L}{dT} + \left[\tau(h_H^A\theta_L) - \tau(h_H^{\neg A}\theta_H)\right]\frac{dA_H}{dT}$$

Alternative Non-Neoclassical Model: Inattention Now suppose that agents have correct beliefs (so we have $\epsilon_L = \epsilon_H = 0$), but a fraction $(1-\alpha)$ of agents are inattentive where $\alpha \in [0,1]$. This fraction is assumed to be independent of private utility cost of applying. Attentive agents make a choice about whether or not to apply for benefits, but inattentive agents simply do not

apply because they forget, don't read the paperwork, etc. The Information Only treatment then reduces the fraction of inattentive agents (or increases the fraction of attentive agents). In other words, $dT = d\alpha$.

Let $U_j(\cdot)$ be individual utility regardless of whether the individual chooses to apply. As before,

$$\begin{aligned} W = &\int U_L(\cdot)dF_L(c) + \int U_H(\cdot)dF_H(c) \\ &- [B(A_L + A_H)] \\ &+ \Big[A_L\tau(h_L^A\theta_L) + (1-A_L)\tau(h_L^{\neg A}\theta_L) + A_H\tau(h_H^A\theta_H) + (1-A_H)\tau(h_H^{\neg A}\theta_H)\Big], \end{aligned}$$

Attentive agents apply if

$$\begin{aligned} &u(h_j^A\theta_j - \tau(h_j^A\theta_j) + (1+\epsilon_j)B) - v(h_j^A) - (\bar{\Lambda}\kappa_j + c_j) > u((1-\tau)h_j^{\neg A}\theta_j) + v(h_j^{\neg A}) \\ \Rightarrow\ &c_j < u(h_j^A\theta_j - \tau(h_j^A\theta_j) + B) - v(h_j^A) - \bar{\Lambda}\kappa_j - u((1-\tau)h_j^{\neg A}\theta_j) + v(h_j^{\neg A}) \equiv c_j^* \end{aligned}$$

By contrast, inattentive agents don't apply, regardless of their value of c_j. Let $\nu_j = \int_0^{c_j^*} \Big[c_j^* - c\Big]\, dF_j(c)$, which is the applicants' aggregate amount of the difference between threshold in application cost and individual application cost. In the case where there is no misperception of the probability of application being accepted, this is the aggregate amount of utility gain by turning to an applicant of those whose optimal choice it is to apply. Therefore, we can rewrite (focusing on type H):

$$\begin{aligned} \int U_H(\cdot)dF_H(c) =& \alpha\left[\int_A U_H(\cdot|A)f_H(c)dc + \int_{\neg A} U_H(\cdot|\neg A)f_H(c)dc\right] + (1-\alpha)\left[\int U_H(\cdot|\neg A)f_H(c)dc\right] + \\ =& \alpha\left[u(h_H^{\neg A}\theta_H - \tau(h_H^{\neg A}\theta_H)) - v(h_H^{\neg A})\right] \\ &+ \alpha\left[\int_0^{c_H^*}\left[(u(h_H^A\theta_H - \tau(h_H^A\theta_H) + B) - v(h_H^A) - (\bar{\Lambda}\kappa_H + c)) - (u(h_H^{\neg A}\theta_H - \tau(h_H^{\neg A}\theta_H)) - v(h_H^{\neg A}))\right] dF_H(c)\right] \\ &+ (1-\alpha)\left[u(h_H^{\neg A}\theta_H - \tau(h_H^{\neg A}\theta_H)) - v(h_H^{\neg A})\right] \\ =& u(h_H^{\neg A}\theta_H - \tau(h_H^{\neg A}\theta_H)) - v(h_H^{\neg A}) \\ &+ \alpha\int_0^{c_H^*}\left[(u(h_H^A\theta_H - \tau(h_H^A\theta_H) + B) - v(h_H^A) - (\bar{\Lambda}\kappa_H + c)) - (u(h_H^{\neg A}\theta_H - \tau(h_H^{\neg A}\theta_H)) - v(h_H^{\neg A}))\right] dF_H(c) \\ =& u(h_H^{\neg A}\theta_H - \tau(h_H^{\neg A}\theta_H)) - v(h_H^{\neg A}) + \alpha\int_0^{c_H^*}[c_H^* - c]\, dF_H(c) \\ =& u(h_H^{\neg A}\theta_H - \tau(h_H^{\neg A}\theta_H)) - v(h_H^{\neg A}) + \alpha\nu_H, \end{aligned}$$

and similarly,

$$\int U_L(\cdot)dF_L(c) = u(h_L^{\neg A}\theta_L - \tau(h_L^{\neg A}\theta_L)) - v(h_L^{\neg A}) + \alpha\nu_L$$

Applicants are given by

$$A_L = \alpha F_L(c_L^*)$$

and

$$A_H = \alpha F_H(c_H^*)$$

Therefore, welfare is

$$\begin{aligned}
W &= u(h_L^{\neg A}\theta_L - \tau(h_L^{\neg A}\theta_L)) - v(h_L^{\neg A}) + \alpha\nu_H \\
&\quad + u(h_H^{\neg A}\theta_H - \tau(h_H^{\neg A}\theta_H)) - v(h_H^{\neg A}) + \alpha\nu_L \\
&\quad - [\alpha B(F_L(c_L^*) + F_H(c_H^*))] \\
&\quad + \Big[\alpha F_L(c_L^*)\tau(h_L^A\theta_L) + (1 - \alpha F_L(c_L^*))\tau(h_L^{\neg A}\theta_L) + \alpha F_H(c_H^*)\tau(h_H^A\theta_H) + (1 - \alpha F_H(c_H^*))\tau(h_H^{\neg A}\theta_H)\Big]
\end{aligned}$$

Just to compare, in the fully rational benchmark case, welfare is given by

$$\begin{aligned}
W &= u(h_L^{\neg A}\theta_L - \tau(h_L^{\neg A}\theta_L)) - v(h_L^{\neg A}) + \nu_L \\
&\quad + u(h_H^{\neg A}\theta_H - \tau(h_H^{\neg A}\theta_H)) - v(h_H^{\neg A}) + \nu_H \\
&\quad - [B(F_L(c_L^*) + F_H(c_H^*))] \\
&\quad + \Big[F_L(c_L^*)\tau(h_L^A\theta_L) + (1 - F_L(c_L^*))\tau(h_L^{\neg A}\theta_L) + F_H(c_H^*)\tau(h_H^A\theta_H) + (1 - F_H(c_H^*))\tau(h_H^{\neg A}\theta_H)\Big]
\end{aligned}$$

We consider the effect of an Information Only intervention that increases attention:

Proposition 5. *If $\epsilon_L = \epsilon_H = 0$, and there is at least some inattention, then the effect of Information Only treatment ($dT = d\alpha$) on welfare is given by:*

$$\begin{aligned}
\frac{dW}{dT} &= \nu_H + \nu_L \\
&\quad - B(F_L(c_L^*) + F_H(c_H^*)) \\
&\quad + F_L(c_L^*)\Big[\tau(h_L^A\theta_L) - \tau(h_L^{\neg A}\theta_L) - g_L\Big] + F_H(c_H^*)\Big[\tau(h_H^A\theta_H) - \tau(h_H^{\neg A}\theta_H)\Big]
\end{aligned}$$

Proof: Simply differentiating W with respect to α yields

$$\begin{aligned}
\frac{dW}{dT} &= \nu_H + \nu_L \\
&\quad - B(F_L(c_L^*) + F_H(c_H^*)) \\
&\quad + F_L(c_L^*)\Big[\tau(h_L^A\theta_L) - \tau(h_L^{\neg A}\theta_L)\Big] + F_H(c_H^*)\Big[\tau(h_H^A\theta_H) - \tau(h_H^{\neg A}\theta_H)\Big]
\end{aligned}$$

Note that this result makes clear that there are effects on private welfare from a treatment that reduces inattention, but the targeting properties of this intervention will depend on relative magnitude of ν_H and ν_L, which depend on the conditional distribution of costs of applying (conditional on cost being between 0 and the threshold cost) as well as the κ_H and κ_L.

A.3 Welfare effects of Intervention in the Empirical Setting

As described in the main text, we use the conceptual framework developed in Section II to assess the normative implications of the empirical findings from our RCT. We tailor the framework in two specific, but minor, ways in order to apply it to our empirical setting. First, to facilitate our calibration of the model based on our empirical evidence, we allow for an exogenous probability the application is accepted, given by π_j. Second, we allow for two different benefit levels: individuals may receive either $\overline{B}$ or B_{min}, with $\overline{B} > B_{min}$. These are benefits individuals receive conditional on application being accepted.

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. This does not constrain the fiscal externalities from the intervention since, as discussed, in Section II, the framework and propositions developed apply generally to any fiscal externality. So, we will still use the G_j^A and $G_j^{\neg A}$ to denote fiscal externalities of type j individuals, as in the main text, but in our empirical setting we view these fiscal externalities as coming from application processing costs rather than endogenous earnings.

Without endogenous earnings, $h_j^A = h_j^{\neg A}$ for both types of individuals, and so we will just use $h_j\theta_j$ to denote labor earnings for type j individuals. Importantly, since earnings do not adjust, the level of benefits that individuals receive is fully 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 can be based on the level of benefits received: $e = E_L/(E_L + E_H)$, where E_L is enrollees who receive minimum level of benefits and E_H is enrollees who receive higher benefits. We thus interpret benefit level as a direct proxy for type in our setting. Given this proxy, our empirical results therefore indicate that both interventions decrease targeting ($de/dT < 0$).

Lastly, since expected benefits are now a function of both probability application is accepted and the benefits (conditional on application acceptance), the misperceptions can either take the form of misperceptions in acceptance rate or misperceptions in benefits (conditional on application acceptance). In this section we focus on misperceptions in $\overline{B}$ or B_{min},, and we refer readers to the NBER Working Paper version (#24652, www.nber.org/papers/w24652) for analogous results (which hold as first-order approximations) in the case where misperception are based on probability application is accepted (i.e., the π_j). The results are first-order approximations because we need to assume that the welfare effects of receiving benefits B with probability π can be approximated by the welfare effects of receiving benefits $\pi * B$ with certainty.

With these modifications, we re-state Propositions 1 and 2 (as 1a and 2a) as follows:

Proposition 1a. *With benefit level as a direct proxy for type in our setting, now let* $\mu_j \equiv \pi_j u(h_j^A\theta_j - \tau(h_j^A\theta_j) + B_j) - \pi_j u(h_j^A\theta_j - \tau(h_j^A\theta_j) + (1+\epsilon_j)B_j)$. *The effect of the Information Only treatment on welfare is given by the following expression:*

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

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

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

Proof: Welfare is given by

$$
\begin{aligned}
W =& V_L + V_H \\
& - (\pi_L \overline{B} A_L + \pi_H B_{min} A_H) \\
& + G_L^A A_L + G_L^{\neg A}(1 - A_L) + G_H^A A_H + G_H^{\neg A}(1 - A_H) \\
=& u(h_L\theta_L - \tau(h_L\theta_L)) - v(h_L) \\
& + \int_0^{c_L^*} \left[\pi_L u(h_L\theta_L - \tau(h_L\theta_L) + \overline{B}) + (1 - \pi_L) u(h_L\theta_L - \tau(h_L\theta_L)) - v(h_L) - (\bar{\Lambda}\kappa_L + c)\right] dF_L(c) \\
& - \int_0^{c_L^*} \left[u(h_L\theta_L - \tau(h_L\theta_L)) - v(h_L)\right] dF_L(c) \\
& + u(h_H\theta_H - \tau(h_H\theta_H)) - v(h_H) \\
& + \int_0^{c_H^*} \left[\pi_H u(h_H\theta_H - \tau(h_H\theta_H) + B_{min}) + (1 - \pi_H) u(h_H\theta_H - \tau(h_H\theta_H)) - v(h_H) - (\bar{\Lambda}\kappa_H + c)\right] dF_H(c) \\
& - \int_0^{c_H^*} \left[u(h_H\theta_H - \tau(h_H\theta_H)) - v(h_H)\right] dF_H(c) \\
& - \pi_L \overline{B} A_L - \pi_H B_{min} A_H \\
& + G_L^A A_L + G_L^{\neg A}(1 - A_L) + G_H^A A_H + G_H^{\neg A}(1 - A_H),
\end{aligned}
$$

where $c_j^* = \pi_j u(h_j\theta_j - \tau(h_j\theta_j) + (1 + \epsilon_j)B_j) - \bar{\Lambda}\kappa_j - \pi_j u(h_j\theta_j - \tau(h_j\theta_j))$, defining $B_j = B_{min}$ for type L individuals, and $B_j = \overline{B}$ for type Hindividuals. This welfare expression can be simplified to the following:

$$
\begin{aligned}
W =& u(h_L\theta_L - \tau(h_L\theta_L)) - v(h_L) \\
& + \int_0^{c_L^*} \left[\pi_L u(h_L\theta_L - \tau(h_L\theta_L) + \overline{B}) - \pi_L u(h_L\theta_L - \tau(h_L\theta_L)) - (\bar{\Lambda}\kappa_L + c)\right] dF_L(c) \\
& + u(h_H\theta_H - \tau(h_H\theta_H)) - v(h_H) \\
& + \int_0^{c_H^*} \left[\pi_H u(h_H\theta_H - \tau(h_H\theta_H) + B_{min}) - \pi_H u(h_H\theta_H - \tau(h_H\theta_H)) - (\bar{\Lambda}\kappa_H + c)\right] dF_H(c) \\
& - \pi_L \overline{B} A_L - \pi_H B_{min} A_H + G_L^A A_L + G_L^{\neg A}(1 - A_L) + G_H^A A_H + G_H^{\neg A}(1 - A_H)
\end{aligned}
$$

Applying Leibniz's Rule, we get:

$$\frac{d}{d\epsilon}\int_0^{c_L^*}\Big[\pi_L u(h_L\theta_L-\tau(h_L\theta_L)+\overline{B})-\pi_L u(h_L\theta_L-\tau(h_L\theta_L))-(\bar{\Lambda}\kappa_L+c)\Big]\,f_L(c)dc$$
$$=\Big[\pi_L u(h_L\theta_L-\tau(h_L\theta_L)+\overline{B})-\pi_L u(h_L\theta_L-\tau(h_L\theta_L))-(\bar{\Lambda}\kappa_L+c_L^*)\Big]\,f_L(c_L^*)\frac{dc_L^*}{d\epsilon}$$
$$=(\pi_L u(h_L\theta_L-\tau(h_L\theta_L)+\overline{B})-\pi_L u(h_L\theta_L-\tau(h_L\theta_L)+(1+\epsilon_L)\overline{B}))f_L(c_L^*)\frac{dc_L^*}{d\epsilon}$$

Since the number of applicants is given by $A_j = F_j(c_j^*)$,

$$\frac{dA_j}{d\epsilon} = f_j(c_j^*)\frac{dc_j^*}{d\epsilon}.$$

Putting all this together (and using $dT = d\epsilon$), we have

$$\begin{aligned}\frac{dW}{d\epsilon} =&\mu_L\frac{dA_L}{d\epsilon}+\mu_H\frac{dA_H}{d\epsilon}\\ &-\left[(\pi_H B_{min})\frac{dA_H}{dT}+(\pi_L\overline{B})\frac{dA_L}{dT}\right]\\ &+[G_L^A-G_L^{\neg A}]\frac{dA_L}{dT}+[G_H^A-G_H^{\neg A}]\frac{dA_H}{dT}\end{aligned}$$

This completes the first part of the proposition.

Assistance Only ($dT = -dc$): This proof combines the arguments above and the steps in the proof of Proposition 1.

Relationship Between Targeting Impacts and Changes in Welfare

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:*

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

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

Application acceptance rates are exogenous and given by π_j, so $E_j = \pi_j A_j$. Since $e = \frac{E_L}{E_H+E_L} =$

$\frac{\pi_L A_L}{\pi_L A_L + \pi_H A_H}$

$$
\begin{aligned}
\frac{de}{dT} &= \frac{d}{dT}\frac{\pi_L A_L}{\pi_L A_L + \pi_H A_H} \\
&= \frac{\pi_L \frac{dA_L}{dT}(\pi_L A_L + \pi_H A_H) - (\pi_L \frac{dA_L}{dT} + \pi_H \frac{dA_H}{dT})\pi_L A_L}{(\pi_L A_L + \pi_H A_H)^2} \\
&= \frac{1}{E_L + E_H}(\pi_L \frac{dA_L}{dT} - \frac{(\pi_L \frac{dA_L}{dT} + \pi_H \frac{dA_H}{dT})E_L}{E_L + E_H}) \\
&= \frac{1}{E_L + E_H}(\pi_L \frac{dA_L}{dT} - \frac{(\pi_L \frac{dA}{dT} + (\pi_L - \pi_H)\frac{dA_L}{dT})E_L}{E_L + E_H}) \\
&= \frac{1}{E_L + E_H}(\frac{\pi_L E_L + \pi_H E_H}{E_L + E_H} \cdot \frac{dA_L}{dT} - \frac{\pi_L E_L}{E_L + E_H} \cdot \frac{dA}{dT})
\end{aligned}
$$

From Proposition 1, we know that change in welfare from Information Only is the following

$$
\begin{aligned}
\frac{dW}{dT} &= \mu_L \frac{dA_L}{dT} + \mu_H \frac{dA_H}{dT} - (\pi_H B_{min})\frac{dA_H}{dT} - (\pi_L \overline{B})\frac{dA_L}{dT} + [G_L^A - G_L^{\neg A}]\frac{dA_L}{dT} + [G_H^A - G_H^{\neg A}]\frac{dA_H}{dT} \\
&= \left[\mu_L - \pi_L \overline{B} + (G_L^A - G_L^{\neg A})\right](\frac{dA_L}{dT} + \frac{dA_H}{dT}) \\
&\quad + \left[(\mu_H - \pi_H B_{min} + (G_H^A - G_H^{\neg A})) - (\mu_L - \pi_L \overline{B} + (G_L^A - G_L^{\neg A}))\right]\frac{dA_H}{dT} \\
&= \left[\mu_L - \pi_L \overline{B} + (G_L^A - G_L^{\neg A})\right]\frac{dA}{dT} + \left[(\mu_H - \pi_H B_{min} + (G_H^A - G_H^{\neg A})) - (\mu_L - \pi_L \overline{B} + (G_L^A - G_L^{\neg A}))\right]\frac{dA_H}{dT}.
\end{aligned}
$$

Now, since dA/dT is held constant in the Proposition, we just need to derive how the second term varies with de/dT. Thus, we can solve for $-\frac{dA_H}{dT}$ in terms of de/dT:

$$
\begin{aligned}
-\frac{dA_H}{dT} + \frac{dA}{dT} &= ((E_L + E_H)\frac{de}{dT} + \frac{\pi_L E_L}{E_L + E_H} \cdot \frac{dA}{dT})\frac{E_L + E_H}{\pi_L E_L + \pi_H E_H} \\
&= \frac{(E_L + E_H)^2}{\pi_L E_L + \pi_H E_H} \cdot \frac{de}{dT} + \frac{\pi_L E_L}{\pi_L E_L + \pi_H E_H} \cdot \frac{dA}{dT} \\
-\frac{dA_H}{dT} &= \frac{(E_L + E_H)^2}{\pi_L E_L + \pi_H E_H} \cdot \frac{de}{dT} + (\frac{\pi_L E_L}{\pi_L E_L + \pi_H E_H} - 1)\frac{dA}{dT}
\end{aligned}
$$

which can then be substituted back into the dW/dT expression above. Then, taking the partial derivative with respect to de/dT gives the expression in Proposition 2a:

$$
\begin{aligned}
\frac{\partial}{\partial(de/dT)}\left(\frac{dW}{dT}\right)\Bigg|_{\frac{dA}{dT}} &= \frac{\partial}{\partial(de/dT)}\left[(\mu_L-\mu_H)-(\pi_L\overline{B}-\pi_H B_{min})+(G_L^A-G_L^{\neg A})-(G_H^A-G_H^{\neg A})\right] \\
&\quad\times\left(\frac{(E_L+E_H)^2}{\pi_L E_L+\pi_H E_H}\cdot\frac{de}{dT}+\left(\frac{\pi_L E_L}{\pi_L E_L+\pi_H E_H}-1\right)\cdot\frac{dA}{dT}\right) \\
&= \left[(\mu_L-\mu_H)-(\pi_L\overline{B}-\pi_H B_{min})+(G_L^A-G_L^{\neg A})-(G_H^A-G_H^{\neg A})\right] \\
&\quad\times\frac{\partial}{\partial(de/dT)}\left(\frac{(E_L+E_H)^2}{\pi_L E_L+\pi_H E_H}\cdot\frac{de}{dT}+\left(\frac{\pi_L E_L}{\pi_L E_L+\pi_H E_H}-1\right)\cdot\frac{dA}{dT}\right) \\
&= \left[(\mu_L-\mu_H)-(\pi_L\overline{B}-\pi_H B_{min})+(G_L^A-G_L^{\neg A})-(G_H^A-G_H^{\neg A})\right]\frac{(E_L+E_H)^2}{\pi_L E_L+\pi_H E_H}
\end{aligned}
$$

A.3.1 Derivation of MVPF formula from marginal welfare gain expression

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.

We begin with the dW/dT expression for the Information Only intervention in Proposition 1a and impose our baseline assumption $G_L^A = G_H^A \equiv -g$, and $G_L^{\neg A} = G_H^{\neg A} = 0$ (which we will relax later). We use a first-order Taylor approximation around actual utility (which we also used to calibrate ϵ) to approximate μ_H as $\xi_H \pi_H \epsilon_H B_{min}$, and μ_L as $\xi_L \pi_L \epsilon_L \overline{B}$. The first-order Taylor approximation is a useful simplification to allow us to implement this formula empirically, but we assess the sensitivity to this assumption in the next sub-section.This transforms $\frac{dW}{dT}^{Information\ Only}$ in Proposition 1a into:

$$
\begin{aligned}
\frac{dW}{dT}^{Information\ Only} &= \underbrace{\xi_L \pi_L \epsilon_L \overline{B}\frac{dA_L}{dT}+\xi_H \pi_H \epsilon_H B_{min}\frac{dA_H}{dT}}_{\textit{Change in Private Welfare}} \\
&\quad - \underbrace{\left[(\pi_L\overline{B}+g)\frac{dA_L}{dT}+(\pi_H B_{min}+g)\frac{dA_H}{dT})\right]}_{\textit{Change in Government Revenue and Public Expenditure on Benefits}}
\end{aligned}
$$

To translate the change in private welfare into a change in dollars of surplus to each type of individual, we divide the change in private welfare for each type by the marginal utility of consumption for each type (ξ_j). We also express the formula as a ratio of change in private welfare to change in costs - rather than the difference between them - so that we can interpret it (in the spirit of Hendren 2016), as the marginal value of public funds (MVPF) of the intervention. This yields:

$$MVPF^{Information\ Only} = \frac{\mu_L \frac{dA_L}{dT} * (1/\xi_L) + \mu_H \frac{dA_H}{dT} * (1/\xi_H)}{(\pi_L \overline{B} + g)\frac{dA_L}{dT} + (\pi_H B_{min} + g)\frac{dA_H}{dT}}$$

Intuitively, this expression 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. Smaller values of MVPF mean that transfering surplus to these two types requires more resources than larger MVFP values.

A.3.2 MVPF for Information Plus Assistance Intervention

In subsection VI.C we describe an MVPF formula for the Information Only intervention. 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 κ_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 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.

A.3.3 Sensitivity of Model Calibrations to Alternative Assumptions

Many of the assumptions we made in the calibration exercises in subsection VI.C 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.

Alternative fiscal externalities 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.[20] 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.

Assumptions about private costs of applying We examined sensitivity to our assumptions about the private costs of applying. 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.[21] 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.[22]

[20] 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.

[21] 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.

[22] 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

Allowing for Risk Aversion In Section VI, the model calibrations back out values for ϵ_H and ϵ_L by calculating values such that individuals (with these biased beliefs) are indifferent to applying, given time cost of applying of \$75, and no non-time costs of applying. By taking a first-order Taylor approximation around the utility of not applying, the difference for type H individuals between the expected utility of applying and not applying is given by $(1+\epsilon_H)*\pi_H*B_{min}*u'(x_H^{\neg A})$. Dividing by $u'(x_H^{\neg A})$ turns this into a money metric that will be equal to \$75 for the marginal applicant. Solving for ϵ_H gives $\epsilon_H = 75/(432) - 1 = -0.833$. For ϵ_L the analogous calculation is $\epsilon_L = 75/4806 - 1 = -0.984$.

To see sensitivity to different assumptions on risk aversion, we assume CRRA utility function ($u(x) = x^{1-\gamma}/(1-\gamma)$ and we assume consumption is equal to income. We also assume that for type H non-applicants $y_H = \$36,000$, and for type L non-applicants $y_L = \$18,000$. These values correspond to approximately 36 months of income for a single person at 100% and 50% of the federal poverty line, and since we assume labor supply is fixed, this value is the same whether the individual applies or not. With these assumptions it is possible to back out ϵ_j for a given value of γ using the following expression: $(1+\epsilon_j)\pi_j u(y_j+B_j)+(1-(1+\epsilon_j)\pi_j)u(y_j)-c_j^* = u(y_j)$, assuming that c_j^* can be approximated as $\$75 * u'(y_j)$.

For a value of $\gamma = 2$ we calculate $\epsilon_H = -0.831$ and for $\gamma = 5$ we calculate $\epsilon_H = -0.826$. For type L individuals, for a value of $\gamma = 2$ we calculate $\epsilon_L = -0.979$ and for $\gamma = 5$ we calculate $\epsilon_L = -0.968$. These calculations show very little sensitivity of the misperception parameters to allowing for risk aversion, which means that we calculate very similar implied change in private welfare for marginal applicants as well as similar MVPFs.

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.

B: Background on SNAP Eligibility and Benefits

While SNAP program rules are mostly determined at the federal level, there is some variation across states. In PA at the time of our intervention (2016), there were three ways an elderly individual can be eligible for SNAP.[23] First, the household would be categorically eligible if all household members received a qualifying benefit - SSI, TANF, General Assistance, State Blind Pensions, or Family Works benefits. Second, the household would be eligible if its gross income were below 200 percent of the Federal Poverty Income Guidelines (FPIG) and has resources below the $3,250 resource limit.[24] Third, the household would be eligible if its gross income were above 200 percent of FPIG but its net income (gross income minus certain exempt income and deductions for certain expenses)[25] were less than 100% FPIG and it had resources below the $3,250 resource limit. Once deemed eligible, an elderly household is certified to receive SNAP benefits for 36 months, although there are exceptions that require earlier re-certification.[26]

If eligible, the benefit amount is set, based on a federally determined formula, as a decreasing function of net income, subject to a minimum and maximum. Benefits are designed so that households spend approximately 30% of their net income (i.e., gross income minus certain deductions and exemptions) on food. Specifically, the maximum benefit is set equal to the cost of food under the USDA's Thrifty Food Plan, which is the minimum amount deemed necessary to buy enough food for a household of a particular size. A family with no income receives the maximum benefit, with benefits taxed away by 30 percent of net income, up to a floor. Thus – subject to a minimum and maximum – monthly benefits are the Thrifty Food Plan Amount (which varies by household size) minus 30 percent of Net Monthly Income. During our study period, the minimum monthly benefit for a categorically eligible household of size 1 or 2 was $16; the minimum monthly benefit was $0 for other enrollees. 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.

The state has 30 calendar days to process an application.[27]

Given the SNAP program rules, both the individual and state's determination of eligibility and benefit amounts require the individual to actively apply with the required information. From the individual's perspective, there is uncertainty about the benefit function, the inputs into it (e.g., various shelter and medical expenses that serve as deductions to income and affect benefits), and

[23] Unless noted otherwise, all information in this section comes from Pennsylvania Department of Human Services (online).

[24] Resources counted toward the limit include bank accounts, cash on hand, cars and motorcycles; many resources are excluded from the resource limit.

[25] Net income is gross income minus a standard deduction and certain exempt income (e.g., TANF benefits, loans, and interest on savings and checking accounts) and minus certain deductions (e.g., for earned income, dependent care, utilities excess medical expenses and excess shelter expenses).

[26] At the time of the intervention, households were required to submit an annual reporting form. Additionally, these households were required to report certain changes, such as when gross monthly income exceeds 130% of FPIG. In June 2016, the state announced a policy change for eldery (age 60 and older) and disabled households, which included a change in the re-certification process, which extended the time period to 36 months for our study sample. https://www.media.pa.gov/Pages/DHS_details.aspx?newsid=209

[27] Households who – by virtue of extreme need – qualify for expedited review must have their application reviewed within 5 calendar days of application.

the potential for mistakes in the process (e.g., not showing up for the interview, not filing the appropriate documentation of expenses, etc.) which cause an otherwise eligible application to be rejected or assigned a lower benefit amount. From the government's perspective, the needed information cannot be passively obtained, even if it had access to data on the individual from tax returns and other public benefit programs. In particular, three specific types of information are not available from other sources. The first is the definition of a household, which is a SNAP-specific definition: people who "live together and customarily prepare food together" (Gray et al., 2016). The household unit is required both to assess eligibility and to determine benefit amounts. Second, the resource limit that is applied to all non-categorically eligible households requires information on resources like bank accounts and second properties that are not readily available in other administrative data. Third, the calculation of net income – which is required in some cases to determine eligibility and in all cases to determine benefits – likewise can be affected by information not otherwise available (like excess out-of-pocket medical expenses and shelter expenses), although of course one could provide less information here and receive commensurately lower benefits. Underlining the difficulty of circumventing the active application process is the experience of the tax preparer Intuit (TurboTax), which in 2015 tried - through a program called Benefits Assist - to submit applications for SNAP on behalf of their low-income clients, using the information that had been provided on their tax returns. States encountered substantially increased administrative burden in response to the noticeable increase in applications, and it appeared that many of these applications were incomplete and could not be approved as filed.[28]

C: Interventions

Description of "assistance" component

An individual who responds to an outreach letter by calling into BDT is connected to a BDT employee –a "Benefits Outreach Specialist" (BOS) - who provides assistance over the phone. BOS's are highly knowledgeable of available benefits. They receive 4 weeks of classroom and experiential learning to become well-versed in the public benefits application process and policies. The up-front training includes coaching and training on phone-based assistance skills so that the caller receives a person-centered and results-driven experience. After this initial training, the BOS continues to receive continuous monitoring and coaching.

The BOS has real-time access to a searchable history of information on the caller from previous interactions with BDT and administrative data sources; in PA these administrative data sources include identified information BDT regularly receives on individuals enrolled in Medicaid, LIHEAP and PACE, and individuals who have exhausted unemployment compensation benefits. BDT has built an internal software platform that stores all this data in a household "portfolio" and allows for the collection of additional self-reported information for each individual linked to the portfolio.

[28] See, e.g., `https://fns-prod.azureedge.net/sites/default/files/snap/State-Guidance-on-Intuit-SNAP-Applications.pdf`; `http://www.macssa.org/memberlogin/15minutes/selfsufficiency_dec15.pdf`; and `https://benkallos.com/press-release/memorandum-automatic-benefits-using-government-data-deliver-better-citizen-services-le`.

The software provides a clickable interface through which BOS can access notes on previous calls, question prompts to determine likely eligibility, an estimated benefits calculator, and a platform for scheduling follow-up actions. BDT customizes question prompts and the benefit calculator to each state's benefit regulations, to ensure that all of the necessary information is collected to estimate eligibility and benefits amounts. This software also allows for direct submission of the application and related verification documents.

Upon being connected to a caller, the BOS asks a series of intake questions designed to collect information relevant for eligibility and benefit screening. Information collected include demographic characteristics (e.g., number of people in the household, current enrollment in other public benefit programs, sex, ethnicity, disability, etc.), legal information (citizenship, marital status, etc.), self-reported monthly income (including pension), other financial resources when necessary (e.g., checking and savings account balances), and expenses by category (rent, utility bills, medical expenses, etc.). Collection of detailed information on expenses may increase the amount of benefits the individual is likely eligible for by increasing their allowable deductions. BDT's custom screening tool allows the BOS to use this self-reported information to inform the caller of whether they are likely eligible for SNAP and their estimated benefit amount.

If the caller decides based on this that they are interested in potentially applying, the BOS then provides information and assistance with the application process. The full set of assistance (which about half of applicants in this intervention arm avail themselves of), includes several stages. BDT completes the application for the caller based on the information received over the telephone and, in that same phone call, informs the applicant of required verification documents. Leveraging state policy options and technology, BDT also minimizes paper verification requirements by proactively informing individuals that they can self-declare shelter expenses (unless questionable) and that DHS can electronically verify Social Security income, identity, residency, and certain medical expenses. BDT then mails an envelope to recipients to collect verification documents, reviews the verification documents it receives, and re-contacts the individual if documentation is inadequate. BDT can then submit the application on behalf of the individual. The individual themselves however must participate in a phone interview with DHS.

BDT may also provide assistance after the application is submitted by reviewing and submitting any follow-up verification documentation requested by DHS, or working with DHS to troubleshoot issues with individual cases. The BDT custom software stores digital records of all received documents in an individual's record, including those submitted to DHS, which allows BDT to keep a detailed history of all application information and to advise applicants on how to advocate for themselves if there are issues with their application. For example, DHS may request a document that has already been provided or that is not necessary. In addition, some applicants miss their interview, or fail to receive an interview call, but still wish to apply. These incidences delay the application process, or even worse, can result in DHS rejecting an application. If contacted by a client about such an issue, BDT advises on how to navigate DHS customer services, and as a last resort, may elevate these issues to their point of contact at DHS to find a solution.

Comparison of standard outreach materials: "Information Plus Assistance" and "Information Only" interventions

The outreach materials in the two main treatment arms were designed to be as similar as possible. The outreach materials in the baseline Information Plus Assistance treatment were the standard materials BDT uses. Appendix Figure A1 shows the letter, envelope and postcard that were sent in this treatment arm. Appendix Figure A2 shows the analogous letter, envelope and postcard. They are designed to be as similar as possible - including the sender (the Secretary of the Pennsylvania Department of Human Services), the layout, and the content. There were, however, some unavoidable differences in the letters which we detail here.

First, the Information Plus Assistance letters reference the PA Benefits Center (the local name of BDT), while the Information Only letter, naturally, does not. Specifically in the former the outreach materials say "We are working closely with the PA Benefits Center to help you get SNAP" and "Please call the PA Benefits Center today" while the information-only outreach materials say ""We want to help you get SNAP" and "Please call the Department of Human Services today." Second, and relatedly, the PA benefits center logo was included - in addition to the PA Department of Human Services logo - on the outreach materials in the Information Plus Assistance interventions, while only the PA Department of Human Services logo was included in the Information Only materials. Third, the hours of operation provided for the call in numbers were slightly different, reflecting the practical reality that BDT hours are 9:00am to 5:00pm while the DHS HELPLINE hours are 8:45am - 4:45pm. Finally, the phone numbers to call naturally differed (although all phone numbers were "1-800" numbers) and the PO box for the return address on the envelope also differed.

Sub-treatments

Appendix Figure A3 shows the study design with all of the treatments and sub-treatments.[29]

One sub-treatment was a "marketing" intervention. 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 an attempt to grab potential applicants' attention and potentially reduce stigma surrounding applying for SNAP. To grab attention, it included a catchy banner that read "Need help buying groceries? Apply today!", bolded text to highlight key information followed by an informative explanation, were printed in color rather than black and white, and included a PA benefits "ACCESS" card image. To try to reduce stigma, it included language such as "Join thousands of older Pennsylvanians already claiming their SNAP benefits" and did not explicitly define SNAP as food stamps. This design was motivated in part by the finding in Schanzenbach (2009) that individuals randomized to outreach materials describing the Food Stamp program in arguably more positive terms (including emphasizing a "Gold State Advantage" card) expressed somewhat higher rates of

[29] Appendix Table A3 showed balance across each the three main groups we study on the study characteristics we examined in Table I; for completeness, Appendix Table A4 shows balance of characteristics across the sub-treatments.

interest in receiving information about food stamp benefits than those whose outreach materials reflected standard USDA materials.

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") to try to make them appear larger by focusing on the maximum benefit amount the individual could receive, rather than the average benefit amount received.

Our main analysis compares across three groups: the (pooled) standard (with postcard) and marketing treatments in the Information Only arm, the (pooled) standard and marketing treatments in the Information Plus Assistance arm, and the control. We down-weight the individuals in the standard treatment in the Information Plus Assistance arm so that the (weighted) share in standard vs. marketing is the same (50 percent) in the Information Plus Assistance and Information Only arms.

D Data

D.1 DHS Data

Data sharing protocols

To construct our study population, DHS supplied BDT with a Medicaid outreach file of approximately 230,000 individuals aged 60 and older who were enrolled in Medicaid as of October 31, 2015. BDT removed the Medicaid recipient ID and created a unique, non-identifying scrambled study ID that uniquely identifies each individual. We received de-identified data files from DHS for all individuals on the initial outreach list (see Table I, column 1). The data consist of: Medicaid enrollment and claims data, SNAP applications and enrollment data, and SNAP benefits data.

BDT provided DHS with the crosswalk between these de-identified study IDs and their unique Medicaid recipient ID. DHS then attached information on SNAP applications, SNAP enrollment, SNAP benefits, and Medicaid enrollment and claims. For the SNAP data, DHS sent the data to BDT who removed all personally-identifying information (i.e., full name, social security number, full address, and Medicaid recipient ID) and transmitted the de-identified data to us via a secure FTP process. For the Medicaid enrollment and claims files, DHS removed the same identifying information and directly transmitted the data to us.

Medicaid outreach list

The Medicaid outreach file we analyze contains the individual's birth year, gender, city, primary language, an indicator of SNAP enrollment, and information on which Medicaid program the individual is enrolled in. All that information was provided by DHS; in addition, BDT supplemented it with a pseudo "household" ID that BDT created to denote people in the outreach file with the same last name and full address.

Medicaid enrollment and claims data

We received Medicaid enrollment and claims data from DHS for everyone on the outreach list. The Medicaid data contains seven files. There is an enrollment file that contains Medicaid enrollment spells from 1981 - 2016; we use the enrollment file to define the start date of the individual's last enrollment spell in Medicaid, and the days enrolled in 2015. We also use the enrollment file to construct a measure of race (since we do not have that in the outreach file).

In addition, there are six claims files that contain claims in 2015 for everyone on the outreach list. The claims include not only payments by Medicaid but also payments by Medicare if the individual is eligible for both. Only insurer payments are included; out-of-pocket spending is not observed but is unlikely to be large in this population.

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.

In the data we received, we can distinguish between managed care and fee-for-service based on claims filed. For our study population (see Table I, column 5), we estimate that about 60 percent of claims and about 80 percent of spending was in fee for service in 2015 .

We use the 2015 claims data to construct healthcare utilization and health measures. The healthcare measures are all measures of annual spending or healthcare use. However about one-quarter of our study population was not enrolled in Medicaid for the entirety of 2015. We therefore annualize the healthcare utilization and healthcare spending measures by multiplying our raw measures by the ratio of 365 to the number of days enrolled in Medicaid in 2015.

Below we describe the construction of specific variables.

Start of Last Medicaid Enrollment: We use the enrollment file to define the start date of the last consecutive enrollment period in Medicaid.

Indicator of Full Year Enrollment in 2015: Following the construction of enrollment spells as above, an individual is indicated as full-year enrollment in 2015 if she has the entirety of 2015 enrolled in Medicaid.

Total Health Care Spending: Total healthcare spending is defined as the sum of inpatient, outpatient, and pharmaceutical spending paid by Medicare or Medicaid. We winsorize spending at twice of the 99.5th percentile of the study population, which is $371,620.

Hospital Days: We measure the number of hospital days based on the total length of inpatient stays in the inpatient file. Stays with a discharge date earlier than the admission date are dropped, and overlapping periods are removed. By construction, the maximum value of this measure is 365.

Emergency Room (ER) Visits: We measure the number of emergency room visits in the outpatient file. ER visits are identified by HCPCS codes 99281-99285, 99291-99292, G0380-G0384, and G0390. We count the total number of ER visits for each individual, allowing a maximum of one ER visit per individual per day.

Doctor Visits: We measure the number of doctor visits as the sum of the number of primary care visits and the number of specialist visits, allowing a maximum of one primary care visit and one specialist visit per individual per day. To identify primary and specialist visits in the outpatient files, we match provider type and provider specialty to taxonomy codes using a crosswalk from DHS[30]. The taxonomy codes are then matched to HCFA specialty codes using a crosswalk from CMS[31]. Finally, HFCA codes are matched to a primary care or specialist classification using a crosswalk from Finkelstein et al. (2016)[32].

Skilled Nursing Facility (SNF) Days: We identify SNF stays in the inpatient files by provider type code 03 and provider specialty codes 030 or 031. Stays with a discharge date earlier than the admission date are dropped, and overlapping periods are removed. The total number of SNF days for each individual is calculated as the sum of length of stays. By construction the maximum value of this measure is 365.

Total Number of Visits and Days: This is the sum of number of hospital days, number of ER visits, number of doctor visits, and number of SNF days.

Weighted Total Number of Visits and Days: This is the (weighted) sum of hospital days, ER visits, doctor visits, and SNF days, where the weights are based on the average cost per encounter in our study population. The average cost per hospital day or SNF day is calculated by dividing total spending over the period of hospital stays or SNF stays experienced in the data by our study population by the total number of hospital days or SNF days experienced by our study population. The average cost per ER visit or doctor visit is calculated by averaging spending in our study population across ER visits or doctor visits. The resulting estimates of average costs are $1,607 for a hospital day, $197 for an ED visit, $147 for a SNF day, and $79 for a doctor visit.

Chronic Conditions: We measure the number of chronic conditions recorded for each individual using the claims files. Each claim has between one and nine ICD-9 or ICD-10 codes for diagnoses. We unify diagnoses codes to ICD-9[33] and identify which ICD-9 codes correspond to chronic conditions following the method developed by Hwang et al. (2001).[34] We measure

[30]See http://www.dhs.pa.gov/cs/groups/webcontent/documents/document/p_002941.pdf.

[31]See https://www.cms.gov/Medicare/Provider-Enrollment-and-Certification/MedicarePr oviderSupEnroll/Downloads/TaxonomyCrosswalk.pdf.

[32]Finkelstein et al. 2016. Sources of Geographic Variation in Health Care: Evidence From Patient Migration. Quarterly Journal of Economics. 131 (4): 1681-1726.

[33] Retrived from http://www.nber.org/data/icd9-icd-10-cm-and-pcs-crosswalk-general-equivalence-mapping.html.

[34]Hwang et al. 2001. Out-Of-Pocket Medical Spending for Care of Chronic Conditions. Health Affairs. 20 (6): 267-278.

the number of chronic conditions for each individual by counting the number of unique ICD-9 codes matched to chronic conditions for each individual across their 2015 claims.

SNAP application and enrollee data

We received SNAP application data from DHS for all individuals on the original Medicaid outreach list. We obtained data on SNAP applications from March 2008 through February 2018 . For each SNAP application, the data contain the date the application was received, a disposition code, a disposition date, and a reason for rejection (if rejected). We use the date the application was received to define the date the individual applied for SNAP - our primary application measure is whether the individual applied for SNAP within 9 months of her initial mail date.

We use the disposition code to determine whether the application was approved, rejected or pending. The disposition date tells us when this disposition occurs. We define an application as rejected if they applied after the initial mail date and were rejected with a disposition date within 9 months after the initial mail date.

We define an individual as enrolled in SNAP if her application is approved with a disposition date within 9 months after the initial mail date. Note that because the definition of enrollee does not depend on the date the application was received, it is possible for us to record someone as a SNAP enrollee but not a SNAP applicant, if they applied before their initial mail date but enroll after it. In practice, this applies to about 2 percent of our SNAP enrollees, with the earliest application date 40 days before the initial mail date.

SNAP enrollee benefits

We received monthly benefit information for anyone on the original Medicaid outreach list enrolled in SNAP from March 2008 through February 2018 . We use these data to measure monthly benefits for our SNAP enrollees (defined in the previous section as individuals with an application approved during our nine-month observation window) in any months they are enrolled in the 9 months after the initial mail date. In principle the monthly benefit amount should be constant. However, many enrollees have a different benefit amount in the first month they are enrolled, presumably reflecting some pro-rating of benefits that (partial) month. We therefore measure benefits based on subsequent months if they disagree; for about two-thirds of enrollees, benefits are the same for all remaining months; when they are not, we use the modal benefit amount (or in rare cases of two modes, the most recent modal amount).

As noted in the text, while in principle we should be able to observe benefits for all individuals whom we measure as enrolled, in practice we are missing benefit information for about 4 percent of enrollees. If we instead define enrollment based on receiving positive benefits in any month since enrollment, this slightly increases our enrollment estimates (because we do not require the application to be approved during our nine-month observations window) but does not otherwise affect our results. Specifically, compared to our baseline findings in Table II of enrollment rates of

5.8% for control, 10.5% for Information Only, and 17.6% in Information Plus Assistance, we now estimate 8.8% for control, 14.2% for Information Only, and 21.0% for Information Plus Assistance.

Because SNAP is a benefit at household level, we also receive the number of individuals in the household linked to a given case; we use this to define the household size for enrollees.

D.2: Call-in data and call forwarding service

This section provides more detail on the call-in data, the call forwarding service, and the script for the call receptionists, which was provided in English and in Spanish.

We report the "raw" call-in rates in each study arm. Because the call forwarding service is 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. We also provide details on this adjustment procedure here.

Call-in Data for Information plus Assistance Treatment

BDT tracks all calls that it receives, and this forms the basis for the measure of call-ins to the BDT number in response to the outreach letters. We define a caller as someone who calls in to the appropriate phone number during business hours (9am-5pm) in the 9 months after the mail date. We exclude very small amount of cross-arm call contamination (e.g., individuals in control group calling in to the BDT number), and we also exclude calls beyond the 9 month window. BDT uses internal software that attempts to automatically determine the identity of caller. If the software is not able to determine identity automatically in real time, the BOS handling the phone call will ask the individual for additional identifying information (e.g., name, address) to try to determine identity. The BOS will also ask for the identification number on the printed letter or postcard to determine identity, if necessary. BDT provided us with both "caller-level" data as well as "call-level" data, and we take union of two files to determine individual-level call-in rates.

Call-in Data for Information Only Treatment

In order to capture comparable information on which individuals call in to DHS in response to the Information Only treatment, we contracted with a call forwarding service, HostedNumbers (HN) (www.hostednumbers.com). We arranged for a different 1-800 number for each treatment arm. If an individual called into one of these numbers, it would be directed to a call receptionist employed by HostedNumbers. The call receptionists were asked to read from a standard script and were asked to record the individual's identification number (printed on the outreach materials) before forwarding the call to DHS.

We worked with HostedNumbers (HN) to design a protocol that would try to capture comparable information to what BDT captures on which individuals call in to DHS in response to

the Information Only treatment. We provided a "call script" in English and Spanish which the call receptionists were instructed to follow. The receptionists were instructed to ask for the nine digit beneficiary ID number on the letter (or postcard) that was received. The receptionists were instructed not to ask for any other information, and were told to interrupt callers if the caller was providing any other information. The goal was simply to collect the ID number and then forward the call to the Pennsylvania Department of Human Services.

The call receptionists used HN software to record the date and time of call, the length of call, ID number, and whether or not the call was successfully transferred to DHS. This data is transferred to us as "call-level" data, which we use to define valid calls during business hours (8:45am-4:45pm). Note that the valid call time is very slightly different from BDT hours (9am-5pm) because BDT hours are slightly different from DHS hours.

Calculating an adjusted call-in rate Because HN does not have access to the same software as BDT to determine the identity of caller, we expected that the HN data would be less successful at measuring call-in rates when using only calls with a valid ID number. This is one explanation for the 3 percentage point lower raw caller rate in the Information Only treatment arm shown in Table II.

As a result, we also developed an adjusted measure of the call-in rate that adjusts the "raw" call-in rate. The adjustment uses information on the number of calls from callers who call in and are successfully transferred to DHS, but do not provide a valid ID number in the HN database. This might occur, for example, if the call receptionist was not able to record it properly or the individual did not find the number of the letter or postcard.

To construct an adjusted call-in rate for the Information Only treatment arm, we pool the data across each of the Information Only sub-treatments, and we make the following assumptions and calculations:

1. Let N be the total number of individuals in Information Only treatment.

2. Let A be the total number of calls with a valid ID during the time period from the first day after the first mail batch to 9 months after the last letter batch (i.e., between 1/6/2016 and 12/15/2016). We use this period rather than 9 months after the mail date because for unknown calls, we do not know the mail date. Since distribution of calls is heavily skewed towards the beginning of time period, we still expect this to be a good estimate of the actual number of calls with a valid ID during the "study period" (defined as 9 months after mail date).

3. Let B be the total number of callers in Information Only treatment without any adjustment (the "raw" call-in rate).

4. Let C be the total number of calls *without* a valid ID. We only include calls between 1/6/2016 and 12/15/2016, during business hours, and calls that were successfully transferred to DHS.

5. Let D be the estimated number of calls that are not part of study, either because they are "test calls" that we made ourselves to HN during study period or because the calls are unsolicited calls from individuals outside of study population. To construct estimate of unsolicited calls, we calculate number of unsolicited calls in 4 months before study period and we multiply by 2.875 (11.5/4) to scale up this estimate to match period 1/6/2016-12/15/2016. This assumes that rate of unsolicited calls from outside study population is same during study period as in the 4 months before study period.

6. Let E = C - D, which is number of calls without valid ID that we belief are callers from study population.

7. Let F = B/A represent estimated probability of an unknown call coming from a caller, adjusting from repeated calls. This assumes that rate of repeat calls from population that provides valid ID is same as for callers who do not provide a valid ID.

8. Let G = B + E*F be estimate of number of "adjusted" callers for Information Only treatment.

As can be seen in Table II, the adjusted caller rate in the information-only treatment is about 2 percentage points higher than the raw caller rate. To construct adjustment for each Information Only sub-treatment, we assume that the adjustment ratio is the same across arms and use the overall adjustment ratio for each arm.

Cross-contamination across arms

In processing the call-in data from both BDT and HN, we find a very small amount cross-contamination across all arms, meaning that we find calls from individuals to phone numbers different from the phone number that they are assigned. In some cases, this could reflect the fact that individuals find out about BDT through other channels. In other cases, this could reflect mistakes in the mail room in assigning letter batches. In either case, we proceed by only analyzing calls to the appropriate phone number, and we ignore cross-contamination calls. The extent of cross contamination is extremely small; see Appendix table A8.

E: Additional Analyses

Lee (2009) Bounds for Missing Benefits

In principle, we should observe benefits for all individuals whose applications have been approved during our nine-month observation window (our measure of "enrollee"). In practice, we are missing such information for about 4 percent of enrollees, and this missing rate is not balanced across arms. As shown in Table IV, 7.3 percent of control enrollees are missing benefit information, compared to 4.3 percent of the Information Only enrollees and 2.8 percent of the Information Plus Assistance enrollees; differences in missing benefit rates are statistically significantly different between either

intervention arm and the control group. Such non-random attrition could bias our comparison of enrollee benefits across arms.

Therefore we implement the fairly conservative procedure of Lee (2009) to bound the potential bias arising from differential rates of missing benefits across study arms. We apply this approach to the the average monthly benefit of enrollees in each arm, as well as the share of enrollees with certain benefit amounts. Since we found that enrollees in both treatment arms had lower benefit amounts than the control arm, and enrollees in both treatment arms have lower missing benefit rates than the control arms, we remove the lowest observed benefits from the enrollees in each treatment arm until the share of enrollees with observed benefits in each treatment arm is equal to the share in the control arm. Lee (2009) shows that under a monotonicity assumption - any enrollee for whom we observe benefits in the treatment arm we would have observed benefits for had he/she been in the control arm - a lower bound for the effect of the treatment arm can be calculated for dropping treatment individuals with the lowest outcome values until the effective response rates are equalized between the two groups.

The computation follows Tauchmann (2014)[35]. We use STATA command: leebounds. The results - shown in Table A1 below, are similar to the unadjusted results in Table IV, which for comparison purposes we reproduce here. The adjusted benefit amounts for each intervention arm are still lower than for the control arm, and the differences are statistically significantly if they were in the unadjusted results in Table IV.

Generating Predicted Benefits and Predicted Enrollment

We estimate predicted benefits using everyone in the study population who enrolled in SNAP in the 9 months following the initial mail date and for whom we observe a benefit amount. The covariates are all pre-randomization variables taken from Table I. Specifically, our predictors are dummies for age 80+, white, black, other race (unknown is the omitted race category), male, primary language non-English, residence in Pittsburgh, last Medicaid enrollment spell started before 2011, and enrolled in Medicaid for the full year of 2015; we also include as predictors continuous measures for the number of recorded chronic conditions in the Medicaid claims in 2015 and 2015 annualized health measures for health care spending, number of hospital days, SNF days, ED visits, and doctor visits.

There are clear modes in the distribution of benefits received, corresponding to minimum and maximum benefit amounts. To address this, we classify benefits into one of seven categories as shown in Appendix Table A2 . We use the "One-Vs-All" method for multi-class classification (Rifkin and Klautau 2004). Specifically, we estimate seven separate Logit models, where each model has dependent variable that takes on value of 1 for a given category and 0 otherwise. We then compute fitted value from each of these Logit models and we assign predicted category based on which fitted value is highest (e.g., if the fitted value is highest from the Logit model with

[35]The Stata Journal (2014) 14, Number 4, pp. 884–894, retrieved from https://www.stata-journal.com/sjpdf.html?articlenum=st0364.

the category 3 indicator as the dependent variable, then we assign category 3 as the predicted category). In order to avoid systematically underpredicting extreme categories, we adjust fitted values by adding and subtracting constant terms in each of the 7 Logit models and we iteratively adjust these constant terms until we have the overall predicted category shares that match the actual data. This does not adjust any of the Logit coefficients themselves, but ensures that the predicted category assignments are "unbiased" (i.e., for each category we predict the same number of observations as actually appear in that category in the data). We then convert each category to a predicted benefit amount in dollars by using the average actual benefit level in each category in the actual data.

Appendix Figures A6 shows our fit, which shows very close match across categories by design. This confirms that the algorithm appears to be unbiased in its predictions. To assess accuracy of the predictions, we calculate that roughly 38% of the observations are categorized correctly, and 78% of the predicted categories are only "off by one" category. Thus, we conclude that the machine learning algorithm appears to have limited bias and a high degree of accuracy.

We estimate predicted enrollment in SNAP in the 9 months after the initial mail date using everyone in the control group. The predictors are the same as the estimation of predicted benefits. Since enrollment is a binary outcome, we estimate a Logit model, with the dependent variable taking on value of 1 for enrollee and 0 otherwise. We then compute the fitted value from the Logit model. To achieve an unbiased prediction, we adjust a "threshold" value interactively by adding or subtracting constant terms so that by assigning the observations whose fitted values are greater than the "threshold" to predicted enrollees, we have the overall predicted enrollment rate that matches the actual enrollment rate in the control group. We then apply this "threshold" to the whole study population.

We assess the accuracy of our predictions. We calculate that roughly 90% of the observations have the correct predicted enrollment. In addition, we calculate sensitivity to be 10% and specificity to be 94%.[36] Thus, we conclude that the algorithm appears to have a high degree of accuracy when prevalence is low, which is the case we expect for the study population.

[36] We calculate sensitivity as the ratio of true positive and the sum of true positive and false negative; and we calculate specificity as the ratio of true negative and the sum of true negative and false positive. Note that the overall accuracy is the prevalence weighted sum of these two values.

F: Related Literature

Our paper relates to two strands of literature: analysis of barriers to take-up and analysis of how barriers to take-up affect the *characteristics* of applicants and enrollees. As noted in the Introduction, our information and assistance inteventions are similar to those studied in two prior randomized evalutaions of SNAP interventions: an informational intervention by Daponte et al. (1999) and an assistance intervention by Schanzenbach (2009). We discuss both briefly here, as well as other studies of barriers to take-up and of targeting. Studies of barriers to take-up have, with the exception of Bettinger et al. (2012), focused on either informational barriers or transaction costs, rather than analyzing them together as we do here.[37] This targeting literature has focused primarily on the descriptive, with little of the normative analysis we add here.

Informational Barriers to Take-up

Reductions in informational barriers have been found to be quantitatively important in generating take-up in some contexts but not others. In a recent series of randomized interventions aimed at increasing take-up of the EITC among likely eligible individuals, Day Manoli and co-authors have found that take-up is highly sensitive to both the frequency and nature of reminder letters sent by the IRS, although the effects of the reminder do not persist into the following year when the individuals would have to sign up again (Bhargava and Manoli 2015, Manoli and Turner 2016, Guyton et al. 2016). Quasi-experimental studies have also found that information is an important barrier to take up of SSDI (Armour, 2018) and post-secondary enrollment among unemployment insurance recipients (Barr and Turner, 2018). Several of these studies conclude, as we do, that the results are consistent with misperceptions by individuals (see, e.g., Bhargava and Manoli 2015, Armour 2018). However, Alcott and Greenstone's (2017) randomized evaluation finds that informational interventions do not affect take-up of home energy efficiency audits, concluding that lack of awareness is not a contributor to low take-up; likewise, Bettinger et al.'s (2012) randomized evaluation finds that providing low-income families with information about financial aid eligibility and nearby colleges had no effect on applications to college, and Dynarski et al. (2018) find that an information intervention that informed high-acheving students about a tuition-free college scholarship increased enrollment at a flagship state university.

In the SNAP context, a survey of likely eligible SNAP non-participants found that about half reported that they were not aware of their eligibility (Bartlett et al. 2004). And 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

[37] The literature has paid comparatively less attention to the role of stigma, but the limited evidence does not point to a large role for stigma (Currie 2006). Recent efforts at "stigma" interventions have proven less successful at increasing take-up than informational interventions such as reminders or simplification (Bhargava and Manoli 2015). In the specific context of SNAP, Currie (2003) describes several pieces of survey evidence consistent with both lack of awareness and transaction costs in reducing SNAP take-up, but concludes that it does not appear from the existing survey evidence that stigma is a major deterrent to SNAP enrollment.

that they had subsequently signed up for SNAP, while no households in the control group reported signing up for SNAP. However, small sample sizes as well as loss to follow-up made definitive conclusions difficult.

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 IV) 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 IV). 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.

Transacational Barriers to Takeup

Reductions in transactional barriers have been found to be important for increasing enrollment in several different programs. Bettinger et al. (2012) found that while information alone was ineffective, combining information with assistance in completing a streamlined application process increased aid applications and ultimately college attendance and persistence by low-income individuals. Our findings in the SNAP context suggest, by contrast, that information alone can have an effect, but that pairing it with assistance doubles the impact. In addition, Deshpande and Li (forthcoming) find that the closing of local field offices where SSDI and SSI applications can be submitted substantially reduces both applications and enrollment, and Rossin-Slater (2013) finds that openings and closings of Women, Infants and Children (WIC) local program office affect program participation. Alatas et al. (2016) present evidence from a randomized evaluation across Indonesian villages that increasing the transaction cost of applying for a conditional cash transfer program reduces enrollment. At the extreme of reducing transaction barriers, defaulting to enrollment has been found to have substantial effects on outcomes such as participation in tax-subsidized 401(k) savings plans (Madrian and Shea 2001).

In the SNAP context, Schanzenbach (2009) provides evidence from one California county of a randomized evaluation in which a low-income tax preparer provided assistance to likely SNAP-eligible individuals. It found that, among those who expressed interest in learning more about SNAP, those in offices randomized into full assistance (in which the 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).

Targeting

Our paper also relates to a second strand of the literature that investigates how barriers to enrollment affect the *characteristics* of applicants and enrollees. The existing "targeting" literature has been primarily descriptive, focusing on the observable characteristics of individuals affected by different barriers. Our theoretical framework, however, suggests that there is no general relationship between this targeting on observables and the impact of the intervention on either private or social welfare. We provide additional conditions that need to be examined empirically in order for an intervention's targeting properties to yield normative implications.

To our knowledge, our study is the first to examine the targeting properties of both an information intervention and an assistance intervention. From prior information interventions, there is evidence that complexity disproportionately deters EITC enrollment of lower income potential recipients (Bhargava and Manoli 2015), and that lower income employees are more likely to choose dominated health insurance plans, due at least in part to a lack of insurance literacy (Bhargava et al., 2017). Our findings, by contrast, suggest that information about eligibility disproportionately encourages enrollment among less needy applicants.

Prior studies have tended to find that transaction costs increase targeting on some but not all dimensions. Alatas et al. (2016) find that introducing transaction costs by requiring individuals to apply for a conditional cash transfer in Indonesian villages rather than have the government automatically screen the individuals for eligibility improves targeting; specifically, it results in substantially poorer enrollees. However, marginally increasing the transaction costs does not further affect the characteristics of enrollees. Deshpande and Li (forthcoming) find that increasing transaction costs in U.S. disability programs (SSDI and SSI) worsens targeting among applicants - by increasing the share of applicants with only moderately severe disabilities - but increases targeting among enrollees, decreasing the share of enrollees with the least severe disabilities (conditional on being severe enough to be eligible); however they also find that the increased transaction costs reduce the share of enrollees with low education levels and low pre-application earnings, suggesting a reduction in targeting. In our context, by contrast, we find that reducing transaction costs decrease targeting on all dimensions, and at all stages (application and enrollment).

G. Appendix References

Alberto Abadie, 2002, "Bootstrap Tests for Distributional Treatment Effects in Instrumental Variable Models," *Journal of the American Statistical Association*, 97 (457), 284-292.

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

Alatas Vivi, Abhijit Banerjee, Rema Hanna, Benjamin Olken, Matthew Wai-Poi, and Ririn

Pernamasari, "Self-Targeting: Evidence from a Field Experiment in Indonesia," *Journal of Political Economy*, 124 (2016), 371-427.

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

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

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

Bartlett, Susan, Nancy Burstein, and William Hamilton, "Food Stamp Access Study: Final Report," United States Department of Agriculture, Economic Research Service, November, (2004). https://www.ers.usda.gov/publications/pub-details/?pubid=43407 (last accessed July 31, 2017).

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

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

Bhargava, Saurabh, and Dayanand Manoli, "Psychological Frictions and the Incomplete Take-Up of Social Benefits: Evidence from an IRS Field Experiment," *American Economic Review,* 105 (2015), 3489–3529.

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

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

Currie, Janet, "US Food and Nutrition Programs", in Means-Tested Transfer Programs in the United States, Robert Moffit, ed. (Chicago, Illinois: University of Chicago Press, 2003).

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

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

Deshpande, Manasi, and Yue Li, "Who Is Screened Out? Application Costs and the Targeting of Disability Programs," *American Economic Journal: Economic Policy*, Forthcoming.

Dynarski, Susan, C. J. Libassi, Katherine Michelmore, and Stephanie Owen, "Closing the Gap: The Effect of a Targeted, Tuition-Free Promise on College Choices of High-Achieving, Low-Income Students," NBER Working Paper No. w25349, 2018.

Finkelstein, Amy, Matthew Gentzkow, and Heidi Williams, "Sources of Geographic Variation in Health Care: Evidence From Patient Migration," *Quarterly Journal of Economics*, 131(2016),

1681-726.

Gray, Kelsey, Sarah Fisher, and Sarah Lauffer, "Characteristics of Supplemental Nutrition Assistance Program Households: Fiscal Year 2015 (No. Report No. SNAP-16-CHAR)," United States Department of Agriculture, 2016.

Guyton, John, Dayanand Manoli, Brenda Schafer, and Michael Sebastiani, "Reminders & Recidivism: Evidence from Tax Filing & EITC Participation Among Low-Income Non-Filers," NBER Working Paper No. 21904, 2016.

Hanratty, Maria J., "Has the Food Stamp Program Become More Accessible? Impacts of Recent Changes in Reporting Requirements and Asset Eligibility Limits." *Journal of Policy Analysis and Management*, 25 (2006), 603-621.

Hendren, Nathaniel, "The Policy Elasticity," *Tax Policy and the Economy,* 30 (2016), 51-89.

Hwang, Wenke, Wendy Heller, Henry Ireys, and Gerard Anderson, "Out-Of-Pocket Medical Spending for Care of Chronic Conditions," *Health Affairs*, 20 (2001), 267-278.

Kabbani, Nader S., and Parke E. Wilde, "Short Recertification Periods in the U.S. Food Stamp Program," *The Journal of Human Resources*, 38 (2008), 1112-1138.

Klerman, Jacob. A., and Caroline Danielson, "The Transformation of the Supplemental Nutrition Assistance Program," *Journal of Policy Analysis and Management*, 30 (2011), 863-888.

Lee, David, "Training, Wages, and Sample Selection: Estimating Sharp Bounds on Treatment Effects," *Review of Economic Studies*, 76 (2009) 1071-1102.

Lewin Group, "Evaluating Encounter Data Completeness," (2012), `https://www.ccwdata.org/documents/10280/19002254/evaluating-encounter-data-completeness.pdf`

Madrian, Brigitte, and Dennis Shea, "The Power of Suggestion: Inertia in 401(k) Participation and Savings Behavior," *Quarterly Journal of Economics*, 116 (2001), 1149-1187.

Manoli, Dayand, and Nicholas Turner, "Nudges and Learning: Evidence from Informational Interventions for Low-Income Taxpayers," NBER Working Paper No. 20718, 2016.

Nichols, Albert L., and Richard J. Zeckhauser, "Targeting Transfers through Restrictions on Recipients," *American Economic Review*, 72 (1982), 372–377.

Ratcliffe, Caroline, Signe-Mary McKernan, and Kenneth Finegold, "Effects of Food Stamp and TANF Policies on Food Stamp Receipt," *Social Service Review*, 82 (2008), 291-334.

Rifkin, Ryan, and Aldebaro Klautau, "In Defense of One-Vs-All Classification," *Journal of Machine Learning Research*, 5 (2008), 101-141.

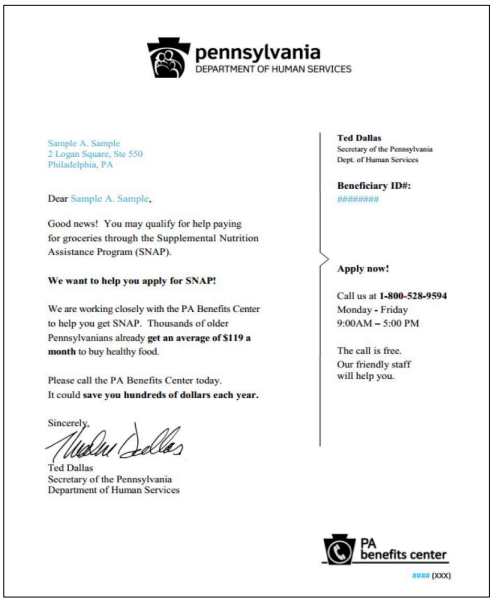
Rossin-Slater, Maya, "WIC in Your Neighborhood: New Evidence on the Impacts of Geographic Access to Clinics," *Journal of Public Economics*, 102 (2013), 51-69.

Schanzenbach, Diane. "Experimental Estimates of the Barriers to Food Stamp Enrollment," Institute for Research on Poverty, University of Wisconsin-Madison, Discussion Paper No. 1367-09, 2009.

Tauchmann, Harald Lee, "Treatment-Effect Bounds for Nonrandom Sample Selection," *The Stata Journal*, 14 (2009), 884–894.

Figure A1
Standard Outreach Materials: Information Plus Assistance

Letter



pennsylvania
DEPARTMENT OF HUMAN SERVICES

Sample A. Sample
2 Logan Square, Ste 550
Philadelphia, PA

Dear Sample A. Sample,

Good news! You may qualify for help paying for groceries through the Supplemental Nutrition Assistance Program (SNAP).

We want to help you apply for SNAP!

We are working closely with the PA Benefits Center to help you get SNAP. Thousands of older Pennsylvanians already **get an average of $119 a month** to buy healthy food.

Please call the PA Benefits Center today.
It could **save you hundreds of dollars each year.**

Sincerely,

Ted Dallas
Secretary of the Pennsylvania
Department of Human Services

Ted Dallas
Secretary of the Pennsylvania
Dept. of Human Services

Beneficiary ID#:
########

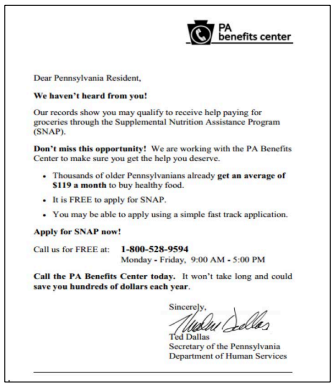
Apply now!

Call us at **1-800-528-9594**
Monday - Friday
9:00AM – 5:00 PM

The call is free.
Our friendly staff
will help you.

PA benefits center
(XXX)

Postcard



PA benefits center

Dear Pennsylvania Resident,

We haven't heard from you!

Our records show you may qualify to receive help paying for groceries through the Supplemental Nutrition Assistance Program (SNAP).

Don't miss this opportunity! We are working with the PA Benefits Center to make sure you get the help you deserve.

- Thousands of older Pennsylvanians already **get an average of $119 a month** to buy healthy food.
- It is FREE to apply for SNAP.
- You may be able to apply using a simple fast track application.

Apply for SNAP now!

Call us for FREE at: **1-800-528-9594**
Monday - Friday, 9:00 AM - 5:00 PM

Call the PA Benefits Center today. It won't take long and could **save you hundreds of dollars each year**.

Sincerely,

Ted Dallas
Secretary of the Pennsylvania
Department of Human Services

Envelope

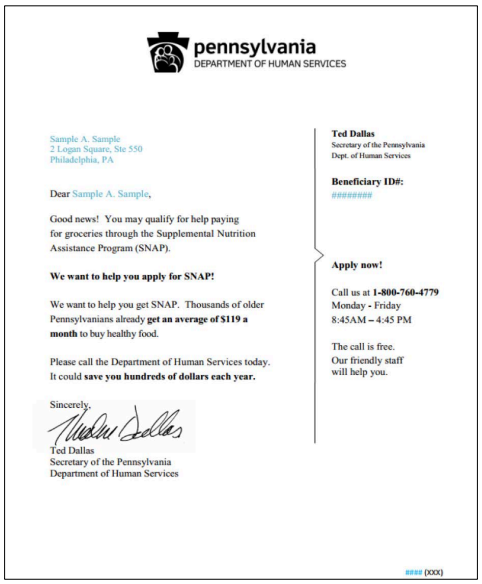
pennsylvania
DEPARTMENT OF HUMAN SERVICES

PA Benefits Center
PO Box 34658 , Philadelphia, PA 19101

FIRST CLASS
OUTBOUND
PAID
POSTAGE
PHILADELPHIA PA

Figure A2
Standard Outreach Materials: Information Only

Letter



pennsylvania
DEPARTMENT OF HUMAN SERVICES

Sample A. Sample
2 Logan Square, Ste 550
Philadelphia, PA

Dear Sample A. Sample,

Good news! You may qualify for help paying for groceries through the Supplemental Nutrition Assistance Program (SNAP).

We want to help you apply for SNAP!

We want to help you get SNAP. Thousands of older Pennsylvanians already **get an average of $119 a month** to buy healthy food.

Please call the Department of Human Services today.
It could **save you hundreds of dollars each year.**

Sincerely,

Ted Dallas
Secretary of the Pennsylvania
Department of Human Services

Ted Dallas
Secretary of the Pennsylvania
Dept. of Human Services

Beneficiary ID#:
########

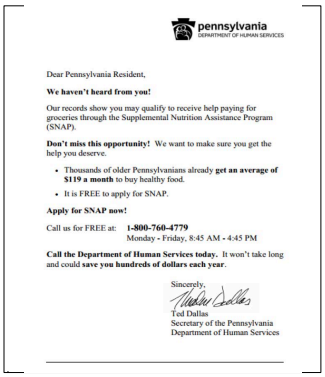
Apply now!

Call us at **1-800-760-4779**
Monday - Friday
8:45AM – 4:45 PM

The call is free.
Our friendly staff
will help you.

(XXX)

Postcard



pennsylvania
DEPARTMENT OF HUMAN SERVICES

Dear Pennsylvania Resident,

We haven't heard from you!

Our records show you may qualify to receive help paying for groceries through the Supplemental Nutrition Assistance Program (SNAP).

Don't miss this opportunity! We want to make sure you get the help you deserve.

- Thousands of older Pennsylvanians already **get an average of $119 a month** to buy healthy food.
- It is FREE to apply for SNAP.

Apply for SNAP now!

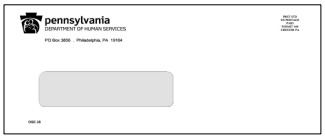
Call us for FREE at: **1-800-760-4779**
Monday - Friday, 8:45 AM - 4:45 PM

Call the Department of Human Services today. It won't take long and could **save you hundreds of dollars each year**.

Sincerely,

Ted Dallas
Secretary of the Pennsylvania
Department of Human Services

Envelope

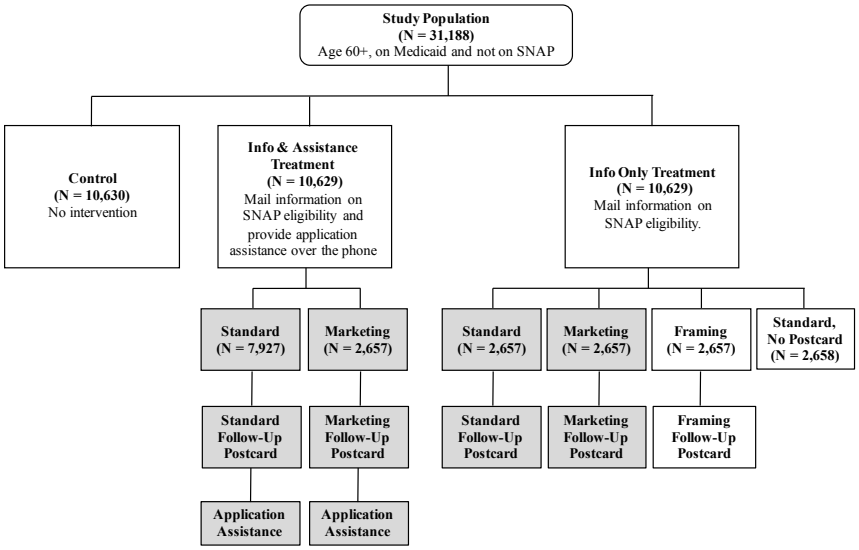


pennsylvania
DEPARTMENT OF HUMAN SERVICES

PO Box 3650 , Philadelphia, PA 19104

FIRST CLASS
OUTBOUND
PAID
POSTAGE
PHILADELPHIA PA

Figure A3
Experimental Design



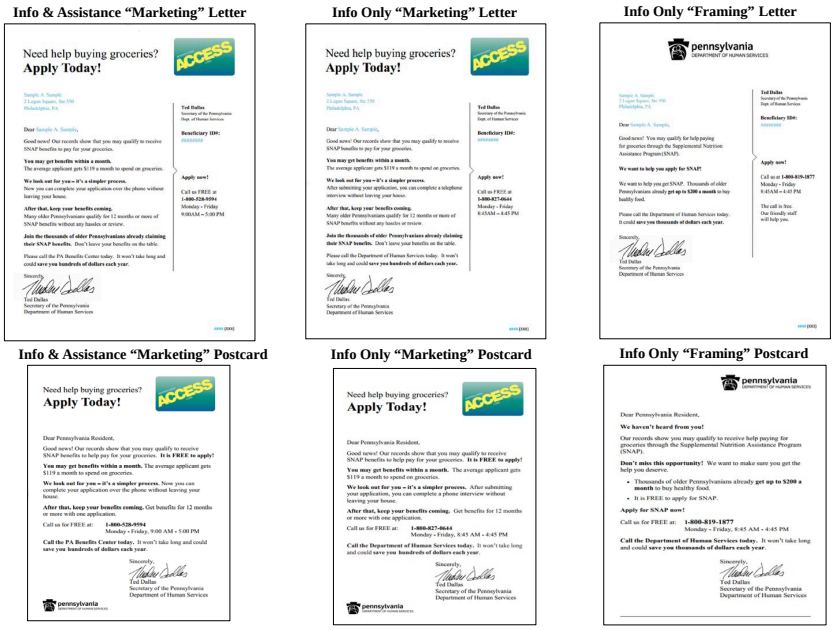
Notes: Figure shows experimental design. Grey arms are the ones included in the main analyses.

Figure A4
Timing and Sample Sizes for Mail Batches

Treatment	Subtreatment	1	2	3	4	5	6	7	8	9	10	11	Total
	Date of Initial Mailing	1/6/2016	1/13/2016	1/20/2016	1/27/2016	2/3/2016	2/10/2016	2/17/2016	2/24/2016	3/2/2016	3/9/2016	3/16/2016	
	Date of Follow Up Postcard mailing	3/2/2016	3/9/2016	3/16/2016	5/26/2016	5/27/2016	4/6/2016	4/13/2016	4/20/2016	4/27/2016	5/4/2016	5/11/2016	
Info & Assistance	Standard	750	750	750	750	750	750	750	750	750	750	472	7972
Info & Assistance	"Marketing"	250	250	250	250	250	250	250	250	250	250	157	2657
Info Only	Standard	250	250	250	250	250	250	250	250	250	250	157	2657
Info Only	Marketing	250	250	250	250	250	250	250	250	250	250	157	2657
Info Only	No Postcard	250	250	250	250	250	250	250	250	250	250	158	2658
Info Only	Framing	250	250	250	250	250	250	250	250	250	250	157	2657
Info & Assistance (Pooled)		1000	1000	1000	1000	1000	1000	1000	1000	1000	1000	629	10629
Info Only (Standard + Marketing Pooled)		500	500	500	500	500	500	500	500	500	500	314	5314
Info Only (All Subtreatments Pooled)		1000	1000	1000	1000	1000	1000	1000	1000	1000	1000	629	10629
		3000	3000	3000	3000	3000	3000	3000	3000	3000	3000	1888	31888

NOTE: Due to an implementation error, postcards for mail batches 4 and 5 were not sent as planned (eight weeks after the mail date) and therefore were sent following the last planned mailings in May 2016.

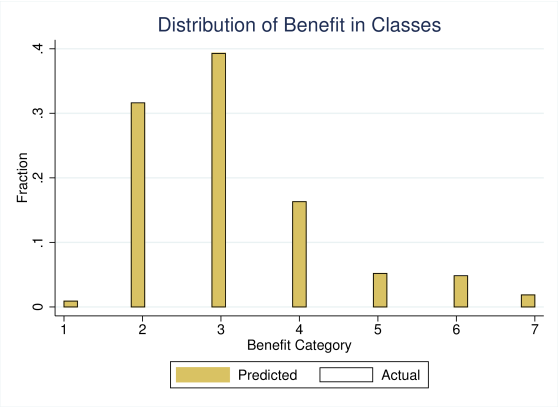
Figure A5
"Marketing" and "Framing" Outreach Materials



NOTE: Envelopes (not shown) were identical to the standard envelopes for the respective arm (Information Plus Assistance, or Information Only) shown in Appendix Figures A1 and A2 respectively.

Figure A6
Predicted and Actual Enrollee Monthly Benefits

Panel A: In Categories



Panel B: In Dollars

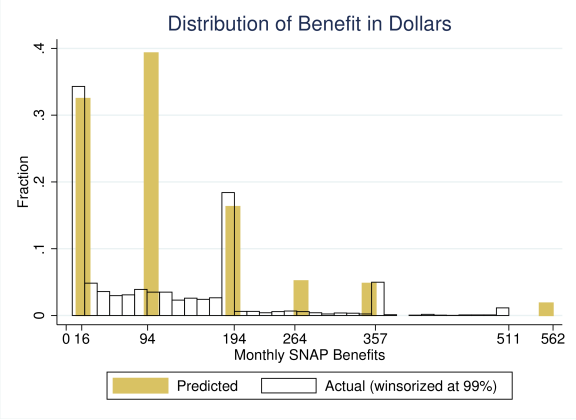
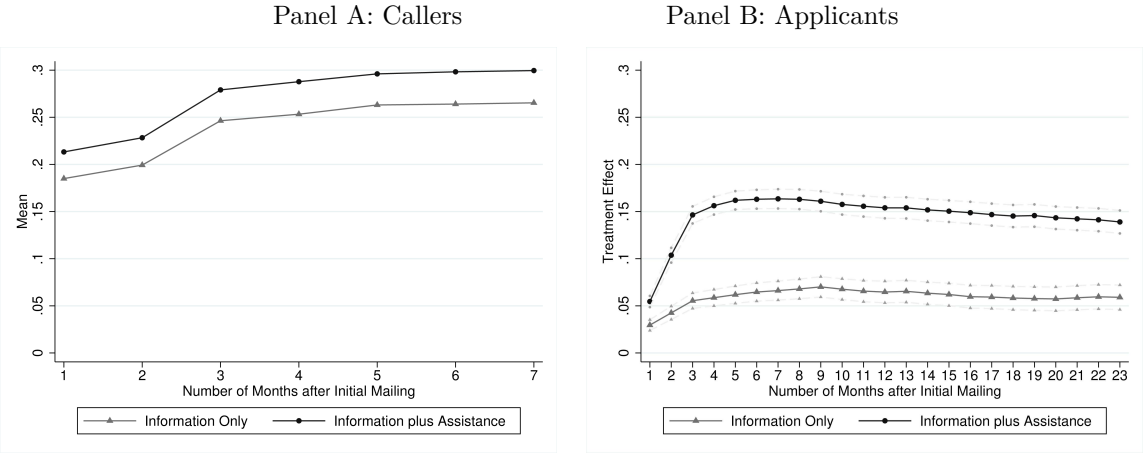
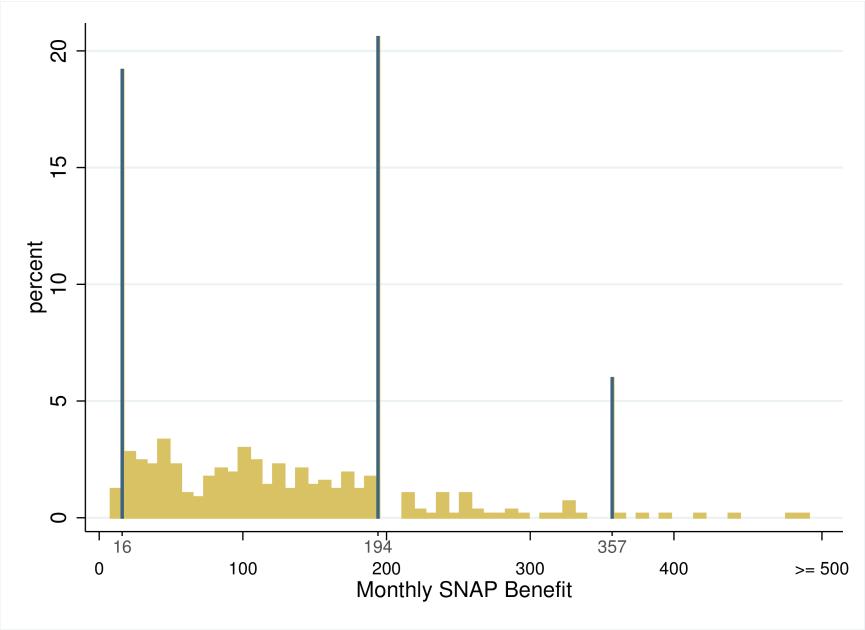


Figure A7
Time pattern of callers and applications



Notes: Figure shows (cumulative) outcomes by month relative to the initial mailing. Panel A shows the mean (cumulative) caller rates by month; the Information Only caller rate shown is unadjusted for under-measurement (see text for more details). Panel B shows the estimated treatment effects (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.

Figure A8
Distribution of enrollee benefits: control group enrollees



Notes: Figure plots the monthly enrollee benefit amount among enrollees in the control group (N=568). The three modes - which are the minimum benefit for a categorically eligible household of size 1 or 2 ($16), the maximum monthly benefit for a household of size 1 ($194), and the maximum monthly benefit for a household of size 2 ($357) - are binned separately from the other values; values greater than $500 are set to $500.

Table A1
Bounding bias from missing benefit amount

	Control	Information Only		Information Plus Assistance	
		Unadjusted	Lee Bound	Unadjusted	Lee Bound
	(1)	(2)	(3)	(4)	(5)
Benefit Amount	145.94	115.38	118.67	101.32	105.51
		[0.000]	[0.000]	[0.000]	[0.000]
Share $16 Benefit	0.192	0.312	0.322	0.367	0.385
		[0.000]	[0.000]	[0.000]	[0.000]
Share $194 Benefit	0.206	0.164	0.170	0.147	0.154
		[0.076]	[0.102]	[0.003]	[0.003]
Share $357 Benefit	0.060	0.052	0.054	0.040	0.042
		[0.587]	[0.691]	[0.077]	[0.135]
N	613	559		1,861	

Notes: Table shows benefit amounts among enrollees in different arms. Enrollees with missing benefits are excluded. Column (1) shows the control means. Columns (2) and (3) show the means (with the p-value (relative tothe control) in [square brackets] show the means for the Information Only intervention. Column 2 reports the unadjusted mean (as shown in Table IV); Column 3 reports an upper bound on the mean, based on the Lee (2009) bounding procedure described in the text. Columns (4) and (5) present parallel results for the Information Plus Assistance Intervention. All p-values are calculated via the bootstrap.

Table A2
Enrollee Monthly Benefits Categorization

Category	Criteria	Observations	Share of Observations
1	Monthly Benefit < $16	31	0.90
2	Monthly Benefit = $16	1,084	31.62
3	Monthly Benefit > $16 and Monthly Benefit < $194	1,346	39.26
4	Monthly Benefit = $194	559	16.31
5	Monthly Benefit > $194 and Monthly Benefit < $357	178	5.19
6	Monthly Benefit = $357	166	4.84
7	Monthly Benefit > $357	64	1.87
Total		3,428	

Table A3
Balance of Characteristics of Study Population Across Arms

	Control	Information Only	Information Plus Assistance	P Value of Difference (Column 2 vs 3)
	(1)	(2)	(3)	(4)
Panel A - Demographics				
Age (as of October 31, 2015)	68.80	68.93	68.80	
		[0.425]	[0.975]	[0.434]
Share Age 80+	0.16	0.17	0.16	
		[0.349]	[0.861]	[0.459]
Male	0.38	0.38	0.38	
		[0.965]	[0.702]	[0.718]
Share White [a]	0.76	0.76	0.75	
		[0.634]	[0.089]	[0.330]
Share Black [a]	0.08	0.07	0.08	
		[0.371]	[0.281]	[0.079]
Share Primary Language not English	0.04	0.04	0.04	
		[0.377]	[0.574]	[0.191]
Share Living in Pittsburgh	0.06	0.06	0.06	
		[0.737]	[0.871]	[0.854]
Share Last Medicaid Spell Starting before 2011	0.33	0.33	0.34	
		[0.629]	[0.287]	[0.665]
Share Enrolled in Medicaid for 2015 Full Year	0.73	0.73	0.72	
		[0.738]	[0.515]	[0.820]
Panel B - (Annual) Health Care Measures, 2015				
Total Health Care Spending ($) [b]	11,755	11,517	12,197	
		[0.632]	[0.325]	[0.201]
Number of Hospital Days	2.09	1.93	2.29	
		[0.470]	[0.378]	[0.151]
Number of ER Visits	0.47	0.59	0.50	
		[0.160]	[0.532]	[0.297]
Number of Doctor Visits	7.20	7.01	7.23	
		[0.515]	[0.920]	[0.514]
Number of SNF Days	2.65	2.39	2.81	
		[0.459]	[0.623]	[0.269]
Number of Chronic Conditions	5.46	5.34	5.44	
		[0.337]	[0.809]	[0.477]
F Statistic		0.560	0.660	0.746
P Value		[0.906]	[0.825]	[0.752]
Observations (N)	10,630	5,314	10,629	

NOTE: Table shows means of pre-randomization characteristics for the study population. Columns 1 through 3 show means by intervention arm with the p-value (relative to the control arm) in [square brackets] Column 4 reports the p-value for the difference between the two intervention arms. The bottom rows of the table report the F-statistic (and the p-value on that F-statistic) from the joint test of equality across all of the pre-randomization characteristics shown. P-values of the differences in characteristics are based on heteroskedasticity-robust standard errors. The F-statistic (and associated p-value) is calculated based a regression in which we "stack" all of the variable values into a single left-hand side outcome variable and interact the treatment indicator with variable fixed effects; the F-distribution is simulated using permutation with 1,000 iterations.

Table A4
Balance of Characteristics of Study Population: By Sub treatments

	Control	Information Only				Information Plus Assistance		P Value of Difference between			
		Standard	No-Postcard	Marketing	Framing	Standard	Marketing	Control vs Treatment (col 1 vs 2+4+6+7)	Standard vs Marketing (col 2+6 vs 4+7)	Information Only Standard vs Framing (col 2 vs 5)	Information Only Standard vs No Follow-up Postcard (col 2 vs 3)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Panel A - Demographics											
Age (as of October 31, 2015)	68.80	68.95	69.10	68.90	68.89	68.68	68.91				
		[0.472]	[0.136]	[0.608]	[0.662]	[0.388]	[0.589]	[0.622]	[0.583]	[0.817]	[0.559]
Share Age 80+	0.16	0.17	0.17	0.16	0.17	0.16	0.17				
		[0.153]	[0.518]	[0.997]	[0.402]	[0.630]	[0.574]	[0.484]	[0.734]	[0.637]	[0.532]
Male	0.38	0.38	0.37	0.38	0.38	0.38	0.37				
		[0.987]	[0.391]	[0.959]	[0.814]	[0.602]	[0.378]	[0.851]	[0.463]	[0.843]	[0.490]
Share White [a]	0.76	0.76	0.75	0.75	0.75	0.75	0.75				
		[0.728]	[0.590]	[0.698]	[0.407]	[0.154]	[0.179]	[0.205]	[0.798]	[0.703]	[0.880]
Share Black [a]	0.08	0.07	0.07	0.07	0.08	0.08	0.08				
		[0.599]	[0.463]	[0.387]	[0.651]	[0.848]	[0.186]	[0.954]	[0.576]	[0.439]	[0.871]
Share Primary Language not English	0.04	0.04	0.05	0.05	0.04	0.04	0.04				
		[0.897]	[0.296]	[0.149]	[0.254]	[0.815]	[0.342]	[0.790]	[0.723]	[0.433]	[0.347]
Share Living in Pittsburgh	0.06	0.05	0.05	0.06	0.06	0.06	0.05				
		[0.338]	[0.468]	[0.685]	[0.852]	[0.552]	[0.520]	[0.760]	[0.853]	[0.370]	[0.858]
Share Last Medicaid Spell Starting before 2011	0.33	0.33	0.33	0.33	0.36	0.33	0.34				
		[0.849]	[0.888]	[0.577]	[0.006]	[0.974]	[0.142]	[0.362]	[0.253]	[0.043]	[0.969]
Share Enrolled in Medicaid for 2015 Full Year	0.73	0.73	0.74	0.73	0.76	0.73	0.72				
		[0.857]	[0.276]	[0.737]	[0.005]	[0.599]	[0.596]	[0.561]	[0.841]	[0.020]	[0.317]
Panel B - (Annual) Health Care Measures, 2015											
Total Health Care Spending ($) [b]	11,755	11,514	12,630	11,520	11,860	11,561	12,833				
		[0.711]	[0.205]	[0.710]	[0.871]	[0.654]	[0.109]	[0.796]	[0.230]	[0.674]	[0.193]
Number of Hospital Days	2.09	1.88	2.33	1.97	2.27	2.29	2.29				
		[0.474]	[0.474]	[0.680]	[0.565]	[0.359]	[0.563]	[0.914]	[0.848]	[0.308]	[0.262]
Number of ER Visits	0.47	0.59	0.44	0.59	0.44	0.54	0.46				
		[0.180]	[0.420]	[0.392]	[0.459]	[0.271]	[0.819]	[0.144]	[0.699]	[0.108]	[0.103]
Number of Doctor Visits	7.20	6.81	6.85	7.22	6.67	7.23	7.23				
		[0.219]	[0.275]	[0.973]	[0.103]	[0.900]	[0.955]	[0.736]	[0.549]	[0.728]	[0.903]
Number of SNF Days	2.65	2.02	2.97	2.76	2.96	2.55	3.07				
		[0.138]	[0.511]	[0.820]	[0.525]	[0.753]	[0.391]	[0.862]	[0.098]	[0.106]	[0.101]
Number of Chronic Conditions	5.46	5.28	5.63	5.40	5.31	5.52	5.35				
		[0.255]	[0.345]	[0.719]	[0.337]	[0.634]	[0.503]	[0.456]	[0.886]	[0.890]	[0.102]
F Statistic		0.856	1.038	0.399	1.367	0.555	0.972	0.617	0.676	1.044	1.143
P Value		[0.644]	[0.450]	[0.980]	[0.197]	[0.918]	[0.481]	[0.867]	[0.816]	[0.377]	[0.336]
Observations (N)	10,630	2,657	2,658	2,657	2,657	7,972	2,657				

Notes: Table shows means of pre-randomization characteristics for the study population. Columns 1 through 7 show means by intervention sub-arm with the p-value (relative to the control arm) in [square brackets] Columns 8 through 11 report the p-value for differences between various groups of sub-arms, as indicated. The bottom rows of the table report the F-statistic (and the p-value on that F-statistic) from the joint test of equality across all of the pre-randomization characteristics shown. P-values of the differences in characteristics are based on heteroskedasticity-robust standard errors. The F-statistic (and associated p-value) is calculated based a regression in which we "stack" all of the variable values into a single left-hand side outcome variable and interact the treatment indicator with variable fixed effects; the F-distribution is simulated using permutation with 1,000 iterations.

Table A5
Behavioral Responses to Interventions: All sub-treatments

	Control	Information Only				Information Plus Assistance		P Value of Difference between			
		Standard	No-Postcard	Marketing	Framing	Standard	Marketing	Control vs Treatment (col 1 vs 2+4+6+7)	Standard vs Marketing (col 2+6 vs 4+7)	Information Only Standard vs Framing (col 2 vs 5)	Information Only Standard vs No Follow-up Postcard (col 2 vs 3)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
SNAP Enrollees	0.058	0.112	0.092	0.098	0.111	0.174	0.179				
		[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.481]	[0.896]	[0.016]
SNAP Applicants	0.077	0.151	0.120	0.143	0.157	0.236	0.239				
		[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.730]	[0.543]	[0.001]
SNAP Rejections among Applicants	0.233	0.224	0.216	0.311	0.281	0.261	0.250				
		[0.751]	[0.536]	[0.005]	[0.071]	[0.116]	[0.442]	[0.115]	[0.133]	[0.065]	[0.777]
Callers	0.000	0.278	0.212	0.256	0.300	0.298	0.303				
		[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.288]	[0.079]	[0.000]
Adjusted Callers	0.000	0.300	0.234	0.278	0.322	0.298	0.303				
		[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.295]	[0.086]	[0.000]
SNAP Applicants among Non-Callers	0.077	0.089	0.074	0.084	0.093	0.085	0.077				
		[0.079]	[0.593]	[0.295]	[0.025]	[0.066]	[0.953]	[0.069]	[0.262]	[0.681]	[0.071]
SNAP Applicants among Callers	0.000	0.311	0.295	0.315	0.306	0.592	0.612				
		[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.238]	[0.830]	[0.524]
SNAP Enrollees among Non-Callers	0.058	0.064	0.054	0.058	0.062	0.060	0.058				
		[0.284]	[0.492]	[0.934]	[0.437]	[0.578]	[0.908]	[0.467]	[0.449]	[0.824]	[0.172]
SNAP Enrollees among Callers	0.000	0.237	0.234	0.215	0.225	0.442	0.457				
		[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.847]	[0.571]	[0.921]
Observations (N)	10,630	2,657	2,658	2,657	2,657	7,972	2,657				

Notes: Columns 1 through 6 show means of outcomes by intervention arm, with the p-value (relative to the control arm) in [square brackets]. Columns 8 through 11 report p-values for comparisons shown in column heading. In column 8, sub-treatments are weighted so that within the Information Plus Assistance arm the standard and marketing sub-treatments receive equal weight, and the Information Plus Assistance treatments receive equal weight as the Information Only treatments. In column 9, sub-treatments are weighted so that Information Plus Assistance and Information Only are equally weighted in Standard and Marketing 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 A6
Enrollee Benefits and Predicted Benefits: All sub-treatments

	Control	Information Only				Information Plus Assistance		P Value of Difference between			
		Standard	No-Postcard	Marketing	Framing	Standard	Marketing	Control vs Treatment (col 1 vs 2+4+6+7)	Standard vs Marketing (col 2+6 vs 4+7)	Information Only Standard vs Framing (col 2 vs 5)	Information Only Standard vs No Follow-up Postcard (col 2 vs 3)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Benefit Amount	145.94	112.60	119.72	118.55	132.33	103.96	98.72				
		[0.000]	[0.004]	[0.003]	[0.174]	[0.000]	[0.000]	[0.000]	[0.754]	[0.065]	[0.471]
Share $16 Benefit	0.192	0.309	0.295	0.316	0.264	0.367	0.368				
		[0.000]	[0.003]	[0.000]	[0.021]	[0.000]	[0.000]	[0.000]	[0.802]	[0.247]	[0.732]
Share $194 Benefit	0.206	0.161	0.184	0.168	0.156	0.148	0.146				
		[0.107]	[0.467]	[0.193]	[0.070]	[0.003]	[0.011]	[0.006]	[0.981]	[0.856]	[0.504]
Share $357 Benefit	0.060	0.035	0.060	0.072	0.069	0.038	0.041				
		[0.094]	[0.999]	[0.527]	[0.622]	[0.056]	[0.176]	[0.166]	[0.127]	[0.073]	[0.193]
Share Missing Benefit	0.073	0.044	0.045	0.042	0.064	0.021	0.036				
		[0.061]	[0.093]	[0.056]	[0.613]	[0.000]	[0.005]	[0.001]	[0.352]	[0.264]	[0.943]
Predicted Benefit for Enrollees w/ Nonmissing Benefit	140.20	111.06	126.11	114.13	131.06	106.19	99.72				
		[0.000]	[0.130]	[0.003]	[0.293]	[0.000]	[0.000]	[0.000]	[0.510]	[0.038]	[0.140]
Predicted Benefit for All Enrollees	138.65	112.99	126.04	115.17	130.07	106.57	101.56				
		[0.001]	[0.166]	[0.007]	[0.302]	[0.000]	[0.000]	[0.000]	[0.581]	[0.067]	[0.193]
Share of Enrollees in Household Size of 1	0.657	0.742	0.673	0.682	0.695	0.752	0.769				
		[0.008]	[0.652]	[0.479]	[0.256]	[0.000]	[0.000]	[0.000]	[0.637]	[0.207]	[0.084]
Benefit Amount for Enrollees in Household Size of 1	116.97	95.32	98.01	90.88	96.45	85.61	86.03				
		[0.004]	[0.010]	[0.001]	[0.005]	[0.000]	[0.000]	[0.000]	[0.686]	[0.895]	[0.754]
Observations (N)	613	298	245	261	295	1,385	476				

Notes: Sample is individuals who enrolled in the 9 months after their initial mailing. Columns 1 through 7 shows means by intervention arm with the p-value (relative to the control arm) in [square brackets] for SNAP enrollees. Column 8 - 11 report p-values for comparisons shown in the column headings. In column 8, sub-treatments are weighted so that within the Information Plus Assistance arm the standard and marketing sub-treatments receive equal weight, and the Information Plus Assistance treatments receive equal weight as the Information Only treatments. In column 9, sub-treatments are weighted so that Information Plus Assistance and Information Only are equally weighted in Standard and Marketing arms. First 5 measures are based on actual benefit amounts received by SNAP enrollees; see text for a description of the predicted benefits. All p-values are based on heteroskedasticity-robust standard errors.

Table A7
Always Taker and Complier Enrollee Benefits and Predicted Benefits

	Always Takers	Compliers		P Value of Difference (Column 2 vs 3)
		Information Only	Information Plus Assistance Arms	
	(1)	(2)	(3)	(4)
Benefit Amount	145.94	78.31	79.66	
		[0.000]	[0.000]	[0.910]
Share $16 Benefit	0.192	0.458	0.453	
		[0.000]	[0.000]	[0.911]
Share $194 Benefit	0.206	0.114	0.119	
		[0.079]	[0.002]	[0.908]
Share $357 Benefit	0.060	0.043	0.030	
		[0.595]	[0.074]	[0.599]
Share Missing Benefit	0.073	0.006	0.007	
		[0.023]	[0.000]	[0.955]
Predicted Benefit for Enrollees w/ Actual Benefit	140.20	78.87	84.83	
		[0.000]	[0.000]	[0.629]
Predicted Benefit for All Enrollees	138.65	84.10	87.21	
		[0.001]	[0.000]	[0.788]
Share of Enrollees in Household Size of 1	0.657	0.782	0.810	
		[0.035]	[0.000]	[0.581]
Benefit Amount for Enrollees in Household Size of 1	116.97	64.69	70.70	
		[0.000]	[0.000]	[0.587]
Share of Sub-Population	0.058	0.048	0.119	

Notes: Sample is individuals who enrolled in the 9 months after their initial mailing. Variables reported are the same as in Table IV. Column 1 shows the mean of the always takers (individuals who enroll regardless of intervention), while columns 2 and 3 show the means for compliers (individuals who enroll if and only if they receive the intervention) for each intervention; p-value (relative to the always takers) is in [square brackets] for SNAP enrollees. Column 4 reports the p-value of the difference between the compliers in the two intervention arms. In column 2 the two equally-sized sub-treatments are pooled; in column 3 the two pooled sub-treatments are weighted so that they receive equal weight. Standard errors and p-values are computed with 10,000 replications of the bootstrap.

Table A8
Cross-Group Caller Rates

Call from: \ Call to:	Info Plus Assistance (1)	Info Only (Standard) (2)	Info Only (No Postcard) (3)	Info Only (Marketing) (4)	Info Only (Framing) (5)	Observations (N) (6)
Control	0.395	0.019	0.000	0.000	0.009	10630
Info Plus Assistance (Standard)	29.767	0.013	0.013	0.000	0.050	7972
Info Plus Assistance (Marketing)	30.335	0.000	0.000	0.000	0.000	2657
Info Only (Standard)	0.414	27.813	0.000	0.038	0.000	2657
Info Only (No Postcard)	0.376	0.000	21.181	0.150	0.075	2658
Info Only (Marketing)	0.414	0.000	0.075	25.555	0.000	2657
Info Only (Framing)	0.489	0.000	0.038	0.188	29.996	2657

Notes: Table reports the percent of the study population in each arm who calls into the phone line for each arm. An individual will be counted multiple times if she calls into more than one phone line; however in practice less than 1 percent of callers who call the number they are supposed to call also call another group's number.

Table A9
Rejection reasons

	Control	Information Only	Information Plus Assistance	P Value of Difference (Column 2 vs 3)
	(1)	(2)	(3)	(4)
Insufficient Interest	0.511	0.433	0.680	
		[0.121]	[0.000]	[0.000]
Ineligible After Review	0.389	0.486	0.232	
		[0.054]	[0.000]	[0.000]
Other Reasons	0.100	0.082	0.088	
		[0.529]	[0.630]	[0.791]
Observations (N)	190	208	650	

Notes: Table reports the percent of rejected applicants rejected for different reasons. Columns 1 through 3 shows share of rejected applicants that were rejected for a given group of reasons, 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 same two equally-sized pooled sub-treatments). Column 3 shows the Information Plus Assistance arms (weighted so that the two sub-treatments received equal weight). Column 4 reports the p-value of the difference between the Information Plus Assistance and Information Only treatment arms. We grouped rejections by reason given. “Insufficient interest” includes "failure to furnish required information", "failure to sign required forms", "failure to supply identification proof", "voluntary withdrawal", and "failure to keep appointments". “Ineligibility after review” includes "failing income, resources, or public assistance tests", "failure to meet citizenship or residence requirements", "categorical ineligibility", "failure to meet employment tests", "failure to meet household composition requirements", and "institutionalization or imprisonment". There other reasons such as "duplicate application", "application entered in error" that we cannot categorize into the preceding groups are reported as “other reasons”.

Table A10
Age and Health Characteristics of Applicants and Enrollees: Additional Detail

	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 - Individual (Annual) Health Care Measures, 2015								
Number of Hospital Days	2.24	1.39	1.21		2.64	1.48	1.19	
		[0.099]	[0.028]	[0.576]		[0.075]	[0.015]	[0.448]
Number of ER Visits	0.75	0.57	0.41		0.84	0.64	0.41	
		[0.308]	[0.034]	[0.037]		[0.388]	[0.042]	[0.025]
Number of Doctor Visits	8.88	7.40	6.39		9.75	7.42	6.38	
		[0.102]	[0.002]	[0.067]		[0.045]	[0.001]	[0.125]
Number of SNF Days	1.47	2.30	1.91		1.56	1.36	1.94	
		[0.357]	[0.517]	[0.647]		[0.824]	[0.646]	[0.472]
Panel B - Demographics								
Age (as of October 31, 2015)	66.07	67.32	67.91		65.94	67.06	68.03	
		[0.001]	[0.000]	[0.095]		[0.011]	[0.000]	[0.022]
Observations (N)	817	781	2,519		613	559	1,861	

Notes: Table reports additional characteristics that are shown in different form in Table V. Columns 1 - 3, and 5 - 7 show means by intervention arm with the p-value (relative to the control arm) in [square brackets] for the study population, 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. Columns 2 and 6 show the Information Only arm (for the same two equally-sized pooled sub-treatments). 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.

Table A11
Demographic and Health Characteristics of Enrollees

	Enrollees						
	Always Takers	Compliers		P Value of Difference (Column 2 vs 3)	Never Takers		P Value of Difference between Compliers and Never Takers (pooling 2 & 3 vs pooling 5 & 6)
		Info Only	Info Plus Assistance		Info Only	Info Plus Assistance	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A - Predicted Benefits							
Predicted Benefits	138.65	84.10	87.21	[0.788]	111.68	115.00	[0.000]
		[0.001]	[0.000]				
Panel B - (Annual) Health Care Measures, 2015							
Total Health Care Spending ($) [a]	10,238	8,676	7,809	[0.767]	11,750	12,968	[0.001]
		[0.675]	[0.213]				
Total Number of Visits and Days	14.79	6.19	7.56	[0.632]	12.04	13.45	[0.001]
		[0.049]	[0.004]				
Weighted Total Number of Visits and Days	5,407	716	1,504	[0.604]	4,214	5,181	[0.000]
		[0.054]	[0.004]				
Number of Chronic Conditions	6.54	4.07	4.80	[0.381]	5.33	5.45	[0.044]
		[0.020]	[0.006]				
Panel C - Demographics							
Share Age above Median (=65)	0.39	0.46	0.49	[0.569]	0.52	0.51	[0.214]
		[0.299]	[0.007]				
Share Age 80+	0.07	0.17	0.18	[0.815]	0.17	0.17	[0.615]
		[0.005]	[0.000]				
Male	0.39	0.44	0.37	[0.155]	0.38	0.38	[0.669]
		[0.443]	[0.436]				
Share White [b]	0.71	0.87	0.82	[0.213]	0.75	0.74	[0.000]
		[0.003]	[0.000]				
Share Black [b]	0.11	0.02	0.10	[0.003]	0.07	0.08	[0.823]
		[0.011]	[0.845]				
Share Primary Language not English	0.06	0.03	0.01	[0.482]	0.04	0.04	[0.008]
		[0.232]	[0.002]				
Share Living in Pittsburgh	0.05	0.08	0.09	[0.696]	0.06	0.05	[0.016]
		[0.366]	[0.029]				
Share Last Medicaid Spell Starting before 2011	0.26	0.42	0.34	[0.087]	0.33	0.34	[0.308]
		[0.006]	[0.024]				
Share of Individuals in Household Size of 1	0.66	0.78	0.81	[0.581]	0.08	0.07	[0.000]
		[0.035]	[0.000]				
Share of Sub-Population	0.058	0.048	0.119		0.89	0.82	

Notes: Sample is individuals who enrolled in SNAP within 9 months of their initial mailing. Column 1 shows the mean of the always takers (individuals who enroll regardless of intervention), while columns 2 and 3 show the means for compliers (individuals who enroll if and only if they receive the intervention) for each intervention; p-value (relative to the always takers) is in [square brackets]. Column 4 reports the p-value of the difference between the compliers in the two intervention arms. Columns 5 and 6 show the means for never takers (individuals who never enroll regardless of intervention) for each intervention. Column 7 reports the p-value of the difference between the compliers and never takers (pooling two intervention arms together). In columns 2 and 5 the two equally-sized sub-treatments are pooled; in columns 3 and 6 the two pooled sub-treatments are weighted so that they receive equal weight. Standard errors and p-values are computed with 10,000 replications of the bootstrap. Variables reported are the same as in Table V.
[a]Total spending is truncated at twice 99.5th percentile of study population, which is 371,620 (99.5th percentile in study population is 185,810). Amounts greater than the threshold are set to missing.
[b]Omitted category is other or missing race.

Table A12
Demographic and Health Characteristics of Applicants

	Applicants						
	Always Takers	Compliers		P Value of Difference (Column 2 vs 3)	Never Takers		P Value of Difference between Compliers and Never Takers (pooling 2 & 3 vs pooling 5 & 6)
		Info Only	Info Plus Assistance		Info Only	Info Plus Assistance	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A - Predicted Benefits							
Predicted Benefits	148.26	100.85	99.65	[0.910]	109.56	112.35	[0.028]
		[0.000]	[0.000]				
Panel B - (Annual) Health Care Measures, 2015							
Total Health Care Spending ($) [a]	9,424	7,707	7,813	[0.937]	12,019	13,403	[0.000]
		[0.533]	[0.314]				
Total Number of Visits and Days	13.33	9.84	8.29	[0.576]	11.96	13.73	[0.004]
		[0.331]	[0.011]				
Weighted Total Number of Visits and Days	4,661	1,752	1,938	[0.857]	4,261	5,362	[0.000]
		[0.117]	[0.011]				
Number of Chronic Conditions	6.21	4.83	4.83	[0.999]	5.31	5.49	[0.079]
		[0.094]	[0.006]				
Panel C - Demographics							
Share Age above Median (=65)	0.41	0.51	0.49	[0.680]	0.52	0.51	[0.277]
		[0.074]	[0.016]				
Share Age 80+	0.06	0.16	0.18	[0.646]	0.18	0.17	[0.811]
		[0.000]	[0.000]				
Male	0.41	0.40	0.37	[0.394]	0.38	0.38	[0.891]
		[0.994]	[0.228]				
Share White [b]	0.67	0.80	0.78	[0.540]	0.76	0.75	[0.120]
		[0.004]	[0.000]				
Share Black [b]	0.10	0.05	0.11	[0.011]	0.07	0.07	[0.058]
		[0.101]	[0.587]				
Share Primary Language not English	0.08	0.04	0.02	[0.232]	0.04	0.04	[0.145]
		[0.133]	[0.000]				
Share Living in Pittsburgh	0.05	0.07	0.08	[0.796]	0.06	0.05	[0.025]
		[0.389]	[0.068]				
Share Last Medicaid Spell Starting before 2011	0.25	0.35	0.31	[0.269]	0.34	0.35	[0.291]
		[0.018]	[0.014]				
Share of Individuals in Household Size of 1	0.53	0.62	0.63	[0.748]	0.08	0.07	[0.000]
		[0.067]	[0.000]				
Share of Sub-Population	0.077	0.070	0.161		0.853	0.762	

Notes: Sample is individuals who applied for SNAP within 9 months of their initial mailing. Column 1 shows the mean of the always takers (individuals who apply regardless of intervention), while columns 2 and 3 show the means for compliers (individuals who apply if and only if they receive the intervention) for each intervention; p-value (relative to the always takers) is in [square brackets]. Column 4 reports the p-value of the difference between the compliers in the two intervention arms. Columns 5 and 6 show the means for never takers (individuals who never apply regardless of intervention) for each intervention. Column 7 reports the p-value of the difference between the compliers and never takers (pooling two intervention arms together). In columns 2 and 5 the two equally-sized sub-treatments are pooled; in columns 3 and 6 the two pooled sub-treatments are weighted so that they receive equal weight. Standard errors and p-values are computed with 10,000 replications of the bootstrap. Variables reported are the same as in Table V.
[a]Total spending is truncated at twice 99.5th percentile of study population, which is 371,620 (99.5th percentile in study population is 185,810). Amounts greater than the threshold are set to missing.
[b]Omitted category is other or missing race.

Table A13
Caller Demographic and Health Characteristics: By Treatment Arm

	Information Only	Information Plus Assistance	P Value of Difference
	(1)	(2)	(3)
Panel A - Predicted Benefits			
Predicted Benefits	104.99	108.76	[0.286]
Predicted Enrollment	0.05	0.06	[0.330]
Panel B - (Annual) Health Care Measures, 2015			
Total Health Care Spending ($) [a]	6779	7792	[0.074]
Total Number of Visits and Days	10.16	8.96	[0.194]
Weighted Total Number of Visits and Days	3167	2575	[0.265]
Number of Chronic Conditions	5.16	5.15	[0.982]
Panel C - Demographics			
Share Age 80+	0.16	0.16	[0.895]
Male	0.38	0.37	[0.561]
Share White [b]	0.79	0.76	[0.044]
Share Black [b]	0.08	0.09	[0.189]
Share Primary Language not English	0.03	0.03	[0.389]
Share Living in Pittsburgh	0.06	0.06	[0.654]
Share Last Medicaid Spell Starting before 2011	0.34	0.31	[0.076]
Observations (N)	1,418	3,179	

Notes: Table shows the demographic and health characteristics of caller in each intervention arm (based on the unadjusted caller measure shown in Table II). The Information Only arm pools the two equally-sized sub-treatments; the Information Plus Assistance pools the two sub-treatments and weights them so that they receive equal weight. All p-values are based on heteroskedasticity-robust standard errors. All demographic and health characteristics are the same as shown in Table V.

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

[b] Omitted category is other or missing race.

Table A14
Demographic and Health Characteristics by Sub-Treatment: Applicants

	Control	Information Only				Information Plus Assistance		P Value of Difference between			
		Standard	No-Postcard	Marketing	Framing	Standard	Marketing	Control vs Treatment (col 1 vs 2+4+6+7)	Standard vs Marketing (col 2+6 vs 4+7)	Information Only Standard vs Framing (col 2 vs 5)	Information Only Standard vs No Follow-up Postcard (col 2 vs 3)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Panel A - Predicted Benefits											
Predicted Benefits	148.26	128.66	133.83	122.46	136.87	116.61	114.13				
		[0.009]	[0.078]	[0.000]	[0.118]	[0.000]	[0.000]	[0.000]	[0.369]	[0.328]	[0.573]
Panel B - (Annual) Health Care Measures, 2015											
Total Health Care Spending ($)[b]	9,424	10,848	7,155	6,238	5,797	8,333	8,335				
		[0.405]	[0.124]	[0.012]	[0.004]	[0.312]	[0.375]	[0.341]	[0.044]	[0.002]	[0.044]
Total Number of Visits and Days	13.33	12.78	9.12	10.49	10.01	9.18	10.65				
		[0.793]	[0.023]	[0.149]	[0.073]	[0.003]	[0.112]	[0.054]	[0.994]	[0.189]	[0.081]
Weighted Total Number of Visits and Days	4,661	3,735	2,513	2,785	3,634	2,607	3,026				
		[0.381]	[0.037]	[0.070]	[0.367]	[0.012]	[0.068]	[0.036]	[0.836]	[0.929]	[0.233]
Number of Chronic Conditions	6.21	5.93	5.45	5.15	4.68	5.15	5.40				
		[0.562]	[0.130]	[0.024]	[0.000]	[0.002]	[0.054]	[0.011]	[0.612]	[0.012]	[0.388]
Panel C - Demographics											
Share Age 80+	0.06	0.10	0.12	0.13	0.11	0.13	0.14				
		[0.050]	[0.006]	[0.001]	[0.008]	[0.000]	[0.000]	[0.000]	[0.174]	[0.541]	[0.359]
Male	0.41	0.40	0.37	0.41	0.39	0.37	0.39				
		[0.903]	[0.299]	[0.928]	[0.628]	[0.070]	[0.642]	[0.436]	[0.364]	[0.757]	[0.417]
Share White[a]	0.67	0.73	0.70	0.73	0.66	0.74	0.74				
		[0.025]	[0.263]	[0.020]	[0.758]	[0.000]	[0.001]	[0.000]	[0.866]	[0.027]	[0.407]
Share Black[a]	0.10	0.09	0.07	0.07	0.12	0.11	0.11				
		[0.464]	[0.055]	[0.040]	[0.312]	[0.855]	[0.469]	[0.710]	[0.883]	[0.131]	[0.296]
Share Primary Language not English	0.08	0.06	0.06	0.07	0.06	0.04	0.03				
		[0.087]	[0.213]	[0.457]	[0.058]	[0.000]	[0.000]	[0.001]	[0.905]	[0.892]	[0.773]
Share Living in Pittsburgh	0.05	0.08	0.06	0.04	0.05	0.07	0.06				
		[0.093]	[0.603]	[0.612]	[0.920]	[0.022]	[0.294]	[0.105]	[0.062]	[0.156]	[0.340]
Share Last Medicaid Spell Starting before 2011	0.25	0.31	0.30	0.28	0.29	0.30	0.28				
		[0.020]	[0.095]	[0.181]	[0.093]	[0.005]	[0.127]	[0.008]	[0.254]	[0.552]	[0.667]
Observations (N)	817	401	320	380	417	1,883	636				

Notes: Table shows demographic and health characteristics of applicants (as shown in Table V) separately for each sub-treatment. In column 8, sub-treatments are weighted so that within the Information Plus Assistance arm the standard and marketing sub-treatments receive equal weight, and the Information Plus Assistance treatments receive equal weight as the Information Only treatment. In column 9, sub-treatments are weighted so that Information Plus Assistance and Information Only are equally weighted in Standard and Marketing arms. All p-values are based on heteroskedasticity-robust standard errors. All demographic and health characteristics are the same as shown in Table V.

[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 A15
Demographic and Health Characteristics by Sub-Treatment: Enrollees

	Control	Information Only				Information Plus Assistance		P Value of Difference between			
		Standard	No-Postcard	Marketing	Framing	Standard	Marketing	Control vs Treatment (col 1 vs 2+4+6+7)	Standard vs Marketing (col 2+6 vs 4+7)	Information Only Standard vs Framing (col 2 vs 5)	Information Only Standard vs No Follow-up Postcard (col 2 vs 3)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Panel A - Predicted Benefits											
Predicted Benefits	138.65	112.99	126.04	115.17	130.07	106.57	101.56				
		[0.001]	[0.166]	[0.007]	[0.302]	[0.000]	[0.000]	[0.000]	[0.581]	[0.067]	[0.193]
Panel B - (Annual) Health Care Measures, 2015											
Total Health Care Spending ($) [b]	10,238	11,938	7,391	6,785	5,394	8,058	9,131				
		[0.429]	[0.113]	[0.037]	[0.002]	[0.095]	[0.468]	[0.317]	[0.246]	[0.002]	[0.045]
Total Number of Visits and Days	14.79	12.48	10.08	9.10	9.52	8.84	10.96				
		[0.352]	[0.047]	[0.009]	[0.016]	[0.001]	[0.070]	[0.012]	[0.977]	[0.193]	[0.329]
Weighted Total Number of Visits and Days	5,407	4,122	2,936	2,335	3,821	2,687	2,868				
		[0.345]	[0.064]	[0.010]	[0.275]	[0.010]	[0.023]	[0.017]	[0.356]	[0.834]	[0.367]
Number of Chronic Conditions	6.54	5.60	5.78	5.23	4.55	5.13	5.60				
		[0.089]	[0.205]	[0.025]	[0.000]	[0.001]	[0.063]	[0.004]	[0.676]	[0.061]	[0.791]
Panel C - Demographics											
Share Age 80+	0.07	0.10	0.13	0.13	0.11	0.15	0.14				
		[0.083]	[0.006]	[0.008]	[0.076]	[0.000]	[0.000]	[0.000]	[0.585]	[0.966]	[0.276]
Male	0.39	0.41	0.35	0.42	0.43	0.37	0.38				
		[0.640]	[0.247]	[0.437]	[0.331]	[0.239]	[0.771]	[0.889]	[0.510]	[0.662]	[0.163]
Share White [a]	0.71	0.79	0.73	0.77	0.69	0.77	0.79				
		[0.007]	[0.504]	[0.038]	[0.688]	[0.005]	[0.001]	[0.000]	[0.638]	[0.009]	[0.118]
Share Black [a]	0.11	0.07	0.07	0.06	0.12	0.10	0.11				
		[0.086]	[0.063]	[0.009]	[0.735]	[0.695]	[0.978]	[0.216]	[0.922]	[0.085]	[0.842]
Share Primary Language not English	0.06	0.04	0.05	0.05	0.05	0.03	0.02				
		[0.232]	[0.442]	[0.464]	[0.357]	[0.010]	[0.001]	[0.012]	[0.545]	[0.823]	[0.769]
Share Living in Pittsburgh	0.05	0.08	0.05	0.03	0.06	0.08	0.07				
		[0.056]	[0.806]	[0.312]	[0.356]	[0.007]	[0.205]	[0.059]	[0.025]	[0.366]	[0.153]
Share Last Medicaid Spell Starting before 2011	0.26	0.34	0.31	0.33	0.32	0.32	0.30				
		[0.029]	[0.225]	[0.044]	[0.116]	[0.007]	[0.166]	[0.007]	[0.506]	[0.598]	[0.465]
Observations (N)	613	298	245	261	295	1,385	476				

Notes: Table shows demographic and health characteristics of enrollees (as shown in Table V) separately for each sub-treatment. In column 8, sub-treatments are weighted so that within the Information Plus Assistance arm the standard and marketing sub-treatments receive equal weight, and the Information Plus Assistance treatments receive equal weight as the Information Only treatment. In column 9, sub-treatments are weighted so that Information Plus Assistance and Information Only are equally weighted in Standard and Marketing arms. All p-values are based on heteroskedasticity-robust standard errors. All demographic and health characteristics are the same as shown in Table V.

[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 A16
Behavioral Responses to Interventions: Robustness to covariates

	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	
		[0.041]	[0.016]	[0.796]
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]	[0.133]
SNAP Applicants among Non-Callers	0.077	0.086	0.081	
		[0.058]	[0.256]	[0.397]
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	
		[0.380]	[0.592]	[0.679]
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: Table shows robustness of our main estimates of behavioral responses (see Table II) to controlling for indicator variables for which of the 11 mail batches the individual was assigned to and for the baseline covariates shown in Table V. As in Table II, 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 same two equally-sized pooled sub-treatments). Column 3 shows the Information Plus Assistance arm (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 A17
Health Characteristics of Enrollees and Applicants: Robustness to restriction to full year of Medicaid

	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)
(Annual) Health Care Measures, 2015								
Total Health Care Spending ($) [a]	10,304	9,313	9,366		10,932	10,585	9,684	
		[0.532]	[0.469]	[0.966]		[0.862]	[0.427]	[0.575]
Total Number of Visits and Days	11.06	10.41	10.05		11.74	10.24	10.14	
		[0.682]	[0.451]	[0.791]		[0.421]	[0.324]	[0.951]
Weighted Total Number of Visits and Days	3,673	2,506	2,966		4,182	2,771	2,909	
		[0.215]	[0.413]	[0.480]		[0.250]	[0.243]	[0.867]
Number of Chronic Conditions	6.54	5.90	5.78		6.88	5.61	5.80	
		[0.182]	[0.074]	[0.758]		[0.029]	[0.039]	[0.669]
Observations (N)	565	562	1,836		425	410	1,396	

Notes: Table shows robustness of our main estimates of the health characteristics of applicants and enrollees (see Table V) to restricting to the approximately three-quarters of the sample who is enrolled in Medicaid for all of 2015. As in Table V, 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 arm (for the same two equally-sized pooled sub-treatments). Columns 3 and 7 show the Information Plus Assistance arm (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.

Table A18
Benefit Outreach Specialist (BOS) Characteristics

	Mean (SD)	Min	25 Percentile	Median	75 Perentile	95 Percentile	Max
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Age	28.82 (6.919)	21	24	27	31	42	57
Experience (in Month)	8.57 (9.893)	0	2	4	15	22	64
Gender (Share of Male)	0.39 (0.492)	0	0	0	1	1	1
Number of Calls Received as the First Call of Some Caller	48.17 (38.031)	1	12	49	76	107	137
Observations (N)				66			

Notes: Table summarizes characteristics of BDT employees receiving calls; they are known as Benefits Outreach Specialists, or BOS. It is limited to BOS who received calls as the first call of some caller in our study population.

Table A19
Relationship between Benefit Outreach Specialist (BOS) Characteristics and Applications or Enrollment

	Applications	Enrollment
	(1)	(2)
Mean in Callers	0.5974	0.4461
Effects of BOS characteristics		
BOS Age	0.0004	-0.0016
	[0.756]	[0.174]
BOS Age above Median (=27)	0.0022	-0.0281
	[0.902]	[0.116]
BOS Experience (in Month)	0.0012	0.0001
	[0.386]	[0.955]
BOS Experience above Median (=4)	0.0313	0.0158
	[0.075]	[0.374]
BOS Gender (Male)	0.0026	0.0062
	[0.885]	[0.731]
BOS Male × Caller Male	0.0369	0.0521
	[0.316]	[0.162]
Observations (N)	3,179	3,179

Notes: Table shows coefficients and p-values in [square-brackets] from regressing the outcome indicator shown in the column heading on the characteristic of the BDT employee (Benefits Outreach Specialist, or BOS) receiving the first call within 9 months of the initial mailing for the each caller in the Information Plus Assistance arm. Each row shows results from a different regression. In the last row, the regression includes separate indicator variables for whether the BOS is male, whether the caller is male, and the interaction of the two; it is the interaction effect that is shown.