The Effects of Information and Application Assistance on Take-up, Targeting, and Welfare: Experimental Evidence from SNAP

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November 2017

Abstract

This paper develops a framework for evaluating the welfare impact of various interventions designed to increase take-up of social safety net programs in the presence of potential behavioral biases. We then calibrate 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 or provided with this information and also offered assistance in applying; a "status quo" control group receives no contact. Only 6 percent of the control group enrolls in SNAP over the next 9 months, compared to 11 percent of the Information Only group and 18 percent of the Information Plus Assistance group. The individuals who apply or enroll in response to either intervention receive lower benefits and are less sick than the average enrollee in the control group. Despite the poor targeting properties of the interventions, our rough calculations suggest that they are nonetheless a cost-effective way to redistribute to low-income individuals relative to other safety net programs.

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, Manasi Deshpande, Colin Gray, Nathan Hendren, Henrik Kleven, Kory Kroft, Ben Olken, and Jesse Shapiro for helpful comments. We gratefully acknowledge financial support from the Alfred P. Sloan Foundation.

1 Introduction

Enrollment in U.S. social safety net programs is not automatic: individuals must apply and demonstrate eligibility. Often, eligibility rules are complicated, application forms long, and documentation requirements substantial. Perhaps as a result, incomplete take-up is a pervasive feature of many social safety programs, For example, Currie (2006) documents take-up rates ranging from a low of 10 to 20 percent for the State Children's Health Insurance Program in the late 1990s to 82 to 87 percent for the Earned Income Tax Credit (EITC) and 60 to 90 percent for cash welfare (TANF). Two main explanations are typically offered for incomplete take-up: lack of knowledge about eligibility and transaction costs associated with enrollment.[1]

There are frequent public policy proposals to try to increase awareness of eligibility and to simplify application processes in order to increase take-up among eligible individuals. For example, in the Supplemental Assistance Nutrition Program (SNAP) context, 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 for SNAP (Aaronson, 2011), and Congress provided funding to study various models for facilitating access to SNAP among the elderly (Kauff et al., 2014).

However, incomplete take-up may represent a constrained social optimum for at least two reasons. First, for many programs, an application is necessary to assess eligibility: the relevant criteria are not measurable in passively-collected administrative data. The SNAP program is one such example, and Social Security Disability Insurance (SSDI), Supplemental Security Income (SSI), and Medicaid are other examples. As a result, eligibility cannot be assessed without the government incurring administrative costs; these represent a negative externality which can drive a wedge between privately optimal and socially optimal application decisions (e.g. Kleven and Kopczuk 2011).

Second, neoclassical theory has emphasized that informational or transactional barriers to enrollment may serve as useful screens, allowing a given amount of public spending to be directed to individuals with higher marginal utility from enrollment (or higher social welfare weights); these theories suggest a trade-off between productive efficiency and allocative efficiency (e.g Nichols et al., 1971, Nichols and Zeckhauser 1982, Besley and Coate 1992). However, recent work in behavioral economics has conjectured that these barriers may have exactly the opposite targeting effect, discouraging the applicants with the highest marginal utility of enrollment (e.g. Bertrand et al., 2004; Mani et al., 2013; Mullainathan and Shafir 2013). For example, in their book "Scarcity", Mullainathan and Shafir (2013) argue that poverty imposes a "bandwidth tax" that makes poor individuals more likely to fail to undertake a high net value activity, such as taking chronic disease medication, paying regular bills on time, or enrolling in a public benefit program for which one is eligible.

This paper formalizes an approach to analyzing the normative consequences of how information and transaction costs affect the number and type of people who apply and enroll in social safety

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

net programs, and applies it to a randomized evaluation of interventions aimed at elderly non-participants in the SNAP program. As the only benefit that is virtually universally available to low-income households, SNAP - also known as food stamps - is one of the most important social safety net programs in the United States. During the Great Recession, as many as one in seven individuals received SNAP (Ganong and Liebman, 2013). In 2015, public expenditures on SNAP were about \$70 billion, the same amount as the EITC and surpassing the \$60 billion spend on SSI and the \$30 billion spent on cash welfare (TANF).[2] Take-up of SNAP is disproportionately low among the elderly, who are the focus of our study; in 2012, only 42 percent of eligible elderly enrolled in SNAP, compared to 83 percent overall (Eslami 2014). The stakes associated with non-participation are not trivial for eligible households; average annual benefits are about \$1,500, or about 15 percent of household income in a population where over two-thirds of households are below the federal poverty line (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. BDT in turn has partnered with the Pennsylvania state government to receive administrative data on applications and enrollment in a variety of public benefits, including SNAP and Medicaid. In particular, BDT uses Medicaid enrollment among elderly individuals (ages 60 and over) as a marker of likely eligibility for SNAP, and links this information to SNAP enrollment data to identify individuals likely eligible for but not enrolled in SNAP. These individuals form the basis of our study population.

We selected about 30,000 elderly individuals from this poulation in late 2015 and randomized them into three equally sized groups: an Information Only treatment, an Information Plus Assistance treatment, and a status quo control group. The treatments 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 the average monthly benefit, and providing them a phone number to contact the Department of Human Services to call to apply. Study participants in the Information Plus Assistance arm received a virtually identical letter and reminder postcard, with one key change: the Secretary of DHS instead provided them 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 ask a series of questions that allow them to inform the caller of their eligibility and likely benefit amount; the employee then use this information to fill out the application, assist the applicant in collecting necessary verification documents, submit the application, and assist with any follow up questions that arise 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

[2] US Department of Agriculture 2016, US Department of Health and Human Services 2016, US Internal Revenue Service 2015, US Social Security Administration (2016)

study participants to both BDT and DHS, and received administrative data from DHS on SNAP applications, enrollment and benefit amounts after the intervention; additional demographic and health data pre-intervention is provided from the study participants' Medicaid records.

The experiment itself yields two main empirical findings. First, information alone increases enrollment, while information combined with assistance increases enrollment even more, but at a higher cost per enrollee. Nine months after the intervention - at which point the initial impact appears to be fully in place - enrollment is 6 percentage points in the control arm compared to 11 percentage points in the Information Only arm and 18 percentage points in the Information Plus Assistance arm; these enrollment rates are all statistically distinguishable ($p<0.001$). Each intervention increases applications proportionally to its effect on enrollment; the success rate of applications is about 75 percent in each arm. 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 suggest that the larger application and enrollment effects of Information plus Assistance relative to Information Only are due to the assistance per se, rather than the anticipation of assistance. We also find that a sub-treatment which omits the reminder postcard from the Information Only intervention 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. 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.

The second main empirical finding from the experiment is that the interventions decrease targeting. Seemingly contrary to the "behavioral" hypothesis that information barriers and transaction costs deter the neediest eligible individuals, 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 from the progressive benefit formula (if they enroll), are less sick, and are more likely to be white and more likely to have English as their primary language. The characteristics of marginal enrollees and applicants are indistinguishable between the two intervention arms.

We develop a simple model that allows us to assess the normative implications of interventions that inform individuals about their likely eligibility ("information interventions") or reduce the private costs of applying ("assistance interventions"). Beyond our particular study, such interventions are common in enacted and proposed policies (as described above), as well as in previous academic studies (e.g. Alatas et al. 2016, Bettinger at al. 2012, Bhargava and Manoli 2015), although their welfare properties have not been much-examined. In particular, analyses of targeting have focused primarily on observable characteristics of affected individuals that are related to poverty but, as we show, are not necessarily related to the welfare properties of targeting.

We assume a fiscal externality on the government from the public costs of processing each application; this creates the standard wedge between privately and socially optimal application choices. In addition, we allow for the possibility that individuals may not make privately optimal application decisions; specifically, we assume they may misperceive the expected benefits from applying. Without such misperceptions, interventions that increase applications have no first-order effect on marginal enrollees' private welfare (by the envelope theorem); the information intervention will strictly decrease social welfare due to the fiscal externality, while the assistance intervention's effect on social welfare is *a priori* ambiguous. Moreover, in this neoclassical benchmark with no misperceptions, the targeting properties of the interventions are irrelevant for private welfare; and contrary to the "folk wisdom" that targeting poorer individuals is desirable, interventions that improve targeting to poorer individuals actually *decrease* social welfare if benefits are progressive. If, however, individuals underestimate expected benefits from applying - which is presumably the premise of an information intervention - then marginal applicants who apply as a result of either intervention have positive private welfare gains, with these gains increasing in the size of the misperception; the sign of the social welfare impact remains an empirical question. Even when individuals underestimate expected benefits from applying, the targeting properties of the intervention have not general relationship to its social welfare impact. We show that a sufficient condition for interventions that improve targeting toward less well off individuals to be more likely to improve welfare is that underestimation of expected benefits is (weakly) greater for the targeted individuals.

We use the empirical findings from the experiment to calibrate the model. The evidence in our setting is consistent with the "behavioral" hypothesis that individuals under-estimate their expected benefits from applying, and that such under-estimation is greater for less well-off individuals. This suggests potential private and social welfare gains from each intervention, but it also suggests that our finding that the interventions target less needy individuals bodes poorly for their welfare benefits. Nonetheless, our rough quantitative calculation suggests that our interventions to increase SNAP take-up among the elderly are about as effective at transferring resources to low-income beneficiaries per dollar of public expenditure as the Earned Income Tax Credit, and likely more effective than public subsidies for low-income adults' health insurance.

Related literature

Our paper relates to two strands of literature: analysis of barriers to take-up and analysis of how barriers to talk-up affect the *characteristics* of applicants and enrollees. 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.[3]This literature has also focused primarily on the descriptive, with little normative analysis.

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 authors have

[3]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.

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 to not persist into the following year when the individuals would have to sign up again (Bhargava and Manoli 2015, Manoli and Turner 2014, Guyton et al. 2016). Quasi-experimental studies have also found that information is an important barrier to take up of SSDI (Armour, forthcoming) and post-secondary enrollment among unemployment insurance recipients (Barr and Turner, forthcoming). 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 forthcoming). 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.

There is reason to suspect that informational barriers are important on the context of SNAP enrollment. A survey of likely eligible, nonparticipants in SNAP concluded that lack of awareness of eligibility was a primary barrier to participation, estimating that about half of likely eligible, nonparticipating households reported that they were not aware of their eligibility (Bartlett et al. 2004). Daponte et al. (1999) conducted an early and innovative small randomized trial in 1993 in Pennsylvania designed to inform non-participating, eligible households about their eligibility in SNAP. Their findings suggest that the information treatment affected food stamp applications; however, issues of small sample size (32 households were in the treatment arm and responded to the follow-up survey) as well as loss-to-follow up make definitive conclusions difficult.

Reductions in transactional barriers have also 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 (2017) 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).

Direct information on the impact of assistance on SNAP enrollment is limited. To our knowledge, the only direct evidence comes from Schanzenbach (2009), who conducted a randomized experiment with different levels of assistance to likely SNAP-eligible individuals. The intervention was randomized across different offices of a low-income tax preparer. The preliminary results from one California county suggested that, among individuals 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).

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 analysis below, however, suggests that there is no general relationship between the targeting properties of the intervention and the impact of the intervention on either private or social welfare, and suggests additional conditions that need to be examined empirically in order for such targeting properties to yield normative implications.

To our knowledge, our study is the first to examine targeting in the context of an information intervention as well as an assistance intervention. In the context of informational studies, 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., forthcoming). Our findings, by contrast, suggest that information about eligibility disproportionately encourages enrollment among better-off applicants. Two recent studies have examined targeting in the context of changes in transactional costs of applying, tending to find that transaction costs improve targeting on some dimensions but may worsen it on others. Alatas et al. (2016) find that introducing transaction costs by requiring individuals to apply for a conditional cash transfer in Indonesia 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 application costs does not further affect the characteristics of enrollees. Deshpande and Li (2017) 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 improves 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 reduces 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 worsens targeting on all dimensions, and at all stages (applications and enrollees).

The rest of the paper proceeds as follows. Section 2 provides background information on the SNAP program and the application and enrollment process. Section 3 presents our theoretical framework. Section 4 describes the experimental design and our data sources. Section 5 presents the results from the experiment. Section 6 uses the empirical results to calibrate the model from Section 3 and present welfare analysis of the interventions. The last section concludes.

2 Setting and Background

2.1 SNAP Eligibility and Benefits

SNAP expenditures make it the second-largest means-tested program in the United States after Medicaid (US Congressional Budget Office 2013). It is a household-level benefit designed to ensure a minimum level of food consumption for low-income families (Hoynes and Schanzenbach 2016).

Our study focuses on elderly households – i.e. households with an individual aged 60 or over - in Pennsylvania (PA) in 2016.[4]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. 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.[5] 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)[6] 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 24 months, although there are exceptions that require earlier re-certification.[7]

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 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; there was no minimum 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. Average monthly benefits per SNAP recipient in elderly households in Pennsylvania was $136 in fiscal year 2013 (Center on Budget and Policy Priorities, 2015). In practice, as we will see in our data, there are distinct modes of benefit receipt at the minimum and maximum amount, which influenced our decision to model a discrete type space.

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

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

[6]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).

[7]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.

SNAP benefits are thus a substantial source of potential income for eligible houseolds. About two-thirds of elderly households in Pennsylvania receiving SNAP had household incomes below the federal poverty line; successful enrollment entitles the household to benefits for at least 24 months, boosting annual income by about 15 percent (Center on Budget and Policy Priorities, 2017).

2.2 Application and Enrollment Process

For an elderly household to successfully enroll in SNAP in PA during our study period, the household 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 (such as Social Security Number, name and address), information about each household member, information on resources and income, and information on various household expenses such as medical expenses, rent and utilities. They must provide documentation verifying their 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). The interview is used, among other things, to make sure the application is complete and if not to request additional documentation or request answers to additional questions. The state has 30 calendar days to process an application. However, households who – by virtue of extreme need – qualify for expedited review must have their application reviewed within 5 calendar days of application. Successful applicants access their benefits electronically, using plastic cards that can be used to buy food in authorized food stores.

Given the SNAP program rules, both the individual and state's determination of eligibility and benefit amounts requires the individual to actively apply with the required information. From the individual's perspective, there is uncertainty about the the benefit function, the inputs into it (e.g. various shelter and medical expenses that serve as deductions to income and affect benefits), and the potential for mistakes in the process (e.g. not showing up for your 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 eligibility 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), who 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.[8]

The application imposes costs on both the applicant and the state. 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).

In addition to the costs on the applicant, the application imposes costs on the government. They must process applications to determine eligibility, including verifying self-reported information in various available administrative data systems. Isaacs (2008) estimates that the annualized administrative costs of the SNAP program are about $178 per recipient. This corresponds to approximately $240 per applicant (given the 75% acceptance rate we estimate below) or about $0.16 per dollar of SNAP benefits issued. She estimates that certification costs (that is the initial determination of eligibility, calculation of benefit amounts and re-certification of households) account for three-fifths of these costs, suggesting that state administrative costs associated with applications are about nine percent of benefits (or slightly lower given that this number includes re-certification). She notes that the administrative costs of the SNAP program are substantially higher than those of the Earned Income Tax Credit - where there is concern about ineligible people enrolling; this highlights empirically the trade-off that Kleven and Kopczuk (2011) analyze theoretically between greater administrative costs and balancing false rejections and false acceptances.

3 Conceptual framework

This section develops a simple take-up model to analyze effects of interventions that provide information and/or application assistance. The model is useful for informing the welfare consequences of the impacts of the interventions, and also for providing sufficient conditions for when a finding that an intervention improves (or worsens) "targeting" translates into higher (or lower) welfare benefits from the intervention.

[8]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.

3.1 Set-up

The model is one in which both the applicant and the state are uncertain about eligibility and benefit amounts unless the individual actively applies with the required information, and in which the application - regardless of outcome - imposes costs on both the applicant and the government. These features correspond to the SNAP program we have just described, as well as other U.S. transfer programs (e.g., SSDI, SSI, and Medicaid).

Individuals decide whether to apply for benefits in a safety net program. For simplicity, we assume there are three possible outcomes from applying for the safety net program: (1) enrolling in the program and receiving a high level of benefits from program, b_H, (2) enrolling in the program and receiving a low level of benefits, $b_L < b_H$, and (3) being rejected after applying. We assume that there is ex-ante uncertainty about benefit eligibility both on the part of the potential applicant and on the part of the government. The government can determine eligibility and benefit amounts only through reviewing an application, which costs the government g for each application processed (whether it is rejected or not). Applications thus generate a fiscal externality on the government.

For the individuals, we assume for simplicity that there are two types ($j \in \{h, l\}$) in the population who are potentially affected by the treatment. Individuals of type j know their income y_j for sure. Note that we use lower case j to refer to individual types and upper case J to denote benefit amounts, so an individual of type j can either receive benefits b_H or b_L if his application is accepted.

We denote expected benefits, if the individual applies and is accepted, by $B_j = b(y_j)$. As discussed in the previous section, uncertainty about benefits can come from several sources: uncertainty about the inputs into the benefit function, uncertainty about the function $b()$ that maps inputs to benefits, and the potential for mistakes in the application process.

We parameterize the uncertainty about benefits for each type j as follows: type h individuals receive b_H with probability π_{hH} and b_L with probability π_{hL}, while type l individuals receive b_H with probability π_{lH} and receive b_L with probability π_{lL} (with $\pi_{hH} > \pi_{lH}$ and $\pi_{lL} > \pi_{hL}$ and $y_l > y_h$).[9] Thus, an application of a type j individual is accepted with probability $\pi_j = \pi_{jH} + \pi_{jL}$ and rejected with probability $1 - \pi_j$. Conditional on an application being accepted, a type j individual receives expected benefits $B_j = \frac{\pi_{jH} b_H + \pi_{jL} b_L}{\pi_j}$; expected benefits for type j from applying are therefore given by $\pi_j B_j$. We assume that $\pi_h > \pi_l$ so that type h individuals are both more likely to have application accepted and are more likely to receive high benefits (conditional on acceptance). Applying involves a private application cost c, which captures private costs such as the time and effort spent compiling documents, filling out forms, and participating in the interview. Within each type j, individuals vary in their private cost of applying c, which is distributed $f_j(c)$ for each type. Application also involves a public cost on the government, g,which captures their costs of processing the application to determine eligibility and benefit amounts.

Finally, we allow for some possibility of individual "mistakes" - such as misperceived expected

[9] Note that there could also be individuals who are likely to be ineligible, but we exclude them from model because the experiment targeted individuals who are likely to be eligible.

benefits from applying or inattention. For our baseline model of "mistakes", we assume that each type may misperceive their true probability of their application being accepted by ϵ_j, which changes the perceived expected benefit of applying for benefits for type j individuals by changing the perceived probability of acceptance from π_j to $(1+\epsilon_j)\pi_j$. While it is possible to have misperceptions in either direction, we focus on the case of under-estimation of the probability of acceptance ($\epsilon_j < 0$); this is both a natural motivation for an information intervention of the type we study, and - we show later - consistent with the empirical results. With $\epsilon_j < 0$, misperception reduces the perceived expected benefit of applying. For simplicity, we assume that the misperception is "proportional" so that it changes the perceived probability of acceptance but does not alter the expected benefits conditional on an application being accepted; we also assume that any misperceptions are the same across individuals within a type, although we discuss relaxations of this below. Finally, while our baseline model focuses on misperceived probabilities of acceptance as the departure from the neoclassical benchmark, in Appendix E we show that our main results are robust to an alternative departure based on inattention as well as an alternative model based on misperceptions in cost of applying (rather than in probability of application acceptance). We refer to the special case of no misperceptions ($\epsilon_j = 0$) for $j \in \{h, l\}$ as a "neoclassical" benchmark case.

Individual application decision and private welfare

In deciding whether to apply for benefits, we assume that individuals maximize their private utility, given their (possibly incorrect) perceptions of the expected benefits from applying. Individuals of type j therefore apply if the expected utility of applying is greater than the utility of not applying. For an individual with private application cost c this condition is defined as follows:

$$(1+\epsilon_j)\pi_j u(y_j + B_j - c) + (1 - (1+\epsilon_j)\pi_j)u(y_j - c) > u(y_j).$$

If we take a first-order Taylor approximation around $u(y_j)$, the decision to apply for the program simplifies to:

$$(1+\epsilon_j)\pi_j B_j > c. \tag{1}$$

Equation 1 is the key equation for the individual's application decision. Crucially, for our subsequent analyses, we allow for the possibility that the individual decision is not privately optimal. We model this by allowing for the possibility of mispercetion, specifically under-estimation of the probability of acceptance ($\epsilon < 0$). This type of informational friction seems natural given the information content of our interventions, but of course there are other frictions that could create a wedge between individual choices and privately optimal choices and serve the same purpose in the model. As noted, in Appendix E we consider models with other "behavioral" frictions - such as inattention or misperception of the cost of applying; non-behavioral frictions could also exist, such as (in the case of non-elderly applications) agency issues within the family between the parent (who bears the cost of applying) and the child (who receives some of the benefits).

We can use the private application decision rule in equation (1)and integrate across the distribution of private costs to get the total private welfare of type j individuals:

$$\begin{aligned} V_j &= E[u()|apply] + E[u()|\neg apply] \\ &= \int_0^{(1+\epsilon_j)\pi_j B_j} (\pi_j u(y_j + B_j - c) + (1-\pi_j)u(y_j - c)) f_j(c)dc + \int_{(1+\epsilon_j)\pi_j B_j}^{\infty} u(y_j) f_j(c)dc \\ &\approx u(y_j) + \int_0^{(1+\epsilon_j)\pi_j B_j} u'(y_j)(\pi_j B_j - c) f_j(c)dc \end{aligned}$$

where the last line again uses a first-order Taylor approximation around $u(y_j)$. Note in the above expression that the ϵ_j term affects the individual application decision but not realized utility, since the ϵ_j term only changes perceived probability of acceptance.

Social welfare

We consider a redistributive social welfare function, which seems natural given the redistributive purpose of the SNAP program. Specifically, we consider a utilitarian social welfare function, although we could easily accommodate alternative individualistic social welfare functions at the cost of additional notation for the social welfare weights on different types. Because the individual's private decision does not internalize the additional application cost g on the government, even a privately optimal decision may be socially sub-optimal. This fiscal externality g is the source, in our setting, of the classic tension between the private and social optimum. In other settings there may be alternative or additional fiscal externalities from individuals distorting their labor supply in the presence of means-tested benefits, which in turn reduces the tax revenue for the government.

We define a utilitarian social welfare function W, which is the sum of total (private) welfare of both types of individuals minus the public cost of processing applications and paying benefits:

$$W = \underbrace{V_l + V_h}_{\text{Private Welfare}} - \underbrace{[(\pi_l B_l + g)A_l - (\pi_h B_h + g)A_h]}_{\text{Public Costs (Benefits, Application Costs)}} . \tag{2}$$

where A_j is the expected number of applications from type j individuals, which is equal to $A_j = 1 - F_j((1+\epsilon_j)\pi_j B_j)$ based on take-up decision in equation (1). The social cost of an application by type j is $g + \pi_j B_j$ - the cost of processing the application (which is incurred regardless of success) plus the expected benefit payout $\pi_j B_j$.[10] For ease of exposition in what follows, we denote the expected number of enrollees receiving high benefits (b_H) as $E_H = \pi_{hH}A_h + \pi_{lH}A_l$, and the expected number of enrollees receiving low benefits (b_L) is $E_L = \pi_{hL}A_h + \pi_{lL}A_l$. Thus, the total

[10] We assume this cost is born by someone with the average marginal utility of consumption in society; implicitly, our W expression in equation 2 is thus a "money metric" social welfare expression, normalized by the average marginal utility of consumption in the population.

expected benefits (ignoring application costs) can be written as $b_H E_H + b_L E_L$, which is equal to $\pi_l B_l A_l + \pi_h B_h A_h$ but is easier to measure empirically since we do not necessarily directly observe types in our data, but we know the benefit levels in the program and we can observe number of individuals receiving different benefits levels.[11]

Interventions and their targeting properties

We now model two alternative interventions to motivate the design of our experiment. In the **Information Only treatment**, the treatment increases the perceived probability of the application being accepted ($d\epsilon$), which reduces misperceptions if individuals previously had underestimated the probability of acceptance (i.e., $\epsilon < 0$ initially). In the **Information Plus Assistance treatment**, both the perceived probability and the actual private cost of applying are affected ($d\epsilon$,$-dc$). We define "targeting" as the share of enrollees who are high-benefit enrollees (relative to low-benefit enrollees); i.e., $e = E_H/(E_H+E_L)$. We say that a treatment T **increases targeting** if $de/dT > 0$.[12]

3.2 Welfare Impacts of Interventions

We begin with an analysis of the private and social welfare effects of the two interventions defined above. The treatments affect application decisions of each type of individual, which in turn affects enrollments. For simplicity, we assume the interventions have zero marginal cost; this would be the case, for example, for interventions that reduce the complexity of the program in a way that increases the perceived probability of acceptance, or that simplify the application process to reduce application costs. When we use the model to quantify the normative implications of the experimental results in Section 6 below, we consider both costless interventions such as we model here as well as interventions with the actual costs of the ones we implemented.

The effects of each treatment on social welfare can be summarized by the following proposition:

Proposition 1. *Let $\mu_j \equiv -u'(y_j)\epsilon_j(\pi_j B_j)$, which equals the marginal utility of income times the expected benefit of applying times the misperception of the application acceptance probability. The effect of the Information Only treatment on welfare is given by the following expression:*

$$\frac{dW}{dT}^{Information\ Only} = \underbrace{\mu_l \frac{dA_l}{dT} + \mu_h \frac{dA_h}{dT}}_{Change\ in\ Private\ Welfare} - \underbrace{\left[(\pi_l B_l + g)\frac{dA_l}{dT} + (\pi_h B_h + g)\frac{dA_h}{dT}\right]}_{Change\ in\ Public\ Cost} \quad (3)$$

[11]Given these definitions, the total public costs can be re-written as $b_H E_H + b_L E_L + g(A_h + A_l)$, where $A_h + A_l$ represents total applications across all individuals.

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

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

$$\frac{dW}{dT}^{Information\ Plus\ Assistance} = \underbrace{\mu_l \frac{dA_l}{dT} + \mu_h \frac{dA_h}{dT} + u'(y_l)A_l + u'(y_h)A_h}_{Change\ in\ Private\ Welfare} - \underbrace{\left[(\pi_l B_l + g)\frac{dA_l}{dT} + (\pi_h B_h + g)\frac{dA_h}{dT}\right]}_{Change\ in\ Public\ Cost}. \quad (4)$$

Proof: *See Appendix E.*

Note that if individuals underestimate the probability of acceptance (which is the premise of the information interventions), then $\epsilon_j < 0$ and thus $\mu_j > 0$.

To develop intuition for these two expressions, consider first the specific case where individuals have accurate beliefs (i.e., $\epsilon_j = 0$). In this case, the Information Only treatment, which affects the perceived probability of acceptance ($d\epsilon$), has no effect on private welfare (since $\mu_j = 0$). But if it increases applications it increases public costs through both processing costs and expected benefit payouts. The intervention therefore unambiguously reduces social welfare if it increases applications. This is a stark result, but it can be intuitively understood as a consequence of the envelope theorem: individuals have accurate beliefs and are already optimizing, so the marginal applicants who apply as a result of the intervention were close to indifferent to applying before the intervention.[13] Moreover, since there are public costs of processing applications and paying benefits, any treatment which increases applications raises social costs and therefore reduces social welfare. Intuitively, individuals do not consider public costs when making their application decisions, so encouraging more applications for individuals who are (privately) indifferent to applying will reduce social welfare. Typically benefit payments would "net out" because they would show up both as a cost and a benefit; here they show up as a pure cost; since individual has to pay a private cost c to obtain these benefits, the marginal individual receives zero net (of private application costs) utility from enrolling.

For the Information Plus Assistance treatment - which affects both the perceived probability of acceptance ($d\epsilon$) and the private cost of applying ($-dc$) - the overall change in welfare is similar to the Information Only treatment but with two additional terms ($u'(y_l)A_l + u'(y_h)A_h$) that represent the additional change in private welfare from reducing costs for the infra-marginal applicants of

[13] While this result is obtained in a model where everyone has correct beliefs, in Appendix E we work through an extension where individuals have heterogeneous (but mean unbiased) beliefs, and we prove a similar proposition in this extended model. To do so we need several additional technical assumptions: (1) misperceptions are independent of private application costs; (2) private application costs are symmetrically distributed around the level of expected benefits; and (3) the heterogeneous misperceptions are symmetrically distributed around 0. Each of these assumptions is needed for the heterogeneous (but mean unbiased) misperceptions to "cancel out" in the expression for the change in welfare from Information Only treatment. We also show in the Appendix E that if beliefs are heterogeneous and mean unbiased and the treatment reduces the variance in beliefs that this will increase private welfare. The intuition for this result is that with heterogeneity in misperceptions, some individuals are over-estimating benefit of applying and some are under-estimating benefit of applying. In both cases, this leads to some marginal individuals making "mistakes" (relative to perfect information benchmark). Reducing variance in beliefs is thus equivalent to reducing magnitude of these mistakes.

both types. In this case, the intervention raises private welfare because it reduces infra-marginal applicants' application costs. However, the social welfare effect of the Information Plus Assistance treatment is ambiguous and depends on the increased public costs from encouraging more applicants relative to the increase in private welfare from reducing infra-marginal applicants' private costs of applying.

These results make sense in light of a model where individuals privately optimize with accurate beliefs, and there is a fiscal externality due to the public costs of processing applications. The standard solution to this problem would be to charge a Pigouvian "application tax" to each applicant equal to the public cost of processing. In the absence of such a policy instrument, both changing perceptions of probability of application acceptance as well as increasing "hassle costs" to applicants may several as alternative instruments for addressing the fiscal externality. Changing perceptions has no welfare effect on the infra-marginal applicants, so one solution is to have a policy that induces under-estimation of expected benefits such that the marginal applicant has an expected welfare gain from applying equal to the public application cost. This is very similar to the Pigouvian solution (in terms of the actual costs of the marginal applicant). By contrast, using hassles instead of "manipulating perceptions" imposes additional costs on all of the infra-marginal applicants. As a result, the second-best solution using "hassle costs" may not be as effective.

If we move away from the neoclassical benchmark and assume that individuals under-estimate the probability of acceptance before the intervention (i.e., $\epsilon_j < 0$), there are now two additional terms to consider in the welfare expressions ($\mu_j \frac{dA_j}{dT}$) for $j \in \{h, l\}$. The two additional terms represent the change in private welfare for the marginal applicants. Recall that $\mu_j \equiv -u'(y_j)\epsilon_j(\pi_j B_j)$; without misperceptions (i.e., $\epsilon_j = 0$), these terms were zero due to envelope theorem. However, if individuals under-estimate the probability of acceptance before the intervention, marginal applicants who apply experience a positive change in private welfare. The magnitude of the expected welfare change for marginal individuals is increasing in the magnitude of the under-estimation before the intervention.[14] Specifically, the marginal enrollee has a non-zero expected private welfare change equal to his marginal utility of income times the size of his under-estimation of the probability of acceptance times the benefits he is expected to receive conditional on applying. However, the social welfare impact of the intervention is ambiguous, given that any private benefits to marginal applicants (and to infra-marginal ones in the case of the Information Plus Assistance intervention) must still be balanced against the increased public costs from processing additional applications and paying benefits.

3.3 Relationship between targeting impacts and changes in welfare

As discussed in the Introduction, a theoretical literature has considered how various barriers to enrollment may potentially deter those with either higher or lower marginal utility from enrollment,

[14]This result is somewhat similar to the recent analysis of optimal Unemployment Insurance in the presence of biased beliefs in Spinnewijn (2015). In the standard optimal unemployment insurance model, unbiased beliefs and agent optimization imply that behavioral responses will only matter to the extent that they affect the government budget (Baily 1978; Chetty 2006).

and two recent empirical studies (Alatas et al. 2016 and Deshpande and Li 2017) have examined this by looking at how interventions that increase hassle costs affect the characteristics of applicants and enrollees. These studies - like ours - examined observable characteristics of the applicants and enrollees that are likely correlated with marginal utility of consumption - Alatas et al. (2016) directly measure consumption while Deshpande and Li (2017) analyze health and socioeconomic status. We will follow in this tradition, examining how our information and assistance interventions affect targeting, which we have defined as the share of enrollees who are high-benefit (i.e. low net resource and therefore presumably a higher marginal utility of consumption).

Such analyses rest on the intuition that an improvement in targeting along some observable dimension due to the intervention will, all else equal, increase the social welfare benefits from that intervention. We show here that this is not unconditionally true. We then derive sufficient conditions under which an improvement in targeting due to the intervention increases the intervention's impact on social welfare.

To see the intuition, consider the expressions in Proposition 1 for the impact of the intervention on social welfare, and assume that $\mu_l > 0$ but that $\mu_h = 0$. In this case, an intervention that increases applications and enrolled only high-benefit enrollees would unambiguously improve targeting but would have a negative effect on welfare (because there would be no change in private welfare and social welfare would be reduced due to the increased public costs of processing applications and paying benefits). Similarly, in this same set up, for μ_l "large enough", even an intervention that reduced targeting by only enrolling low-benefit enrollees could still improve welfare since these individuals could still experience change in their private welfare well in excess of the public costs. As a result, there is no general relationship between changes in targeting and changes in welfare. However, our model can be used to describe specific situations where changes in targeting and changes in social welfare go hand-in-hand. This is summarized in the following result:

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 either intervention (Information Only or Information Plus Assistance treatment) is giving by the following expression:*

$$\frac{\partial}{(de/dT)}\left(\frac{dW}{dT}\right)\Bigg|_{\frac{dA}{dT}} = [(\mu_h - \mu_l) - (\pi_h B_h - \pi_l B_l)]\,(E_H + E_L)\frac{(E_H + E_L)}{E_H(\pi_{lL} - \pi_{hL}) + E_L(\pi_{hH} - \pi_{lH})} \tag{5}$$

Proof: *See Appendix E.*

This result shows that the ceteris paribus (i.e. holding constant the change in applications) change in welfare from changes in targeting is increasing in $(\mu_h - \mu_l)$. The intuition for why μ_h enters positively but μ_l enters negatively is because in the proposition the change in total applications is being held constant. As a result, to increase targeting while holding the effect of interventions on applications constant, an application from a type l individual is essentially "swapped out" for an application from a type h individual. For the improvement in targeting holding applications constant to increase social welfare, it must be the case that $\mu_h - \mu_l$ - the welfare gain from "swapping an l individual's application for an h individual's application exceeds the increased public cost of that swap, $\pi_h B_h - \pi_l B_l$.[15]

Recall that $\mu_j \equiv -u'(y_j)\epsilon_j(\pi_j B_j)$. Therefore, in the neoclassical benchmark case ($\epsilon_j = 0$ and thus $\mu_j = 0$ for $j = \{h, l\}$), an improvement in targeting does nothing for private welfare and *reduces* social welfare. The lack of private welfare consequences from the intervention's targeting property follows directly from the envelope theorem: the enrollees who are marginal to the intervention have no change in private welfare and thus their type is irrelevant for private welfare.[16] The decrease in social welfare from an increase in targeting follows directly from the progressive benefit formula: increasing targeting brings in more high-benefit enrollees relative to low-benefit enrollees at higher fiscal cost but with no change in private 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. [17]

Once we move away from the neoclassical benchmark and allow for the possibility that individuals under-estimate the probability of application acceptance ($\epsilon_j < 0$), then μ_j is increasing in three type-specific factors: the marginal utility of consumption ($u'(y_j)$), the expected benefit level for each type of applicant ($\pi_j B_j$), and the magnitude of the underestimation ($-\epsilon_j$). The first two type-specific factors are higher for h types than l types. This presumably drives the standard intuition that the change in welfare due to the intervention would be higher with increased targeting on observables correlated with the marginal utility of consumption. (Moreover, with a non-utilitarian social welfare function that put higher social welfare weights on h, this would add yet another type-specific factor that was higher for h than l). However, these are not sufficient statistics because the size of under-estimation is also important in determining relative magnitude of μ_h and μ_l. Assuming that the h types have a higher marginal utility of consumption (or a higher social marginal utility of consumption), a sufficient condition for an increase in targeting holding constant the change in applications to increase social welfare is that under-estimation is non-zero for at least one type and weakly higher (in absolute value) for the h type (i.e., $\epsilon_h < \epsilon_l < 0$).[18]

Overall, Proposition 2 provides guidance for specific situations in which improved targeting properties of the intervention increase the likelihood the intervention is social welfare improving.

[15]Consider the simplifying case where each type has no uncertainty about benefit level or whether application is accepted (so that $\pi_{lH} = \pi_{hL} = 0$ and $\pi_{lL} = \pi_{hH} = 1$). In this case, the right-hand side of the formula above simplifies to the following: $\frac{\partial}{(de/dT)}\left(\frac{dW}{dT}\right)\Big|_{\frac{dA}{dT}} = [(\mu_h - \mu_l) - (b_H - b_L)](E_H + E_L)$.

[16]In Appendix E, we develop extension that considers "non-marginal" changes. We describe how even in this case there is no general relationship between changes in targeting and changes in welfare.

[17]The distinction between this result and the "standard intuition" from papers such as Nichols and Zeckhauser (1982) that ordeals can improve social welfare by disproprotionately screening out the high ability types is precisely that in their setting, enrollment by high ability types comes with a larger fiscal cost on the goverment (via the greater loss in tax revenue from their higher labor market earnings). In both settings, if the fiscal cost from enrollment were constant across individuals, the targeting property of the intervention would be irrelevant for the impact of the intervention on social welfare.

[18]This discussion implicitly assumes that $u'(y_j) > 1$ for $j \in \{H, L\}$, so that the marginal utility of consumption from $\pi_j B_j$ exceeds its financial cost. 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 than the average in the population, as would be expected from any means tested benefit.

In our empirical work below we will show that both the Information Only intervention and the Information Plus Assistance intervention worsen targeting. We will also present evidence consistent with a departure from the neoclassical benchmark ($\epsilon_j \neq 0$) and suggestive of $\epsilon_h < \epsilon_l < 0$. Proposition 2 tells us that under these conditions, our findings that the interventions decrease targeting suggest that they are less likely to increase social welfare.

However the proposition also highlight that there is no general relationship between targeting properties and the likelihood the interventions improve social welfare, and that therefore additional empirical estimates are required in order to draw normative inferences from targeting results. This is because the change in social welfare from the intervention depends on the "net" benefits of individuals affected by the treatment, not the "gross" benefits to individuals (i.e., the amount of the benefits times their marginal utility of consumption). If, for example, the l types underestimate the probability of acceptance by much more than the h types, the l types could have a higher net private welfare gain from the intervention and thus the welfare of the treatment could be larger the more l types brought in, even though they get lower benefits and have lower marginal utility of consumption. This is reminiscent of the distinction between gross and net benefits to schooling (Card 2001). In that setting, individuals differ both in their return to schooling and their cost of schooling. As a result, interventions which reduce the costs of schooling will attract students whose return (net of costs) was close to zero, but the "gross" returns to schooling for these individuals is not clear; it may or may not be similar to the average (gross) return to schooling in the population. Similarly, in our model without misperceptions, reduction in costs of applying will attract individuals with very low net benefit of applying, but their gross benefit could be large or small.

4 Empirical Design and Data

4.1 Design of Interventions

We partnered with Benefits Data Trust (BDT), a national not-for-profit organization based in Philadelphia that strives to be a "one-stop shop" for benefits access, screening individuals for benefit eligibility and providing application assistance. Since its inception in 2005, BDT has submitted over 500,000 benefit applications across multiple states, resulting in approximately \$5 billion in benefits delivered to low-income individuals and families. (Benefits Data Trust 2016). Through existing data share agreements with the Pennsylvania Department of Human Services and other state agencies, BDT receives application and enrollment data for a variety of public benefits, including SNAP and Medicaid. 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 supplied

BDT with administrative data that allowed them to identify likely eligible SNAP non-participants. Specifically, BDT received 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 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.[19]

Information Plus Assistance

BDT conducts a series of outreach services to inform these individuals of their 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 1 shows these standard outreach materials. We model this in Section 3as increasing the perceived probability of an application's acceptance ($d\epsilon$).

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 them questions so that BDT staff can populate an application and submit it on their behalf, advising them of what documents they need to submit and offering to review and submit documents on their behalf, and assisting with post-submission requests or questions regarding the application. BDT also tries to ensure that the individual receives the maximum benefit for which they are eligible

[19] Loosely speaking, our information treatment resembles the control arm in Schanzenbach (2009) - in which likely eligible individuals who expressed interest were handed a blank Food Stamps application with the address of the county agency where it could be submitted - while our Information Plus Assistance arm lies somewhere in between the lighter and heavier touch assistance provided in Schanzenbach (2009). She found that the heavier touch assistance - in which the applications were filed on behalf of the participants - increased SNAP applications relative to either of the two other groups, but that the lighter touch assistance did not increase applications relative to the control arm. Information on enrollment was not available.

by collecting detailed information on income and expenses (the latter contributing to potential deductions). Appendix A provides more detail on the nature of BDT's assistance. We model this in Section 3 as reducing the private costs of applying ($-dc$). In our setting, those costs are born by BDT, so there is not obviously a reduction in the total (private + BDT) cost of applying. However one could imagine changes in the application process that produced a net reduction in costs.

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 three-quarters of the applications it submitted. [20] For the applicant or enrollee, BDT has about 2.1 calls, averaging (in total) about 47 minutes across the callers. For callers who end up not applying, the average time on the phone is about 30 minutes.

Information Only

Our "Information Only" intervention contains only the letters and follow-up postcards to non-respondents sent as part of the outreach materials.[21] 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 directs 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 (8:45-4:45) listed on the Information Only outreach materials differed slightly from the BDT hours (9:00am-5:00pm) listed on the Information Plus Assistance outreach materials. Appendix A provides more details, and Appendix Figure 2 shows the outreach materials in the Information Only arm

Estimating the cost of these interventions is difficult because there are both fixed costs (determining a set of likely eligible individuals, designing letters and setting up a mailing system, hiring and training staff etc) and variable costs. BDT estimates the variable costs to be about $1 per attempt for the Information Only intervention; the costs consist primarily of the mailing costs, but there are also printing costs. BDT estimates the variable cost of the Information Plus Assistance intervention to be about $7 per attempt, with the higher costs due to the labor costs of the BDT staff who provide the assistance.

[20]Given that, as we will see in the results below, 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.

[21]We describe in the data section below how we tracked calls to the DHS application number; this allowed us to send analogous follow-up postcards to non-respondents in the Information Only treatment and also to measure call-in responses in this 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. Appendix A provides more detail of the sub-treatments and how they were distributed across arms. One sub-treatment we will present separately in the main text is the one where we found substantial effects: the elimination of the postcard follow-up in the standard Information Only intervention.

4.2 Empirical approach

Definition of Study Population

Our study population consists of individuals ages 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 Section 2, 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. As a result, our outreach informing individuals of their likely eligibility for SNAP may go to some individuals who, were they to apply, would not in fact ex-post be assessed as eligible for SNAP. An ancillary benefit of using Medicaid enrollment as a proxy for likely eligibility is that it enables us to measure the health of our study population.

Another implication of the construction of our study population is that it consists of individuals already enrolled in at least one public benefits program, namely 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 age 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 first order. 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).

To construct the study population, DHS supplied BDT with an outreach list of approximately 230,000 individuals aged 60 and older who are enrolled in Medicaid as of October 31, 2015; DHS also merged on a flag for whether the individual was currently enrolled in SNAP, based on data from their client information system that tracks SNAP benefit issuances. Our study population was limited to the approximately 30,000 of such individuals who were not currently enrolled in SNAP, nor enrolled in a Long Term Care Medicaid program (since individuals living in a nursing home or another "institution" that provides meals are not eligible from SNAP), and did not live in Philadelphia where BDT had already conducted extensive SNAP outreach; we also limited outreach to one individual per household, since SNAP is a household-level benefit, and to individuals BDT had not previously attempted to contact.

Randomization

Our interventions randomize the information and application assistance provided to the study population. Specifically, the 31,888 individuals in our study population were randomly split into three equally sized groups: Information Only treatment, Information Plus Assistance treatment, and control (no intervention). As noted, there were separate sub-treatments within each treatment.[22] Our main analysis compares across 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).

For practical reasons, the outreach letters were randomly distributed across 11 separate, equally sized weekly mailing batches. The first batch was sent on January 1 2016, and the last on March 16 2016; follow-up post-cards were sent eight weeks following each mailing, with the last postcards scheduled to be sent on May 11, 2016.[23] Appendix Figure 4 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 each individual 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 BDT partner 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 A2 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) has approved this research as well as the data sharing outlined in Appendix B (Protocol: 1506106206; FWA: 00004881).[24] The trial was registered on the AEA RCT Registry

[22]Specifically, 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 treatment, one-quarter received the standard outreach, while another one-quarter received the standard letter but no follow-up postcard ("no postcard") and another one-quarter received a letter that varied the description of the expected benefit amounts ("framing"). See Appendix A for more detail.

[23]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 18 and May 25, respectively.

[24]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 (NBER) judged the protocol to be exempt (IRB

(AEA RCTR -0000902) in October 2015, prior to our launch - at which point we pre-specified our primary and secondary outcomes.[25] 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; the exact analysis of study participant characteristics was still left unspecified at that point due to uncertainty on data availability.[26]

Analysis

Our main analysis consists of comparisons among three groups: 10,000 individuals in the Information Plus Assistance intervention, 5,000 individuals in the Information Only intervention, and 10,000 individuals in the control.[27] In Appendix C we present the full set of results separately for each sub-treatment; in general these sub-treatments had little or no impact.

We undertake two principal types of analysis. First, we examine the impact of the interventions on outcomes related to take-up of SNAP. Our primary take-up outcome is SNAP enrollment. However we also examine intermediate steps toward enrollment: calls in response to outreach and applications submitted.

We measure all of these outcomes over the first nine months after the mail date. As a result, our outcomes data span the period January 6, 2016 (the date of the first mailing) through December 16, 2016 (nine months after our last mailing). This was chosen to be a sufficiently long window to capture the full impact of the intervention on these outcomes.[28] Someone who enrolls in SNAP over these nine months likely remains enrolled for about 24 months, after which they need to re-certify their eligibility, although in some cases they may need to re-certify earlier. Our current data allow us to extend the analysis out to 12 months, but not long enough yet to study re-enrollment among our study population but among elderly individuals on the outreach reach who were enrolled in SNAP (Table 1 column 3); as noted earlier, the vast majority of the elderly in PA have to re-certify only every 24 months.[29]

Ref#15_129; FWA: 00003692).

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

[26]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. However we were unable to execute on our aspirations to analyze additional characteristics like earnings and credit reports data due to lack of the relevant data.

[27]As noted above, in our main analyses we pool the standard and "marketing" variant sub-treatments within each arm, re-weighting the sub-treatments so that each is given equal weight within the arm; we exclude from our main analyses two additional sub-treatments in the Information Only intervention that have no analog in the Information Plus Assistance intervention.

[28]Based on their prior outreach efforts, BDT estimated that initial responses (i.e. calls) if they are going to occur will happen within the first three months. We wanted to allow sufficient time for applying and for the state's 30-day decision time and erred on the side therefore of long time window to make sure we had the full impact. In principle we can subsequently study re-enrollment decisions of our sample, but since re-certification may not be required for 2 years, this requires a substantially longer time window.

[29]According to the USDA's SNAP Policy Data Base, in recent years over 90 percent of SNAP eligibility in PA had

Second, we examine the characteristics of the marginal applicant or enrollee whose behavior is affected by the intervention. To do this, we will define the outcome in each arm as the average of a specific characteristic among those who (endogenously) apply or enroll. For example, we will compare the average monthly benefits among those who enroll in each arm, or the average healthcare spending in 2015 of individuals who enroll (or, alternatively apply) in each arm. Differences in the average characteristics of enrollees or applicants in a given treatment arm relative to the control group reveals how the characteristic of the marginal individual who apply or enroll due to a given intervention differs from the average applicant or enrollee who would enroll absent the intervention. This approach to analyzing the characteristics of the marginal person affected by an intervention is analogous to approaches taken in prior work by Gruber et al. (1999) and Einav et al. (2010).

4.3 Data

With the exception of the call-in data, all data come from DHS. BDT received fully identified data; they provided us with a de-identified version of these data that contain a unique (de-identified) study ID. For additional details on the DHS data and the data-sharing protocols, see Appendix B.

4.3.1 Medicaid data, pre-randomization

The 2015 Medicaid outreach list that we use to construct our study population also contains a number of demographic characteristics of the individuals from their Medicaid records that we use to analyze the characteristics of our study population and compliers. In particular, the outreach file contains birth date, gender, address, and primary language, as well as which Medicaid program the individual is enrolled in. Additional characteristics of our study population come from their 2015 Medicaid enrollment and claims data, which DHS also supplied for everyone on the outreach list. The 2015 Medicaid enrollment file allows us to measure the individual's race, and the individual's history of prior Medicaid enrollment spells.

The 2015 Medicaid claims file allows us to construct detailed healthcare utilization and health measures in the year prior to the 2016 intervention. 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.

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.[30] Total medical spending is noisy - due to the well-

a re-certification period of over 13 months, which presumably means it was 24 months since other re-certification intervals are rare. https://www.ers.usda.gov/data-products/snap-policy-database.aspx

[30]Specifically, we sum up the total number of encounters of a given type and the total spending on those encounters

known high variance of medical spending - and conflates variation in utilization with variation in recorded prices. Our total number of days or visits measures attempts 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.

An additional complication is that only about three-quarters of our study population was enrolled in Medicaid for the entirety of 2015. We therefore annualized all of the health care utilization measures by dividing by the number of days enrolled out of 365. Needless to say 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, again we are not unduly concerned given that this adjustment will affect enrollees in randomly assigned arms equivalently, and we confirm this below.

Finally, we analyze the number of measured chronic conditions for each individual. A smaller number of measured chronic conditions could reflect better underlying health of applicants and enrollees in the intervention arms. It could also -partly or entirely - reflect the lower health care utilization of these applicants and enrollees; since chronic conditions are only measured if the individuals use the relevant health care, they are a joint measure of underlying health and health care utilization (Song et al., 2010; Finkelstein et al., 2017) .

Summary statistics: study population

Table 1 illustrates the construction of our study population and the pre-randomization characteristics of the sample. Column 1 shows the initial outreach list of 229,584 individuals aged 60 and over enrolled in Medicaid as of October 31, 2015. In column 2 we exclude individuals enrolled in the Long-Term Care Medicaid program (N= 47,729) and individuals with an address in Philadelphia City (N= 37,932). Of the resulting 143,923 individuals on the outreach list after these exclusions, column 3 shows the 84,038 (about 60 percent) who were enrolled in SNAP or living with someone enrolled in SNAP, while column 4 shows the 59,885 who were not enrolled in SNAP and not living with anyone in SNAP; recall that SNAP is a household-level benefit. Column 5 shows our final study population of 31,888 individuals. These are a subset of the individuals not enrolled in SNAP in column 4. From column 4, we randomly select one individual from each "household" (this excludes 1,842 individuals)[31], and excluded all individuals to whom BDT had previously sent any outreach materials (N=26,155).[32]

There is no clear demographic gradient between Medicaid enrollees who do and do not enroll in SNAP. Columns 3 and 4, respectively, describe some characteristics of each group. Those not on SNAP (column 4) are older, with similar gender, racial, and language makeup than those not

across our study population and divide total spending by total encounters to get a per encounter average "cost". The results are: $2,150 for a hospital day, $240 for an ED visit, $166 for a SNF day,and $79 for a doctor visit.

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

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

on SNAP (column 3). On some dimensions they appear sicker - they have more hospital days and Skilled Nursing Facility (SNF) days, but on other dimensions they appear healthier - 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). [33]

4.3.2 Outcomes data

Applications, Enrollment and Benefit Amounts DHS provided data on SNAP applications from January 2007 through July 2017. As noted, SNAP is a household-level benefit, so application data is at the household level, but contains a list of all individuals in the household associated with the application. DHS matched this list to our individual-level outreach list, so that we can track application information on all individuals. 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 ("did the individual enroll 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 intervention; however we also examine the time pattern of effects out through 16 months post intervention.

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.[34] 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. 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 given 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

[33]We also used national survey data to compare consumption and income for individuals on Medicaid who either were or were not on SNAP; measurement of Medicaid and SNAP enrollment likely has more error in such survey data than in administrative data (Meyer and Mittag 2015). We limited our analysis to individuals 60 and over on Medicaid in households below 200 percent of the poverty line (a proxy for likely SNAP eligibility). In the pooled 2010-2015 Consumer Expenditure Survey, those not on SNAP have higher (mean or median) per capita consumption across a variety of consumption measures. In the 2016 March CPS, those not on SNAP have higher (mean or median) per capital household income; for example, mean per capita annual household income is about \$10,500 among those enrolled in SNAP compared to about \$12,700 among those not enrolled. However mean (or median) total household income is virtually identical between the two groups.

[34]In principle the monthly benefit amount should be constant. For the small number of individuals where it varies, we use the modal amount. See Appendix B for more details.

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. Appendix C provides more details on the call-in data, the call forwarding service, and the script for the receptionists, which was provided in English and in Spanish. 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 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. Appendix C provides more details on this adjustment procedure.

5 Results

5.1 Behavioral Responses to Intervention

Enrollment, applications, and calls

Table 2 presents the main take-up results of the experiment by intervention arm. All outcomes are measured in the nine months after the initial mail date. The first row shows results for enrollment. In the control group, about 6 percent enroll. The Information Only intervention increases enrollment by 5 percentage points, or 83 percent relative to the control. 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 ($p<0.001$).[35]

Figure 1 shows the time pattern of intervention impacts on enrollment by month, for the 16-month period after the initial mail date. 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 persist, at least out through the 16 months we can observe post-intervention. This suggests that the interventions are generating new enrollment, as opposed to merely "moving forward" in time enrollment that would otherwise happen.[36]

[35] For some perspective on these numbers, we considered how these 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).

[36] Note that because most new enrollees do not have to re-certify for 24 months, we expect any new enrollment

The next two rows of Table 2 show the impact of the interventions on applications, and on their failure (rejection) rate. The impacts on applications are roughly proportional to the increase in enrollment. About 8 percent of the control group applies. The Information Only intervention increases applications by 7 percentage points, and the Information Plus Assistance intervention increases applications by 16 percentage points. Again, the intervention impacts are statistically different from the control and from each other ($p<0.001$).

Assistance may increase enrollment over and above only providing information by increasing the supply of applications (willingness to apply) and/or by increasing the the success (i.e. approval) rate of a given application; the assistance includes helping the application provide the appropriate documentation, list all relevant expenses (which can affect eligibility), and navigate any follow-up requests or issues raised by DHS. 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. However, the results suggest that assistance affects enrollment primarily by affecting individuals' willingness to apply, the success rate of applications is effectively the same in the two intervention arms.

If anything, the rejection rate among applications in the two intervention arms may be slightly higher than for the control group; rejection rates are 23.3 percent for the controls, compared to 26.6 in the Information Only arm and 25.5 in the Information Plus Assistance arm, although these rates are not statistically distinguishable at conventional levels. In other contexts, changes in transaction costs have similarly had a small effect on rejection rates of applicants. Deshpande and Li (2017) find that increasing transaction costs (via closings of Social Security field offices) results in about a two percentage point increase in rejection rates for SSDI applicants, while Alatas et al. (2017) find that increasing transaction costs (via requiring individuals to apply for the benefit as opposed to the government automatically screening potential eligibles) decreases rejection rates by about two percentage points in a conditional cash transfer program in Indonesia.

In Appendix Table A8 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 problems with the application - e..g failure to document citizenship.[37] One natural interpretation of these patterns is that the

generated by our interventions to persist at least that long. Recertification rates for our population are not currently available. However, data from the Food and Nutrition Services of SNAP administrative records from six states (non of which are Pennsylvania) from the 2011 - 2012 period indicate that typically about one-quarter to one-third of the elderly fail to recertify. See Mills et al. (2014) for more detail on the data. We are grateful to Colin Gray for producing this statistic from those data.

[37]For example, 24% of rejections in the Information Plus Assistance arm are due to "voluntarily withdrawal", compared to 16 percent in the control arm, and 22 percent of rejections in the Information Plus Assistance arm are due to failure to keep the appointment (interview) compared to 12 percent in the control arm. By comparison, only 6 percent of rejections in the Information Plus Assistance arm are due to failure to document citizenship, compared to 14 percent in the control, and only 1.5 percent of the rejections in the Information Plus Assistance arm are due to an application error (compared to 3.7 percent in the control). For the most part, the share of rejections for different reasons in the Information Only intervention look similar to the control arm.

assistance reduces the error rate on applications, but also pushes marginally motivated individuals to start the application process.

The last four rows of Table 2 examine call-in rates. In both treatment arms, the outreach materials suggested calling as the way to find out more and begin an application. 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.[38] We show the raw caller rate, as well as the adjusted caller rate designed to account for the lower measurement of callers in the Information Only arm (see Section 4.3 above). The raw rates show a 30 percent caller rate in response to the Information Plus Assistance outreach letters, and a 27 percent caller rate in response to the Information Only outreach letters; with the adjustment the adjusted caller rate rises to 29 percent in the Information Only intervention, statistically indistinguishable from the response to the Information Plus Assistance.[39] The similar caller-rate is not surprising given the (deliberate) similarity of the outreach materials (recall Appendix A and Appendix Figures 1 and 2). 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 (results not shown) the average caller made 1.8 calls in the Information Plus Assistance arm and 1.6 calls in the Information Only arm; these differences are statistically distinguishable ($p<0.001$).

The evidence suggests that all marginal applicants affected by the interventions call in response to the outreach materials: the share of people who apply without calling is the same in all three arms - about 8 to 9 percent. Such individuals presumably call the state directly (without being routed through BDT or our tracking service), and/or apply on-line or in person without a call. 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 at least one-quarter of non-callers are actually ineligible for SNAP, given that one-quarter of applications are rejected; perhaps an even larger share of non-callers believe themselves (potentially correctly) to be ineligible.[40]

[38] Appendix Table A7shows callers from each intervention arm into each possible number (with 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. While about 30 percent of the intervention arms call in to the designated phone number, less than half a percent call into a phone number designated for another arm. Indeed, the less than 0.5 percent call into a "wrong" arm in the Information Only treatment. "Wrong calls" to the Information Plus Assistance arm are slightly higher (around 0.4 to 0.5 percent - see column 1), presumably reflecting the fact that a few individuals learn about BDT through non-outreach materials.

[39] Appendix Figure A7 shows the monthly pattern of callers and applications post intervention. The time patterns are similar to what we saw for enrollment in Figure 1.

[40] We generated a predicted eligibility measure in which we predict eligibility based on the relationship between application approval and the pre-randomization demographic and health characteristics shown in Table 1. We estimated this prediction using the sub-sample of the study population that applied, and then used the estimates to predict eligibility rates among non-callers and callers separately. The results suggest that - at least based on the available observable characteristics - non-callers had only a few percentage points lower predicted eligibility rates than callers.

If we interpret calling a 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 calling are about 45 percent when information and assistance is provided compared to 23 percent when only information is provided.

All of the results shown in Table 2 are based on comparisons of mean outcomes by intervention arm. No covariates are needed given the simple random assignment. For completeness however, we show in Appendix Table A14 that all of the results in Table 2 are robust to controlling for baseline demographic and health characteristics of the individuals, as well as for the date of their mail batch.

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; the cost of the latter 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); presumably the remaining 15 cents consists of printing and assembling the mailings. These numbers suggest 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 government from processing applications and paying benefits. Because SNAP benefits are financed by the federal government, our results suggest that the state benefits financially from encouraging 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 \$240 per application; 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 20 percent of the new federal benefits received by state residents, and presumably spent largely at local retail outlets. As a result, both of these interventions seem to pass a very simple cost-benefit test from a state public finance perspective. However, note that this conclusion would generate different conclusion if virtually all of new enrollees received minimum benefit level (\$16 per month or \$192 per year). This is similar to the state's administrative costs.

Effects of reminders and of other sub-treatments

As noted, we conducted a number of sub-treatments that variant the presentation and frequency of the information sent. Table 3 shows results for two of the information-only sub-treatments. Specifically it shows results for 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.[41]

The results indicate that reminders matter: all behavioral responses decrease by about 20 percent without the reminder postcard. Specifically, the standard Information Only intervention (with the reminder post card) 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 ($p = 0.016$). The cost per additional enrollee is similar with and without the reminder postcard. [42]

Our finding of a substantial impact of a similar, follow-up letter is similar to Bhargava and Manoli's (2015) finding in the context of an EITC take-up intervention. There, they found that a similar second reminder letter, sent just months after the first, increased EITC take-up by 14 percentage points. They interpret the effect of the reminder as indicative of it combating low program awareness, inattention or forgetfulness; they present additional survey evidence consistent with low program awareness among those eligible for the EITC. 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; more broadly in the SNAP population, surveys suggest that about half of likely eligible, nonparticipants in SNAP reported that they were not aware of their eligibility (Bartlett et al. 2004). In our framework in Section 3, this is modeled as under-estimating the probability of eligibility (i.e. $\epsilon < 0$). An alternative explanation - that the impact reflects a high rate of non-delivered mail - does not seem warranted; less than 1 percent of outreach materials were returned to sender.

For completeness, Appendix Table A4 reports behavioral responses separately for all of the 6 individual sub-treatments we conducted. Other than the impact of a lack of a postcard reminder, we found no differential effects of other treatments. In particular, we found no differential impact when outreach materials were re-designed to attract clients by using a "marketing" approach that borrowed language and graphics from credit card solicitations; and we found no differential impact when outreach materials were redesigned to try to "frame" the expected benefit amount to seem larger by emphasizing the maximum possible benefit amount rather than the average benefit amount.

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

[42] The reminder postcard had a marginal cost of roughly $0.35 and increased enrollment relative to the "no postcard" Information Only intervention by 2 percentage points; this suggests a cost per additional enrollee of about $18.

5.2 Characteristics of Marginal Applicants and Enrollees

We investigated not only the number of applications and enrollments generated by the interventions but also the characteristics of the marginal applicants and enrollees affected by the interventions. We have already seen that one characteristic of marginal applicants - their probability of rejection - was similar to the control applicants (see Table 2). Here, we present evidence that marginal applicants and enrollees in either intervention arm are better off than the average applicants and enrollees who apply in the absence of the intervention.

In what follows we focus mainly on the comparison of characteristics between enrollees in the control group and enrollees in the interventions. Characteristics of applicants and enrollees tend to be similar in the two intervention arms. And within each intervention arm, characteristics of applicants look similar to characteristics of enrollees (and also to callers - see Appendix Table A11); this suggests that the interventions generated interest (calls) in different types of individuals than the average applicant or enrollee, but that, conditional on inducing interest, there was no further differentiation in the characteristics of those who applied or who successfully enrolled.

Monthly benefits among enrollees

We begin by examining the monthly benefits for individuals who enrolled in the 9 months after the initial mail date, by study arm. Recall that the SNAP benefit formula is progressive; a lower benefit benefit amount therefore implies an enrollee who has higher net resources. This is why in Section (3)we defined "targeting" as the share of enrollees who are high benefit (relative to the share that are low benefit).

Table 4 shows the results.[43] Average monthly benefits for enrollees in the intervention arms are 20 to 30 percent lower than for enrollees in the control arm. 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 ($p<0.001$) s well as from each other ($p=0.013$). 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 A6 reports the average characteristics for always takers and compliers; estimation of these objects is standard (see, e.g., Abadie 2002, Abadie 2003, or Angrist and Pischke 2009) and we describe it in more detail in Appendix F.

There are clear modes in the distribution of benefits received, corresponding to minimum and maximum benefit amounts. For example, 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). Appendix Figure A8 shows the full distribution of monthly benefit amounts among control individuals who enrolled during

[43]For completeness, Appendix Table A5 shows the same analyses for each sub-treatment. There do not appear to be systematic differences in enrollee benefits across the various sub-treatments.

our 9 month observation window. Table 4 shows that the interventions increased the share of individuals receiving the minimum benefit and decreased the share of individuals who received the maximum benefit. For example, in the control arm, 18 percent of enrollees receive the minimum benefit, compared to 30 percent in the Information Only arm and 36 percent in the Information Plus Assistance arm.

A potential concern with our enrollee benefit analysis is that the average 4 percent missing benefit information discussed in Section 4.3 varies by the intervention arm. As shown in Table 4, we are missing 7.5 percent of benefits for control enrollees, but only 4.3 percent for enrollees in the Information Only intervention and only 2.8 percent for enrollees in the Information Plus Assistance intervention; 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 also generated a predicted benefit measure in which we predict the benefit amounts based on the relationship between benefits and the pre-randomization demographic and health characteristics shown in Table 1; Appendix D provides more detail on the prediction algorithm which follows a standard algorithm in machine learning (Rifkin and Klautau 2004). The last two rows of Table 4 shows that predicted benefits show the same pattern as actual benefits, both among enrollees with non-missing benefit amounts (second to last row) and among all enrollees (last row): predicted benefits are statistically significantly lower ($p<0.001$) in either intervention arm relative to the control arm; they are not statistically distinguishable between intervention arms at conventional levels.

Another potential concern with our enrollee benefit analysis is that we are interpreting lower benefits as reflecting individuals with higher resources, but benefits are also increasing in household size. Thus if the interventions disproportionately encourage smaller households to apply, this will lower enrollee benefits without necessarily reflecting higher *per capita* resources. Indeed, the penultimate row of Table 4 shows that the interventions increase the share of enrollees who are in a household size of 1. However, the bottom row of the Table 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. The decline in average benefits thus reflects a per capita decline in benefits, or increase in net resources. An additional attraction of limiting to households with only a single individual is that we have no missing benefits for such households.

Demographics and health of applicants and enrollees

Table 5 shows the demographic and health characteristics of applicants and enrollees.[44] The first four columns show these characteristics for applicants, and the last four for enrollees. For a variety of demographic and health characteristics, marginal applicants and enrollees from the intervention

[44] Once again, for completeness, we also show these results separately for each sub-treatment; see Appendix Tables A12 and A13. We also report results for average takers and compliers (see Appendix Table A10 for results and Appendix F for more detail on how these are computed).

appear better off than the average applicant or enrollee in the control group.

Panel A shows results for a summary measure: predicted benefits, where the prediction is based on the underlying pre-randomization demographic and health information and higher predicted benefits correspond to lower net resources given the progressive SNAP benefit formula. The results for enrollees were already seen in the last row of Table 4. Results for applicants are similar: applicants in either intervention arm have lower predicted benefits than applicants in the control arm ($p<0.0001$).

Panel B shows results for the underlying health measures, which were measured in the calendar year prior to the intervention. Relative to the control arm, applicants and enrollees in either intervention arm are healthier and use less health care.[45] On all three health care utilization measures, applicants and enrollees in the intervention arms have lower pre-randomization health care use than in the control arm, although these differences are not always statistically different from the control. However, when we pool across both intervention arms, the total number of visits and days and the weighted total number of visits and days are statistically different from the control arm ($p<0.05$) for both applicants and enrollees. In the final row of Panel B we show that the number of measured chronic conditions is lower in both intervention arms relative to the control arm for both applicants and enrollees, with most of these differences statistically significant at conventional levels.

Panel C shows results for demographic characteristics measured on the pre-randomization outreach list. Relative to the control group, applicants and enrollees in either of the 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, 7 percent of control enrollees are 80 or older, compared to 12 percent in the Information Only intervention and 14 percent in the Information Plus Assistance intervention; likewise 71 percent of control enrollees are white, compared to 78 percent in either intervention arm.

6 Normative implications

We use the framework from Section (3) to conduct some back-of-the-envelope welfare interpretations of our empirical findings regarding the impact of the interventions on applications, enrollment, and targeting. Data from the controls together with results from the experiment allow us to parameterize the model. This parameterization suggests the presence of non-trivial under-estimation of expected benefits (i.e. $\epsilon < 0$), which is a necessary condition for the interventions to produce private welfare gains (see Proposition 1). It also suggests that this under-estimation of expected benefits is greater for less-well off individuals (i.e., $\epsilon_h < \epsilon_l < 0$), which is a sufficient condition for interventions that worsen targeting to have have lower welfare benefits (see Proposition 2). We

[45] 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 A2). Therefore, not surprisingly, we find in Appendix Table A15 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).

then use the parameterized model to analyze the normative implications of our estimated impacts of the interventions.

6.1 Parameterizing the model

To simplify the parameterization of the model, we collapse the distribution of benefits to be only one of two possible levels: either the minimum benefit of \$16 per month or \$178 / month (which is the mean benefit for the approximately 80 percent of control group enrollees who do not receive the minimum). We further assume that these two benefit levels also correspond theoretically to the l and h types in the model. In other words, l types apply and receive the minimum benefit with certainty conditional on application being accepted, and h types apply and receive the higher benefit level.[46] These assumptions imply that type h enrollees receive \$4,272 during the first 24 months of enrollment, while type l enrollees receive \$384 over 24 months. Recall that after 24 months, the individual must recertify their eligibility; average lifetime benefits are therefore presumably greater than the 24-month amount, but may not extend indefinitely; moreover, additional private costs must be incurred to maintain them. For simplicity, we assume benefits last only 24 months; this is a conservative assumption since, as we will see, higher expected benefits among enrollees translate into larger misperceptions about the probability of successfully enrolling.

To calculate expected benefits from applying, we assume that the probability of rejection is 0.25 for both types (the rejection rate for the controls). We treat this rejection rate as exogenous to the intervention, given that we found no evidence of an effect of either intervention on rejection rates of application. Thus, expected benefits conditional on applying ($\pi_j B_j$) are about $\$3,200$ for the h types and about \$290 for l types. This calculation assumes that SNAP benefits are valued dollar for dollar by beneficiaries.[47]

The model in Section 3 underscores that a key determinant of the welfare analysis will be whether the the neoclassical benchmark ($\epsilon = 0$) is a reasonable assumption. It is difficult to definitively reject this neoclassical benchmark. Given that applying takes the individual 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.[48] If there are no misperceptions of the probability an application is accepted, then to rationalize the decision not to apply requires a non-time cost of applying of roughly \$3,100 for an h 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 forty times larger than their transactional costs of applying. For an l type with no misperception of the probability an application is accepted, the implied non-time cost of applying is roughly \$200.

[46]In other words, $\pi_{lL} = \pi_{hH} = 0.75$ and $\pi_{lH} = \pi_{hL} = 0$, so there is no "type uncertainty" in this setup, only uncertainty over application acceptance probability.

[47]While recent evidence by Hastings and Shapiro (2017) 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.

[48]This is slightly higher than our estimate of BDT's cost per application in the Information Plus Assistance intervention of \$45 (\$60 per enrollee, adjusted for a 75 percent acceptance rate).

Under this rational benchmark, the Information Only intervention overall unambiguously reduces private and social welfare (Proposition 1); in addition, the poor targeting properties of the intervention are irrelevant for its private welfare impacts and increase social welfare for a given change in application rate due to reduction in the average fiscal cost of enrollees (Proposition 2).

However, our reading of the available 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., underestimating expected benefits - is a primary barrier to participation among eligible non-participants (Bartlet et al., 2004); one interpretation of our "Information Only" intervention is that it reduces such misperception. 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 the time cost is the only cost of application. To rationalize non-participation with the time cost estimates above requires $\epsilon_h = -0.98$ and $\epsilon_l = -0.75$. In other words, for a type h individuals with a 75 percent chance of enrolling after applying, the only way to rationalize their not applying for benefits is if their misperceptions are so great that they perceive virtually no chance (less than 2 percent) of enrolling in program, or alternatively they are completely ignorant of the program. For type l individuals with a 75 percent chance of enrolling after applying not to apply, they must perceive a 25 percent or lower chance of success.

6.2 Normative Findings

Implications of the reduction in targeting

Our results indicate that both interventions decrease targeting. However, Proposition 2 established that there is no general relationship between the targeting properties of an intervention and the likelihood the intervention raises social welfare. Indeed, for marginal interventions (which we believe is a reasonable approximation for our context), Proposition 2 establishes the stronger result that in the neoclassical benchmark, there is no relationship between the targeting properties of the intervention and its impact on private welfare, and that - given that benefits are progressive - the worsening of targeting is. for a given change in applications, better for social welfare.

As noted in previous subsection, our empirical results are not likely to be consistent with this neoclassical benchmark. Moreover, if time costs are the only application costs (or if any unmodeled application costs such as stigma are the same across types), our back of the envelope calculation in previous section imply that $\epsilon_h < \epsilon_l < 0$. Proposition 2 says that this condition is sufficient for a reduction in targeting to imply lower welfare benefits from the intervention. Thus our targeting findings, together with our back-of-the-envelope calculations suggesting that misperceptions exist and are higher for the targeted h type, bode poorly for the welfare impacts of the interventions.

Welfare impacts of intervention

Even with misperceptions (and even with $\epsilon_h < \epsilon_l < 0$), the targeting effects of the intervention are neither necessary nor sufficient to sign the overall social welfare impact of the intervention. Consider an intervention that improves targeting (with $\epsilon_h < \epsilon_l < 0$), Proposition 2 tells us that, all else equal, this improvement in targeting is good for the social welfare effects of the intervention. Still, the overall social welfare effect may be negative, for the negative externality from the public processing costs (g) and expenditures on benefits may outweigh the private welfare gains to individuals with misperceptions (see equation (2)). Likewise, if the intervention worsens targeting - thereby reducing the social welfare benefits from the intervention under our sufficient conditions, it may still increase social welfare overall if the private welfare gains to individuals with misperceptions outweighs the public costs.

Proposition 1 tells us that to make quantitative statements about the social welfare impact of the intervention - i.e., $\frac{dW}{dT}$ - we need estimates of the public processing cost g, $\pi_j B_j$, $\frac{dA_j}{dT}$ and $\mu_j \equiv -u'(y_j)\epsilon_j(\pi_j B_j)$ (for $j = \{h, l\}$). As noted earlier, we used Isaacs (2008) to estimate $g \sim \$240$ and we parameterized $\pi_h B_h \sim \$3,200$, $\pi_l B_l \sim \$290$, $\epsilon_h \sim -0.98$, and $\epsilon_l \sim -0.75$.

The impact of the intervention on applications of each type $\frac{dA_j}{dT}$ comes directly from the experiment. Table 2 shows directly the increase in applications - for the Information Only intervention, $\frac{dA}{dT} = 0.07$ and for the Information Plus Assistance intervention, $\frac{dA}{dT} = 0.16$. Appendix Table A6 shows that, for each intervention, 44% of the marginal enrollees are l types (i.e. 44% of the compliers receive the minimum benefit level of \$16); this represents a worsening of targeting relative to the inframarginal enrollees (i.e. the always takers) for whom, Table 2 shows, only about 20% are l type. Given our assumption of a common, 25 percent, rejection 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 $\frac{dA_h}{dT} = .09$. Finally, we still require an estimate of the marginal utility of consumption for recipients (as well as our assumption of a particular set of social welfare weights on them - in our case, utilitarian). Consumption is notoriously difficult to measure in a low income population, and the marginal utility of that consumption is an even more challenging object, as are social welfare weights. Given this difficulty, we instead follow Hendren (2016), which offers a way to assess redistributive programs without having to make assumptions about either individual utility functions or social welfare functions. He defines the marginal value of public funds ($MVPF$) from a marginal expansion of a program (or in our case, an intervention into that program) as the ratio of marginal benefits to marginal costs. In our setting, we can follow Hendren (2016) and derive the following expression:

$$MVPF^{Information\ Only} = \frac{-\epsilon_h(\pi_h B_h)\frac{dA_h}{dT} - \epsilon_l(\pi_l B_l)\frac{dA_l}{dT}}{(\pi_h B_h + g)\frac{dA_h}{dT} + (\pi_l B_l + g)\frac{dA_l}{dT}}.$$

This formula bears a strong resemblance to $\frac{dW}{dT}^{Information\ Only}$ (see equation (3)). Beyond the fact that one is expressed as a ratio and the other as a difference, the key distinction is that the private welfare changes (the numerator) are expressed as a money metric, rather than multiplied by the marginal utility of consumption. We therefore have rough estimates of all the elements we need to evaluation this expression, and these suggest:

$$MVPF^{Information\,Only} = \frac{0.98(\$3,200)0.04 + 0.75(\$290)0.03}{(\$3,200 + \$240)0.04 + (\$290 + \$240)0.03} = 0.86$$

An MVPF estimate of 0.86 suggests that for every dollar spent on the intervention (in the form of benefits and processing costs), low income recipients receive about 86 cents of benefits.[49] An MVPF below 1 is to be expected for a redistributive policy such as SNAP; redistribution inevitably involves some resource cost (Okun, 1975). A more natural benchmark is to compare this estimate to the MVPF of other redistributive programs. Although we know of no elderly-specific estimates, it is interesting to see that this estimate is comparable to the estimate of the MVPF for the Earned Income Tax Credit, which Hendren (2014) estimates to be about 0.9; this is higher than the likely MVPF of public subsidies for health insurance for low-income adults (Finkelstein, Hendren and Shepard 2017) as well as the SNAP program for the non-elderly, which Hendren (2014) estimates has an MVPF of 0.5 to 0.7. In other words, an information intervention about likely eligibility for SNAP among an elderly population transfers more resources to low-income beneficiaries per dollar of public expenditure than either EITC expansions, the SNAP program for the non-elderly, or subsidies for public health insurance.

At a broad level, these comparisons are useful as a benchmark of how much it costs to redistribute to low income individuals through other programs. But of course, this is not an apples-to-apples comparison. The programs affect different types of low-income people - SNAP for low-income elderly as compared to EITC, SNAP, and subsidized health insurance for the low-income working age population. Relatedly the source of social costs are different: administrative costs in the context of the elderly SNAP recipients, labor supply distortions from the EITC and the non-elderly SNAP program, and moral hazard effects of health insurance.

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

$$MVPF^{Information\ Plus\ Assistance} = \frac{-\epsilon_h(\pi_h B_h)\frac{dA_h}{dT} - \epsilon_l(\pi_l B_l)\frac{dA_l}{dT} - (A_h + A_l + \frac{dA_h}{dT} + \frac{dA_l}{dT})\frac{dc}{dT}}{(\pi_h B_h + g)\frac{dA_h}{dT} + (\pi_l B_l + g)\frac{dA_l}{dT}}.$$

The MVPF for the Information Plus Assistance intervention is the same as for the Information Only intervention, plus one additional term in the numerator, which represents 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 $u'(y_j)$

[49] 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 no noticeable effect on the calculation.

terms multiplying the infra-marginal applicants in the expression for $\frac{dW}{dT}^{Information\ Plus\ Assistance}$ (see equation (3)). This unambiguously increases the $MVPF$ because the application costs are assumed to have been costlessly reduced, as would correspond to removing some preexisting barrier or ordeal.

If the intervention costlessly eliminated private application costs (i.e. reducing them from \$75 per application to zero), this would increase the MVPF substantially, from 0.86 in the Information Only intervention to 0.98; this increase comes about from the \$75 per application gain for the 24 percent of the intervention group who applies (see Table 2). If we allow for BDT's cost per application estimate of \$45 (\$60 per enrollee, adjusted for the acceptance rate), the MVPF for the Information Plus Assistance Intervention would fall to 0.91.

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

$$
\begin{aligned}
MVPF_h^{Information\ Only} &= \frac{-\epsilon_h(\pi_h B_h)\frac{dA_h}{dT}}{(\pi_h B_h + g)\frac{dA_h}{dT}} = \frac{0.98(\$3,200)0.04}{(\$3,200 + \$240)0.04} = 0.91 \\
MVPF_l^{Information\ Only} &= \frac{-\epsilon_l(\pi_l B_l)\frac{dA_l}{dT}}{(\pi_l B_l + g)\frac{dA_l}{dT}} = \frac{0.75(\$290)0.03}{(\$290 + \$240)0.03} = 0.41
\end{aligned}
$$

As Proposition 2 predicts, given our estimate of $\epsilon_h < \epsilon_l < 0$, the MVPF of the intervention is larger for h types. The difference is substantial, highlighting the 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 (~0.4). In other words, in an extreme version of the neoclassical theory in which those deterred by barriers are exclusively the better off, our interventions would have looked substantially worse.

7 Conclusion

Policymakers often advocate - and academics often study - interventions to increase take-up of public benefits. We provide a framework for analyzing the welfare impacts of such interventions and apply it to the results of a randomized field experiment of interventions designed to increase take-up of a public benefits program among 30,000 low-income elderly individuals in Pennsylvania who were likely eligible for, but not enrolled in, the Supplemental Nutrition Assistance Program (SNAP). The interventions were designed to address potential information barriers to enrollment as well as transaction cost barriers, and to assess the impact of these barriers on the number who enroll, the types of individuals who enroll, and on private and social welfare.

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 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, and persists out to at least 12 months; the time pattern of effects suggests that the treatments 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", at an intervention cost of about $20 per new enrollee, compared to a cost of about $60 per new enrollee in the Information Plus Assistance intervention.

We were able to track several stages of the behavioral response, including initial calls in response to the outreach materials, and applications. Both interventions generated increases in applications that were roughly proportional to the increase in enrollment; there was no improvement in the approval rate. Both interventions also generated about a 30 percent call in rate (i.e. initial sign of interest). It appears that all marginal applicants affected by the intervention called in response; application rates among those not calling in were similar to the control group. Among those who called in, enrollment rates were much higher in in the Information Plus Assistance arm (45 percent) than in the Information Only arm (23 percent).

The framework we develop for analyzing these results 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. The model 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 - in order to draw normative inferences from targeting results. This should hopefully be useful for analyzing the welfare impacts of related interventions in the academic literature, which has focused on targeting properties based on observable characteristics.

Several pieces of evidence are consistent with standard behavioral models in which individuals under-estimate expected benefits from applying, with this under-estimation greater among less well-off individuals. This is a sufficient condition for an improvements in targeting to increase the welfare gains from intervention. However, in contrast to the assumptions of many behavioral models, we find that reducing informational or transactional barriers worsens targeting: the marginal applicants and enrollees from either intervention are better off than the average enrollees in the control group. The average monthly SNAP benefit (which is based on a progressive formula) is 20 to 30 percent lower among enrollees in either intervention arm than enrollees in the control group. In addition, relative to the control, 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. Nevertheless, given the magnitude of the misperceptions we estimate, our estimates suggest that increasing awareness and reducing transaction costs for elderly individuals eligible for SNAP are relatively cost-effective ways to redistribute to this low income population.

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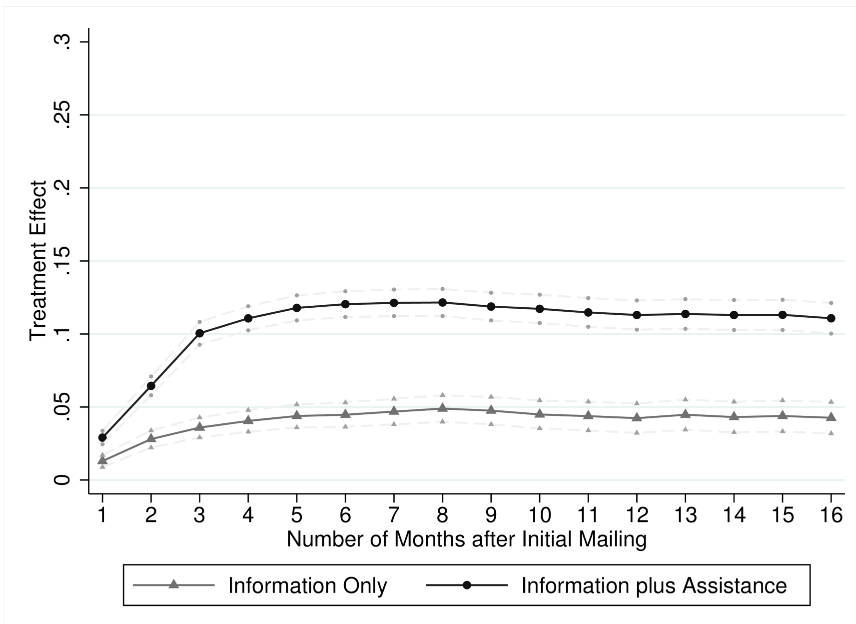
Figure 1: Time pattern of enrollment responses



NOTE: Figure shows, by month, the (cumulative) estimated treatment effects on enrollment (relative to the control) for the Information Only arm and the Information Plus Assistance arm. 95 percent confidence intervals on these estimates are shown in the dashed light gray lines.

Table 1: Description of Study Population

	Original Outreach List	List, After Exclusions	After Exclusions: Receving SNAP	After Exclusions: Not Receiving SNAP	After Exclusions: Study Population
	(1)	(2)	(3)	(4)	(5)
Observations (N)	229,584	143,923	84,038	59,885	31,888
Panel A - Demographics					
Age (as of October 31, 2015)	72.91	70.45	69.77	71.42	68.83
Share Age 80+	0.27	0.18	0.15	0.23	0.16
Male	0.35	0.36	0.36	0.36	0.38
Share White[a]	0.71	0.79	0.79	0.79	0.75
Share Black[a]	0.17	0.10	0.11	0.07	0.08
Share Primary Language not English	0.04	0.03	0.03	0.03	0.04
Share Living in Philadelphia	0.18	0.00	0.00	0.00	0.00
Share Living in Pittsburgh	0.05	0.07	0.07	0.06	0.06
Share Last Medicaid Spell Starting before 2011	0.45	0.47	0.55	0.36	0.33
Share Enrolled in Medicaid for 2015 Full Year	0.83	0.84	0.89	0.77	0.73
Panel B - (Annual) Health Care Measures, 2015					
Total Health Care Spending ($)[b]	18,347	7,683	6,036	9,995	11,838
Number of Hospital Days	5.41	1.51	1.24	1.88	2.16
Number of ER Visits	0.41	0.41	0.41	0.40	0.50
Number of Doctor Visits	6.25	5.87	5.97	5.74	7.11
Number of SNF Days	66.23	1.57	0.85	2.58	2.67
Number of Chronic Conditions	6.50	4.93	5.08	4.70	5.45

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

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

	Control	Information Only	Information Plus Assistance	P Value of Difference (Column 2 vs 3)
	(1)	(2)	(3)	(4)
SNAP Enrollees	0.058	0.105	0.176	
		[0.000]	[0.000]	[0.000]
SNAP Applicants	0.077	0.147	0.238	
		[0.000]	[0.000]	[0.000]
SNAP Rejections among Applicants	0.233	0.266	0.255	
		[0.119]	[0.202]	[0.557]
Callers	0.000	0.267	0.301	
		[0.000]	[0.000]	[0.000]
Adjusted Callers	0.000	0.289	0.301	
		[0.000]	[0.000]	[0.142]
SNAP Applicants among Non-Callers	0.077	0.086	0.081	
		[0.063]	[0.324]	[0.363]
SNAP Applicants among Callers	0.000	0.313	0.602	
		[0.000]	[0.000]	[0.000]
SNAP Enrollees among Non-Callers	0.058	0.061	0.059	
		[0.442]	[0.713]	[0.688]
SNAP Enrollees among Callers	0.000	0.226	0.450	
		[0.000]	[0.000]	[0.000]
Observations (N)	10,630	5,314	10,629	

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

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

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

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

Table 4: Enrollee Monthly Benefits and Predicted Benefits

	Control	Information Only	Information Plus Assistance	P Value of Difference (Column 2 vs 3)
	(1)	(2)	(3)	(4)
Benefit Amount	145.85	115.38	101.32	
		[0.000]	[0.000]	[0.013]
Share $16 Benefit	0.178	0.299	0.357	
		[0.000]	[0.000]	[0.012]
Share $194 Benefit	0.189	0.157	0.143	
		[0.150]	[0.012]	[0.421]
Share $357 Benefit	0.055	0.050	0.039	
		[0.681]	[0.115]	[0.285]
Share Missing Benefit	0.075	0.043	0.028	
		[0.019]	[0.000]	[0.139]
Predicted Benefit for Enrollees w/ Actual Benefit	140.11	112.67	102.63	
		[0.000]	[0.000]	[0.070]
Predicted Benefit for All Enrollees	138.65	114.18	103.73	
		[0.000]	[0.000]	[0.056]
Share of Enrollees in Household Size of 1	0.651	0.701	0.755	
		[0.066]	[0.000]	[0.017]
Benefit Amount for Enrollees in Household Size of 1	113.70	90.59	85.81	
		[0.000]	[0.000]	[0.294]
Observations (N)	613	559	1,861	

Notes: Sample is individuals who enrolled in the 9 months after their initial mailing. Columns 1 through 3 shows means by intervention arm with the p-value (relative to the control arm) in [square brackets] for SNAP enrollees. Column 1 shows the control. Column 2 shows the Information Only arm (with the two equally-sized sub-treatments pooled). Column 3 shows the Information Plus Assistance arms (weighted so that the two pooled sub-treatments received equal weight). Column 4 reports the p-value of the difference between the Information Plus Assistance and Information Only treatment arms. See text for a description of the predicted benefits. All p-values are based on heteroskedasticity-robust standard errors. N reports the sample size of enrollees.

Table 5: Demographic and Health Characteristics: Applicants and Enrollees

	Applicants				Enrollees			
	Means			P Value	Means			P Value
	Control	Info Only	Info Plus Assistance	Info Plus Assistance vs Info Only	Control	Info Only	Info Plus Assistance	Info Plus Assistance vs Info Only
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A - Predicted Benefits								
Predicted Benefits	148.38	125.89	115.11		138.65	114.18	103.73	
		[0.000]	[0.000]	[0.029]		[0.000]	[0.000]	[0.056]
Panel B - (Annual) Health Care Measures, 2015								
Total Health Care Spending ($) [b]	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		14.78	10.91	9.92	
		[0.333]	[0.018]	[0.165]		[0.059]	[0.008]	[0.466]
Weighted Total Number of Visits and Days	5,932	4,102	3,530		6,902	4,151	3,479	
		[0.126]	[0.024]	[0.458]		[0.068]	[0.012]	[0.457]
Number of Chronic Conditions	6.21	5.55	5.27		6.54	5.43	5.37	
		[0.093]	[0.006]	[0.386]		[0.019]	[0.005]	[0.879]
Panel C - Demographics								
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 [a]	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 [a]	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	
		[0.025]	[0.018]	[0.743]		[0.011]	[0.027]	[0.380]
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.