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Essays on Participation in the Social Safety Net

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Essays on Participation in the Social Safety Net

by

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Abstract

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This dissertation studies the role of information and psychological barriers to participation in three means-tested programs: emergency rental assistance (ERA), the Housing Choice Voucher (HCV) program, and the Supplemental Nutrition Assistance Program (SNAP). A growing literature on administrative burden documents three distinct costs that contribute to take-up gaps across the social safety net: learning (or information), compliance, and psychological costs. In parallel, a large body of empirical research focuses on designing and testing light-touch behaviorally-informed methods of reducing these barriers. Yet, these studies have yielded mixed results, suggesting that there is much left to understand about when, why, and for whom such methods work. This dissertation directly contributes to both of these literatures by extending the application of the administrative burden framework, examining all three dimensions of administrative burden, and testing behaviorally-informed methods of reducing information and psychological barriers.

In Chapter 1, my co-author and I study the role of stigma as a barrier to take-up of emergency rental assistance. ERA programs aim to help low-income renters pay off back-owed rent and avoid eviction. In the wake of the COVID-19 pandemic, counties across the US received an unprecedented influx of federal funding for ERA programs. But many had trouble disbursing these funds to residents who needed assistance.

In two randomized experiments (N = 53,544; N = 62,528), we test whether reducing the stigma associated with ERA increases program take-up. In Austin, TX, we find that a destigmatizing email increases engagement by 36% relative to an information only email. We then build on these findings in a second field experiment in Denver, CO. In a mail-based outreach campaign, we find that a destigmatizing message significantly increases applications for rental assistance by 38% compared to a no-communication control group, and directionally by 11% relative to an information only communication. Moreover, we find suggestive evidence of larger effects for Black and Hispanic renters. In two subsequent online studies exploring mechanisms (total N = 1,258), we demonstrate that the destigmatizing language used in the outreach materials significantly reduces the internalized shame associated with participation in ERA, even in the presence of pervasive societal stigma.

In Chapter 2, I extend the administrative burden framework to consider the supply of services, rather than just demand for programs, and examine the barriers to landlord participation in the Housing Choice Voucher program in Minneapolis, MN through three methods: a large-

scale survey, a randomized experiment, and a survey experiment. The HCV program provides very low-income individuals and families with vouchers (i.e., subsidies) to afford housing on the private rental market. This program is unique in that it requires participation both from residents and landlords. Landlords' willingness to rent to tenants with vouchers directly determines both how many units are available, and where these units are located. Despite the centrality of landlords to the success of the program, there is relatively little empirical evidence on the barriers they face to participation, nor on effective methods for reducing them.

Existing evidence and narratives focus largely on the role of compliance hurdles—especially mandatory inspection requirements and time to lease-up—in deterring landlords from participating in the HCV program. Yet, in a survey conducted among all active landlords in Minneapolis ($N \sim 15{,}000$; response $N = 1{,}088$), I find evidence that psychological barriers, particularly the stigma associated with tenants who use vouchers, may be more consequential and pervasive than logistical barriers. In a field experiment ($N = 13{,}419$), I then test the impact of light-touch outreach that aimed to reduce these barriers to participation. Although outreach did not significantly affect landlord interest in the HCV program, in a subsequent survey experiment ($N = 655$), the same outreach materials had a directional impact on interest. This suggests the need for additional research to better understand the potential for light-touch strategies to increase landlord engagement.

In Chapter 3, I test the role of communication modality and message in increasing take-up of California's SNAP program, CalFresh, among likely eligible college students ($N = 275{,}977$). Food insecurity among college students has increased significantly in recent years and is linked to a wide range of adverse health and education outcomes. However, just 20 to 30 percent of food insecure college students participate in the Supplemental Nutrition Assistance Program, in part because traditional eligibility criteria exclude most students. In response to the COVID-19 pandemic, SNAP eligibility was temporarily simplified and expanded. In turn, millions of low-income college students became newly eligible for benefits, which offered an opportunity to test the impact of targeted informational outreach on benefits take-up among a population that may face uniquely high barriers to participation.

In a large-scale randomized experiment, I find that simplified messaging increased application rates by 0.2 percentage points (pp), or 7%, relative to a status quo outreach message, but additional language tweaks aimed at reducing potential psychological costs associated with CalFresh participation had no impact beyond the simplified message. At the same time, I find that multimodal outreach (email and postcard) nearly doubled application rates compared to outreach conducted via email alone.

Contents

Chapter 1

Destigmatizing Emergency Rental Assistance: Two Field Experiments

(With Elizabeth Linos)

1.1 Introduction

Means-tested government programs lift millions of Americans out of poverty and have long-term economic and health benefits (Bailey et al., 2020; CBPP, 2016). Yet, despite clear evidence of net benefits for those who participate, 20 to over 50 percent of households do not utilize programs for which they are eligible (Bhargava & Manoli, 2015; Blumenthal, Erard, & Ho, 2005; FNS, 2020; Giannarelli, 2019). These take-up gaps stem, in part, from high information costs and logistical hurdles that can deter participation, especially among the most vulnerable (Currie, 2004; Finkelstein & Notowidigdo, 2019; Herd & Moynihan, 2019; Heinrich, 2016; Ray, Herd, & Moynihan, 2022).

This study focuses on an often cited, but understudied psychological hurdle that may also contribute to these take-up gaps: the stigma around poverty and government assistance. We define stigma as a social construct that can result in social rejection, devaluation, and discrimination based on a given attribute, identity, or behavior (Dovidio, Major, & Crocker, 2000; Goffman, 1963; Major & O'Brien, 2005). Extant literature documents a pervasive stigma around poverty in the US. There are widespread stereotypes that people living in poverty are lazy, undeserving, lacking ambition and a work ethic, and even morally inferior (Lauter, 2016; Mead, 2019). This stigma originates from societal beliefs about the causes of poverty and norms of deservingness and is also highly racialized and gendered (Brown-Iannuzzi et al., 2017; Feagin, 1975; Federico, 2004; Gilens, 1999; Kluegel & Smith, 1986; Moffitt, 1983; Watkins-Hayes & Kovalsky, 2016). Counterproductively, participation in the very programs that aim to lift people out of poverty is often stigmatized over and above poverty itself (Baumberg, 2015; Stuber & Schlesinger, 2006; Williamson, 1974). For example, poor people who receive government assistance are more likely to be seen as lazy and undeserving of help than poor people who do not participate in benefit programs (Cook & Barrett, 1992; Iyengar, 1990).

Despite this large literature, there is limited empirical evidence on whether the stigma associated with government assistance causally influences take-up behavior, nor on effective methods for reducing stigma. The few existing studies in this area have yielded mixed results. Bhargava and Manoli (2015) found that targeting one potential source of stigma associated with the Earned Income Tax Credit (EITC) through government letters did not meaningfully increase take-up. However, as the authors note, the EITC is not generally as highly stigmatized as other government programs. In the context of the Supplemental Nutrition Assistance Program (SNAP), which is traditionally one of the most highly stigmatized government programs, Schanzenbach (2009) found that individuals were about 30% more likely to express interest in learning about the program when it was called a "benefit transfer" as opposed to when it was called "food stamps," the stigmatized status quo. But it is unknown whether this translated to an increase in actual benefits take-up, nor whether this effect was driven by a reduction in perceived stigma or by some other mechanism.

We contribute to this literature by testing whether subtle changes to the framing of government rental assistance can reduce the stigma associated with the program and increase

take-up. Housing assistance is a relevant test case for this research because it is both a central component of the social safety net and is more stigmatized than many other means-tested programs. In a pre-registered (https://osf.io/surhm/) online pilot study of low-income Americans (N = 493), we found that the stigma associated with participation in rental assistance is significantly higher than Medicaid, for instance, and is similar to the level of stigma associated with having a mental illness—a highly stigmatized attribute that is the subject of much of the existing literature on stigma. As such, we hypothesized that stigma may pose a significant barrier to take-up of rental assistance benefits for many eligible low-income individuals. Crucially, the early stages of the COVID-19 pandemic created a unique opportunity to test this. The demand for rental assistance typically far exceeds supply. But rental assistance programs saw an unprecedented influx of pandemic-relief funds in early 2021 (CRS, 2021). Despite the fact that the number of renters who were behind on rent skyrocketed as a result of the pandemic, a large take-up gap emerged whereby many states and counties found it difficult to get assistance to the renters who needed it most (Benfer et al., 2020; Dougherty, 2020; Narayanswamy et al., 2021).

To test the relative role of stigma as a barrier to take-up of rental assistance, we draw on research from other policy areas to identify two distinct channels through which it could affect the decision-making of prospective beneficiaries: anticipated and internalized stigma (Bos et al., 2013; Fox et al., 2018). Specifically, we define anticipated stigma as expectations of being the target of prejudice, discrimination, or negative stereotypes because of one's association with a public benefits program. We define internalized stigma as the process through which beneficiaries or prospective beneficiaries of government assistance internalize the negative stereotypes and beliefs held by society, which can manifest as shame, poor self-efficacy, low self-esteem and self-worth, or disempowerment. We posit that both anticipated and internalized stigma can affect willingness to participate in anti-poverty programs such as rental assistance. On the one hand, prospective beneficiaries may choose to not participate in available programs if they anticipate that they will be stereotyped or discriminated against as a result. On the other hand, prospective beneficiaries may decide to not participate due to a sense of shame in being associated with this stigmatized group.

In two randomized field experiments (N = 117,073) conducted in two US cities, we designed and tested a communication intervention that targeted the anticipated and internalized stigma associated with emergency rental assistance programs. In Study 1, conducted in Austin, Texas, we found that destigmatizing outreach delivered via email increased engagement with the city's rental assistance application by 36% relative to providing information alone. We conceptually replicate this using a mail-based intervention in Study 2, which was conducted in Denver, Colorado. Study 2 also extends these findings by measuring the intervention's impact on actual take-up behavior. We found that the destigmatizing outreach delivered via mail increased application requests by 79% compared to a no-communication control, and 18% compared to providing information alone, although this difference was not quite statistically significant. The same effects emerge with downstream take-up outcomes: households that received the destigmatizing outreach were significantly more likely to submit their application and receive assistance than households in the control group, and directionally more likely than households receiving information alone. Taken together, the results confirm the existence of large informational barriers to take-up and point toward the relative role of stigma as a psychological hurdle over and above logistical and informational burdens. We also find suggestive evidence that stigma may be a more consequential barrier for renters of color. In two subsequent online experiments (N = 1,258) to test mechanisms, we confirm that the destigmatizing language

reduces the internalized stigma felt by low-income households, without shifting societal stigma or beliefs about the program as a whole.

This paper makes three main contributions. First, it contributes to the evidence base on the causal impact of stigma on decision-making. In other policy areas, stigma has been associated with a range of behaviors, including medication adherence (Rao et al., 2007; Rintamaki et al., 2006), motivation to exercise (Vartanian & Novak, 2011; Vartanian & Shaprow, 2008), academic performance (Brown & Lee, 2005), financial decisions (Gladstone et al., 2021) and help-seeking (Eisenberg et al., 2009; Jennings et al., 2015). However, a majority of research to date has been correlational. Second, this research directly contributes to a growing literature on the role of administrative burdens in understanding take-up gaps. The impact of information and logistical barriers has been well-documented (e.g., Bertrand, Mullainathan, & Shafir, 2006; Chetty, Friedman, & Saez, 2013; Smeeding, Phillips, & O'Connor, 2000; Finkelstein & Notowidigdo, 2019). Yet, evidence from behavioral interventions aimed at reducing these barriers has been mixed, which has left many open questions about when, why, and for whom interventions are effective (e.g., De La Rosa et al. 2021; Bird et al., 2021; Linos et al., 2022). This research suggests that designing interventions aimed at reducing information and logistical barriers may require a more conscious focus on relevant psychological barriers if they are to effectively improve delivery of stigmatized programs. Last, these findings contribute to the broader literature on housing policy and eviction prevention during COVID-19 and beyond (Benfer et al., 2022; Collinson, Ellen, & Ludwig, 2019; Keene et al., 2021). Ultimately, the findings reported in this paper have direct and timely implications for policymakers who face a dual challenge of increasing overall take-up of critical safety net programs, while also ensuring equity in access and delivery.

1.2 Study 1: Austin, TX

Experimental Design and Data

In Study 1, we partnered with the City of Austin's Housing and Planning Department to conduct an email-based outreach campaign to 54,544 residents whose email addresses were part of a listserv maintained by the city. We have no individual-level data on any of the 53,544 residents included in this sample. The city administered both the email and experiment.

The randomization was conducted through the city's email marketing platform, which has built-in A/B testing capability. Half of the residents (N = 27,272) received an *Information Only* email (see Figure 1.1). The other half of the residents (N = 27,272) received an *Information + Stigma* email that provided the same information as the *Information Only* email, but included subtle language changes to target two potential sources of stigma associated with program participation. First, as shown in Figure 1.1, language targeted internalized stigma by emphasizing that it was no one's fault if they were struggling to pay their rent and, in fact, many Austin residents may have needed extra help because of the COVID-19 pandemic. Second, language in the *Information + Stigma* email highlighted that the program was intended to help all eligible Austin residents get the assistance they deserved, and minimized the salience of the selection process. This language targeted anticipated stigma by aiming to reduce prospective beneficiaries' fear or expectations of discrimination and prejudice.

The analytic universe consisted of all 54,544 emails included in this study. Our primary outcomes of interest were (1) total click-throughs on the six embedded links to the rental

assistance program application website; and (2) total click-throughs on any embedded link in the email. Click-throughs were measured by the email marketing platform and provided to the research team via an aggregate report two weeks after the emails were sent.

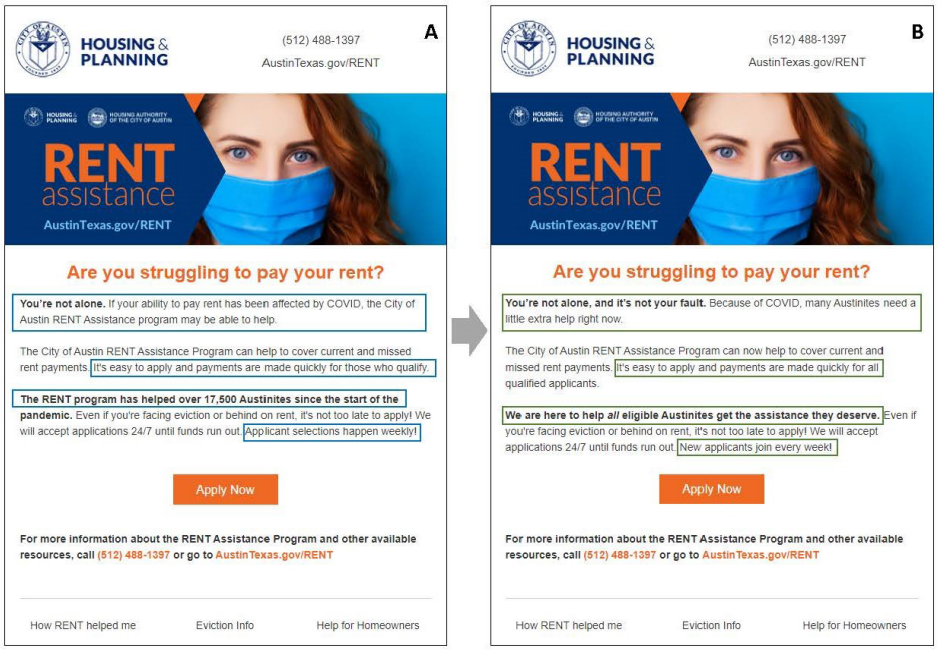
Results

Because we have no individual-level data on the individuals or behavior associated with any of the email addresses, we evaluated differences in our two primary outcomes via a two-sample proportions test. Each email included eleven total links, six of which directed recipients to the Austin rental assistance application web page. Overall, 2.7% of *Information + Stigma* email recipients clicked on one of the rental assistance application links, compared to 2.0% of *Information Only* email recipients ($z = 5.45$, $p < .001$, 95% CI [0.46, 0.97]).

The *Information + Stigma* email also generated higher overall engagement: 3.0% of recipients who received the *Information + Stigma* email clicked on any link in the email, compared to 2.2% of recipients who received the *Information Only* email ($z = 5.87$, $p < .001$, 95% CI [0.53, 1.07]).

Overall, these findings suggest that destigmatizing language increases engagement with outreach beyond providing *Information Only*. However, while the *Information + Stigma* email yielded significantly more interest in the rental assistance program as measured by click-throughs, we do not have the ability to measure whether this translated into increased applications for Austin's rental assistance program. Additionally, because this study did not include a control group, we are unable to assess the effect of providing information by itself. We address these limitations directly in Study 2.

Figure 1.1. Study 1 materials



Notes: (A) *Information Only* email; (B) *Information + Stigma* email. Boxes highlight language that was reframed in the *Information + Stigma* email to target internalized and anticipated stigma associated with temporary rental assistance. The *Information + Stigma* email yielded a 36% increase in click-throughs to the application website.

1.3 Study 2: Denver, CO

Experimental Design and Data

In a pre-registered (https://osf.io/5w7tj) randomized experiment conducted in partnership with Denver County's Department of Housing Stability and Office of Social Equity and Inclusion, we designed and evaluated a mail-based communication intervention that aimed to connect eligible renters with the County's temporary rental assistance program. Denver County is divided into 78 distinct neighborhoods and 144 census tracts. We identified 56 neighborhoods and 106 census tracts with populations at high risk of displacement through a four-step process described in Appendix A.

The final sample universe included 106 census tracts in 56 neighborhoods. We then constructed our experimental universe using publicly available parcel data from Denver County, which included address information for every residence and building in the County. For all 56 neighborhoods in the final sample universe, we identified presumed renter households as addresses for which the parcel owner address did not match the parcel site address, suggesting that the owner was not living at his/her own property. All addresses were then validated using the US Postal Service's National Change of Address (NCOA) database (US Postal Service, n.d.). All invalid addresses were excluded from the experimental universe prior to randomization. The final experimental universe consisted of 62,715 presumed renter households in the 56 sample neighborhoods.

In a stratified randomization, all renter addresses were randomly assigned to one of three experimental conditions. The *Control* group received no communication as part of this study, although they may have received information about the program through other channels. Renters assigned to the *Information Only* group were sent a postcard that provided clear and simple information about Denver County's rental assistance program and instructions for applying (see Appendix A). Renters assigned to the *Information + Stigma* group were sent the same postcard as in the *Information Only* group, but with subtle language changes to target potential sources of anticipated and internalized stigma associated with program participation. Language was similar to the language used in the *Information + Stigma* group in Study 1. All information was provided in English and Spanish, and language aligned with the County's status quo communications.

The randomization was stratified by neighborhood and service area for the three nonprofit agencies that were responsible for administering the County's rental assistance program. All outcome data used in this study came from the Denver County Department of Housing Stability, the three administering nonprofit agencies, and the Denver County Court. Our first outcome of interest, application requests, is defined as any request for a rental assistance application in the eight weeks after the mailing date (Dec. 10, 2020 to Feb. 5, 2021). In order to receive a rental assistance application, residents had to request an application by completing an online form or calling one of the three nonprofit organizations that were responsible for administering Denver County's rental assistance program. The largest of the three nonprofits maintained a record of all application requests, including applicant address and date of inquiry. We discovered belatedly that the other two nonprofits were not systematically tracking application requests. In this manuscript we thus evaluate application requests for the subpopulation served by the largest of the three nonprofit organizations (N = 25,229). Because the randomization was stratified by nonprofit organization, limiting the sample in this way only

affects statistical power—it does not affect the validity of our estimates. Results for the full analytic universe are presented in Appendix A (Table A2).

The second outcome of interest, submitted applications, is defined as submission of a rental assistance application to one of the three administering nonprofit agencies in the eight weeks following the mailing date (Dec. 10, 2020 to Feb. 5, 2021). Each nonprofit organization tracked all households that submitted a rental assistance application, including the date of submission.

Finally, the third outcome of interest, assistance received, is defined as receipt of rental assistance funds in the eight weeks following the mailing date (Dec. 10, 2020 to Feb. 5, 2021). These data were tracked at the address level and came from administrative records maintained by the Department of Housing Stability.

Empirical Strategy

In an intent-to-treat analysis, we first evaluated the average effect of treatment assignment via the following regression specification:

$$(1)\ Y_{is} = \alpha + \tau_1 T_{1is} + \tau_2 T_{2is} + X_s + \gamma_s + \delta_n + \varepsilon_{is}$$

where Y_{is} is the outcome of interest for household *i* in neighborhood *s*; τ_1 and τ_2 are the coefficients of interest on the treatment indicators T_1 and T_2, which correspond to *Information Only* and *Information + Stigma*, respectively; X_s is a vector of neighborhood-level covariates, including the poverty rate, percent non-White residents, percent of rent-burdened residents, and median gross rent; γ_s are neighborhood fixed effects; and δ_n are nonprofit agency fixed effects. The neighborhood-level covariates came from publicly available data from the Eviction Lab and the Urban Institute. The specification reports robust standard errors.

We report both logistic and linear estimates of equation (1), but we preference results from the linear specification. Because the outcomes of interest were relatively rare, six neighborhoods saw no positive outcomes, leading them to be excluded from the covariate-adjusted logistic specification. In addition, we evaluated the impact of random assignment on each outcome of interest using randomization inference based on Fisher's exact test to test the sharp null hypothesis of no effect of assignment to treatment. These results are reported in Appendix A and do not differ meaningfully from those reported in this manuscript.

Prior to obtaining any information on outcomes, we pre-registered an analysis plan at the Open Science Framework (https://osf.io/5w7tj). The final analytic universe excludes 186 addresses that were randomized, but later found to be duplicates due to discrepancies in the NCOA validation process. These addresses represent just 0.3% of our experimental universe and excluding them does not affect our final results. The final analytic universe is thus comprised of 62,529 unique renter households.

Results

Application Requests

Table 1.1 shows results on our first outcome of interest, requests for rental assistance applications in the eight weeks following outreach among the subset of the analytic universe

associated with the largest administering nonprofit agency (N = 25,229). We find that random assignment to either treatment condition significantly increased application requests by 0.6 percentage points (pp) relative to the no-mailer *Control* group ($p < .001$, 95% CI [0.28, 0.91]). On average, 0.9 percent of households in the *Control* group requested an application during the eight weeks following the mail date (all means regression-adjusted; SE = 0.14), compared to 1.5 percent of households in the pooled treatment conditions (SE = 0.09), an increase of 65%.

Evaluating each condition separately, 1.4 percent of households in the *Information Only* group requested an application (SE = 0.12), reflecting an average treatment effect (ATE) of 0.47 pp (52%) relative to the *Control* group (p = .008, 95% CI [0.12, 0.82]). Meanwhile, 1.6 percent of households in the *Information + Stigma* group requested an application (SE = 0.13), reflecting an ATE of 0.72 pp (79%) relative to the *Control* group ($p < .001$, 95% CI [0.36, 1.08]), and an ATE of 0.25 pp (18%) relative to the *Information Only* group (p = .15, 95% CI [-0.09, 0.58]).

Submitted Applications

Next, we find that assignment to either treatment condition increased submitted applications for rental assistance by 0.16 pp relative to the no-mailer *Control* group (p = .03), an increase of 30% (see Table 1.1). On average, 0.53 percent of households in the *Control* group submitted an application in the six weeks following the mail date (SE = 0.07), compared to 0.66 percent of households in the *Information Only* group (SE = 0.05). This difference of 0.13 pp (25%) is not significant at standard levels (p = .13, 95% CI [-0.04, 0.29]). Meanwhile, 0.73 percent of households in the *Information + Stigma* condition (SE = 0.05) submitted an application during the outcome period, reflecting a significant ATE of 0.20 pp or 38% relative to the *Control* group (p = .02, 95% CI [0.03, 0.36]). Compared to the *Information Only* group, the *Information + Stigma* mailer yielded a 0.07 pp (11%) increase, although this difference was not significant (p = .34, 95% CI [-0.07, 0.21]).

Assistance Received

Once renters submitted an application for rental assistance, program staff had to verify their information before approving the disbursement of assistance. We did not pre-register assistance received as a primary outcome due to initial uncertainty about whether data on this measure would be available. Ultimately, however, we did receive data on renters who received assistance between the start of our intervention in December 2020 and April 2021. We thus explore the impact of treatment on assistance received using equation (1).

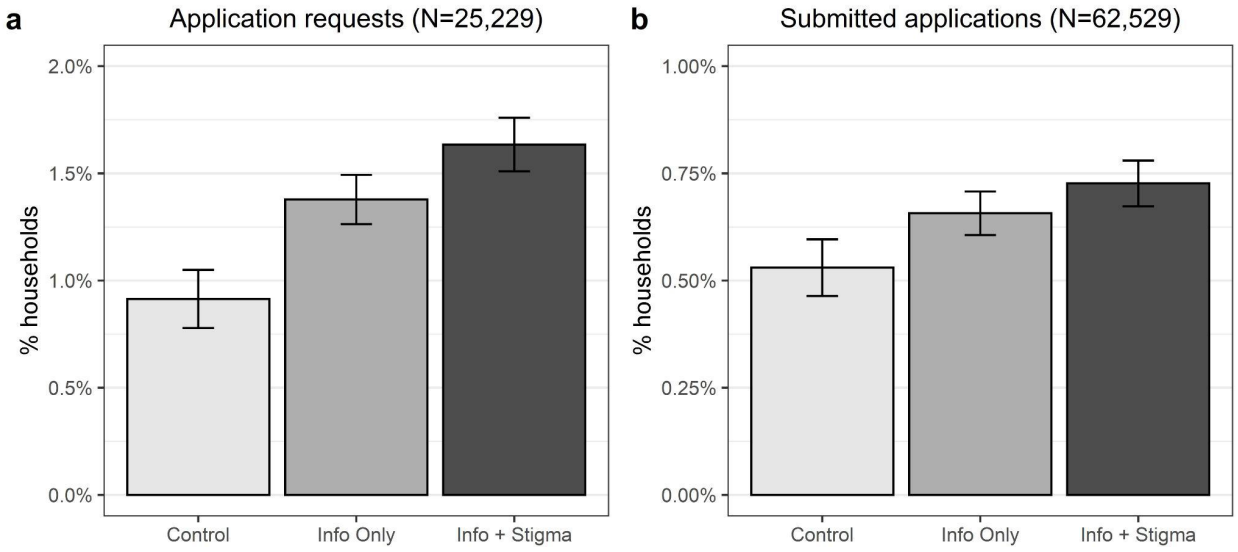
As shown in Appendix A (Table A5), we find that the increase in take-up driven by the mailers also translated into an increase in funds received. Renters assigned to the *Information Only* group were 0.19 pp more likely to receive assistance between December 2020 and April 2021 than renters assigned to the *Control* group (SE = 0.07, p = .01, 95% CI [0.04, 0.33]). Renters assigned to the *Information + Stigma* group were 0.04 pp more likely to receive assistance than renters assigned to the *Information Only* group (SE = 0.05, p = .49, 95% CI [-0.08, 0.16]) and 0.23 pp more likely to receive assistance than renters assigned to the *Control* group (SE = 0.07, p = .002, 95% CI [0.08, 0.37]).

Table 1.1. Study 2 results

	Application Requests		Submitted Applications	
	Logistic	OLS	Logistic	OLS
	(1)	(2)	(3)	(4)
Information Only	0.4204*	0.0047**	0.2134	0.0013
	(0.1723)	(0.0018)	(0.1479)	(0.0008)
Information + Stigma	0.5886***	0.0072***	0.3160*	0.0020*
	(0.1694)	(0.0018)	(0.1461)	(0.0009)
Treatment pooled	0.5077**	0.0060***	0.2659	0.0016*
	(0.1606)	(0.0016)	(0.1367)	(0.0008)
Observations	24,564	25,229	60,394	62,528
Control mean	0.00944	0.00914	0.00550	0.00530

Notes: Estimates of the average effect of treatment assignment on application requests (Columns 1-2) and submitted applications (Columns 3-4) in the eight weeks following the mailing date. The sample for Columns 1-2 is all addresses associated with the administering nonprofit organization that tracked all incoming application requests (*N* = 25,229). Appendix A reports results on application requests for the full analytic universe. The sample for Columns 3-4 is the full analytic universe. Observations excluded from logistic models due to collinearity of neighborhoods and the outcome of interest. Controls include percent rent burdened, percent non-White, poverty rate, fixed effects for neighborhood, nonprofit organization, and an indicator for whether the address was part of an apartment building. Robust standard errors in parentheses. *** p<0.001, ** p<0.01, * p<0.05

Figure 1.2. Study 2 results



Notes: Bars represent the percent of households that requested an application (a) and submitted an application (b) during the eight-week outcome period. Error bars reflect +/- 1 SE.

Overall, providing information about rental assistance benefits increased take-up, as measured by submitted applications, by 25% compared to providing no information. This confirms existing evidence that information and learning may be consequential barriers to take-

up in some contexts. But these findings also suggest that destigmatizing the language used in outreach may further increase take-up relative to providing information alone. In Study 1, the *Information + Stigma* message increased engagement by 36% relative to the *Information Only* message—a highly significant increase. In Study 2, the *Information + Stigma* message increased take-up by an additional 11% compared to *Information Only*, although this difference was not statistically significant. However, it is worth noting that our minimum detectable effect for the pairwise comparison between treatment conditions in Study 2 is 0.2 pp—larger than the observed 0.07 pp effect.

Heterogeneous Effects by Race and Ethnicity

Given that psychological burdens may disproportionately affect some subsets of eligible households, we explore whether the main treatment effect varies by socioeconomic status (SES) and by race and ethnicity.

Data availability on household-level socioeconomic characteristics is limited. However, we use data from the Eviction Lab to consider whether treatment effects differ by census tract-level SES. Specifically, in separate specifications, we interact household-level treatment assignment with continuous measures of census tract-level median household income, poverty rate, and the percentage of households that are rent burdened. We do not find evidence of heterogeneous treatment effects by any measure of SES.

Given the racialized nature of stigma associated with government assistance (e.g., Cohen-Cole & Zanella, 2006), we also explore effects by race and ethnicity in two ways. First, we test heterogeneous effects by census tract-level demographics, specifically the percent of the population that is Black/African-American or Hispanic. We find that the effect of assignment to the *Information + Stigma* mailer on requests for applications is significantly larger in census tracts with higher proportions of minority residents ($F(1,62460) = 7.87$, $p = .005$).[1] The effect of assignment to the *Information Only* condition on application requests does not differ by proportion of minority residents in the census tract ($F(1,62460) = 1.16$, $p = .28$), though the overall interaction between percent minority residents and treatment is significant ($F(2, 62460) = 4.57$, $p = .01$). On submitted applications, we do not find a significant difference in the effect of treatment by proportion of minority residents at the census tract level ($F(2, 62460) = 1.21$, $p = .30$).

Second, we explore variation in the raw distribution of completed applications in condition by race and ethnicity. Household-level demographic information is available for about one-third of renters who submitted applications. We cannot evaluate heterogeneous treatment effects since we do not have demographic data for non-applicants. By extension, we also cannot assess the extent to which applicants differ from non-applicants. However, we observe no statistically significant differences between treatment groups in the likelihood of reporting race and ethnicity (see Table S8). Thus, if the randomization effectively created groups that were statistically similar on race and ethnicity, which we can observe to be the case in our subsample, any differences in the proportions of applicants by race and ethnic groups likely reflect an impact of the intervention.

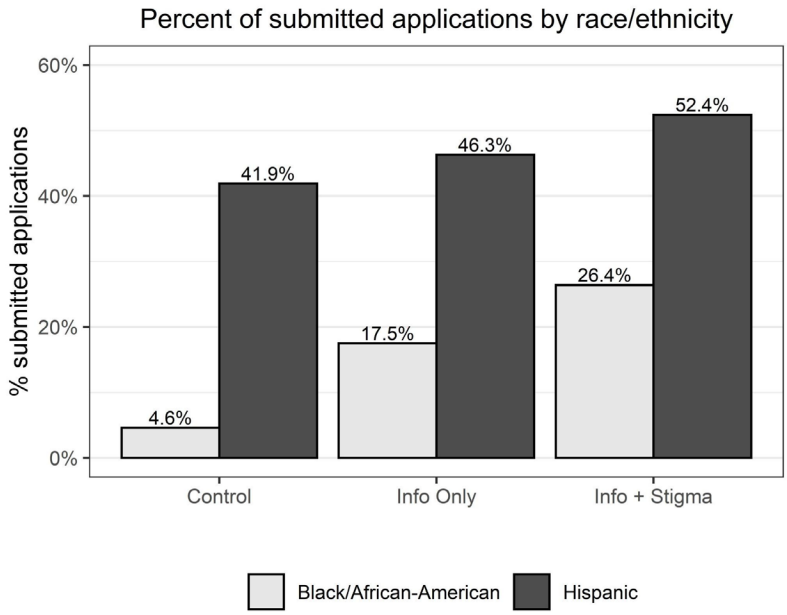
As shown in Figure 1.3, in the *Control* group, just 5% of submitted applications came from Black or African-American residents. In contrast, 17% of submitted applications from

[1] Because this analysis is exploratory, here we examine application requests across all three administering nonprofit agencies in order to increase power to detect interaction effects.

households that were sent the *Information Only* postcard and 26% of submitted applications from households that were sent the *Information + Stigma* postcard came from Black or African-American residents ($\chi^2(2) = 5.01$, $p = .07$). Similar, but smaller, differences can be seen in the proportion of submitted applications by ethnicity: 42% of submitted applications in the *Control* group came from Hispanic renters, compared to 46% in the *Information Only* condition and 52% in the *Information + Stigma* condition ($\chi^2(2) = 1.19$, $p = .55$).

This analysis is exploratory and should be interpreted with caution given that it is based on data from a small number of program applicants. But even still, the large distributional differences found in submitted applications by race across conditions, as well as the differences in treatment effects by census-tract level demographics, point to potentially interesting and important directions for future study.

Figure 1.3. Study 2, distribution of submitted applications by race and ethnicity



Notes: Bars represent the raw percent of households that submitted an application, by race and ethnicity, during the eight-week outcome period.

1.4 Studies 3 and 4: Exploring Mechanisms

Studies 1 and 2 offer evidence that the *Information + Stigma* intervention may yield gains in take-up beyond providing information alone. The nature of the field experiments, however, does not allow us to directly measure whether the intervention actually reduced anticipated or internalized stigma. It is possible that the *Information + Stigma* communications in both field experiments were more effective than the *Information Only* communications because they changed some other aspect of recipients' perceptions of the program. For instance, if the *Information + Stigma* communications led recipients to believe the program was easier to apply for, or that they would be more likely to receive funds should they apply, those changes in

perceptions could have affected take-up through a different channel. In fact, if perceptions about the difficulty of applying for a program is a significant barrier to take-up, which we may expect given previous literature (Herd & Moynihan, 2019), changing beliefs about these compliance costs may affect take-up even without reducing stigma. We disentangle these mechanisms in two pre-registered online studies conducted via Amazon Mechanical Turk (MTurk).

Experimental Design and Data

Study 3

In Study 3, a sample of 832 participants (mean age = 38.3 years, *SD* = 11.2; 39.7% female) with a household income less than $50,000 per year were recruited through MTurk to complete a 2-minute online survey for which they were paid $0.50 each. After relevant data quality exclusions (see Appendix A), balanced evenly across treatment conditions ($\chi^2(2) = .08$, $p = .78$), our final analytic sample consisted of 622 participants (mean age = 39.1 years, SD = 11.4; 43% female).

All participants who consented to participate and passed an initial attention check were randomly assigned by the survey software (Qualtrics) to see a redacted version of either the *Information Only* or *Information + Stigma* mailer from Study 2. Participants were then asked eight questions to measure the internalized and anticipated stigma associated with the temporary rental assistance program in order to allow us to measure whether the mailers affected these two distinct stigma constructs. All questions were presented in a random order and measured on a 7-point Likert scale. Participants were also asked about their perceptions of the difficulty of the application process, as well as their likelihood of applying for the program. See Appendix A for exact question text.

We constructed three indices as our primary outcomes: overall stigma, anticipated stigma, and internalized stigma. Each is calculated as the equal-weighted average of their respective measures. In addition, we measure (1) participants' reported likelihood of applying for the rental assistance program on a 7-point scale, where 7 reflects "extremely likely to apply"; and (2) their perceptions of the difficulty of the application process on a 10-point scale where 10 reflects "extremely difficult to apply." We also construct a binary indicator for likelihood of applying for rental assistance, defined as a response of 5 ("somewhat likely") or higher on the 7-point Likert scale.

Study 4

Study 4 participants were 791 MTurk workers (mean age = 39.8 years, *SD* = 12.9; 49.2% female) whose reported household income was below $50,000 per year and who were recruited to complete a 1-minute online survey for which they were paid $0.30 each. Standard participant qualifications were applied (see Appendix A). After relevant data quality exclusions, balanced evenly across treatment conditions ($\chi^2(2) = .08$, $p = .78$), our final analytic sample consisted of 636 participants (mean age = 40.7 years, *SD* = 13.3; 53% female).

All participants who consented to participate and passed the initial attention check were again randomly assigned everyone to see either the *Information Only* or *Information + Stigma* mailer from Study 2. We then asked about perceptions of (1) their perceptions of the difficulty of the application process; (2) the credibility of the mailer; and (3) their expectations of the

likelihood of receiving money if they applied. They were also asked a comprehension question to assess whether they read and understood the postcard. See Appendix A for exact question text. Our primary outcomes for Study 4 were participants' perceptions of the difficulty of the application process, which was measured on a 10-point scale where 10 reflects "extremely difficult to apply;" perceptions of the likelihood of receiving money, measured on a 5-point scale in which a 5 reflects "very likely to receive money;" and credibility of the postcard measured on 5-point scales in which a 5 reflects "very credible."

Empirical Strategy

For both Studies 3 and 4, we evaluated the average impact of assignment to the *Information + Stigma* condition through the following linear model:

$$(2)\ Y_i = \alpha + \tau_1 T_{1i} + X_i + \varepsilon_i$$

where Y_i is the outcome of interest for participant i; τ_1 is the coefficient of interest on the treatment indicator T_1, which corresponds to assignment to the *Information + Stigma* condition; X_i is a vector of individual-level covariates, including gender, age, a binary indicator for college education, race, income, party affiliation, housing insecurity, and prior experience utilizing rental assistance. The specification reports robust standard errors.

Results

Study 3

All hypotheses and analyses were pre-registered on OSF (https://osf.io/6pxw4). As hypothesized, overall stigma associated with the rental assistance program, calculated as the average of all eight stigma measures, was significantly lower among participants who saw the *Information + Stigma* mailer than those who saw the *Information Only* mailer ($F(1, 603) = 4.46$, $p = .04$, 95% CI [-0.46, -0.02]). This difference appears to be driven by a reduction in internalized stigma. As shown in Table 1.2, internalized stigma among participants who saw the *Information + Stigma* mailer was 0.3 points or 8% lower than among participants who saw the *Information Only* mailer ($F(1, 603) = 5.62$, $p = .02$, 95% CI [-0.55, -0.05]). We see a similar, but smaller and non-significant difference between conditions on anticipated stigma. Anticipated stigma was 0.2 points or 5% lower among participants who saw the *Information + Stigma* mailer than those who saw the *Information Only* mailer ($F(1, 603) = 2.34$, $p = .12$, 95% CI [-0.41, 0.05]). A power analysis reveals that the minimum detectable effect in this experiment is 0.3 points on a 7-point scale. As such, it is possible that we are slightly underpowered to detect significant differences between the two conditions on anticipated stigma. But at a minimum, these findings suggest that our intervention does, in fact, shift feelings of internalized stigma.

We find a small, but non-significant difference across conditions in reported likelihood of applying for the program: 75.0% of participants who saw the *Information + Stigma* mailer reported being at least somewhat likely to apply, compared to 72.2% of participants who saw the *Information Only* mailer ($F(1, 603) = 0.66$, $p = 0.42$, 95% CI [-0.04, 0.10]). However, with a sample of 622 online participants, we are underpowered to detect differences smaller than 9 pp.

Importantly, there was no difference between conditions in perceptions of the ease of applying for the rental assistance program ($F(1, 603) = 1.14$, $p = .29$, 95% CI [-0.17, 0.57]).

Study 4

Study 4 builds on these findings by testing and ruling out other potential explanations for the observed differences in effect between the two mailers. All hypotheses and analyses were pre-registered on OSF (https://osf.io/m56nh).

Overall, 77% of participants in the *Information Only* group and 80% of participants in the *Information + Stigma* group correctly answered the comprehension question ($\chi^2(1) = 1.17$, $p = .28$). As in Study 3, we find no difference across conditions in perceived difficulty of the application process ($F(1, 617) = 0.43$, $p = .51$, 95% CI [-0.49, 0.25]). Similarly, there is no difference across conditions in perceptions of the likelihood of receiving money ($F(1, 617) = 1.35$, $p = .25$, 95% CI [-0.07, 0.29]). However, participants who saw the *Information + Stigma* mailer found the mailer to be less credible than participants who saw the *Information Only* mailer ($F(1, 617) = 4.00$, $p = .05$, 95% CI [-0.35, 0.00]). The juxtaposition between this result and the findings of the field experiments points to one promising area for further research.

Combined, studies 3 and 4 provide suggestive evidence that the larger effects seen from the *Information + Stigma* mailer in the field experiment are being driven by a reduction in internalized stigma, as opposed to a change in how target beneficiaries understand the program.

Table 1.2. Study 3 and 4 results

	Study 3				Study 4			
	Overall stigma	Anticipated stigma	Internalized stigma	Likelihood of applying	Ease of applying	Likelihood of receiving	Credibility	Comprehe-nsion
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Info + Stigma	-0.239*	-0.178	-0.300*	0.028	-0.124	0.106	-0.177	0.032
	(0.113)	(0.116)	(0.127)	(0.035)	(0.188)	(0.091)	(0.089)	(0.029)
Observations	622	622	622	622	636	636	636	636
Mean for Info Only	4.470	4.615	4.325	0.722	5.709	3.053	3.396	0.770

Notes: Estimates of equation (2) in Study 3 (Columns 1-4) and Study 4 (Columns 5-8). Overall, anticipated, and internalized stigma (Columns 1-3) are equal-weighted indices, each measured on a 7-point scale in which 7 reflects high stigma. Likelihood of applying (Column 4) is a binary measure in which a 1 reflects a response of at least "somewhat" likely to apply for rental assistance. Ease of applying (Column 5) is measured on a 10-point scale in which 10 reflects "extremely difficult to apply." Likelihood of receiving money (Column 6) and credibility (Column 7) are both measured on 5-point scales in which 5 reflects "very likely" and "very credible," respectively. Comprehension (Column 8) is a binary measure in which a 1 reflects a correct answer to a question about what program the treatment was advertising. Controls include income, age, gender, college education, race, party, prior experience with housing insecurity, and prior experience using rental assistance. Robust standard errors in parentheses. *** p<0.001, ** p<0.01, * p<0.05

1.5 Discussion

Across four studies, we show that destigmatizing the language used in informational outreach about government rental assistance can yield gains over and above status quo language. A one-time communication that reduced the stigma associated with rental assistance increased engagement with the communication by 36% and program applications by 11% compared to an *Information Only* communication. These effects are practically meaningful: the most successful treatment arms yield effects that are 1.5 to 3 times larger than the average effect of a behavioral intervention (DellaVigna & Linos, 2022). Moreover, the destigmatizing outreach also yielded a large increase in the proportion of applications from Black and African-American renters, although these results are exploratory. Expanding on an expansive literature documenting the existence of societal stigma against low-income households who use government assistance, these studies offer causal evidence that stigma may be a meaningfully barrier to take-up of benefit programs, and demonstrate that internalized stigma can be shifted even in the presence of pervasive societal stigma.

This research also highlights a potentially serious and overlooked shortcoming of existing government outreach efforts around rental assistance. The language in the *Information Only* communication used in both field experiments was similar to the messaging found in status quo outreach from many of the largest cities and counties in the US. Our findings suggest that this status quo messaging may unintentionally and unknowingly reinforce the stigma associated with rental assistance participation, thereby affecting who ultimately benefits from the program.

The studies presented here also have a few important limitations that suggest directions for future work. First, while the *Information + Stigma* message significantly increased engagement with the communication relative to the *Information Only* message in Study 1, the differences between treatment conditions in Study 2 were not significant. This could reflect a lack of statistical power or a difference in context. In either case, the magnitude of the directional effect is large enough to warrant further study.

Second, the field experiments reported in this paper both leveraged an unprecedented influx of federal funding for emergency rental assistance programs in the wake of the COVID-19 pandemic. This offered a unique opportunity to study this traditionally oversubscribed, but also highly stigmatized program, but also raises questions about the generalizability of these findings outside the COVID-19 context. It is possible that the stigma associated with emergency rental assistance during covid is more malleable than historically stigmatized programs even in the same policy area, such as Section 8 vouchers. Further studies could test similar interventions on take-up of rental and utility assistance programs outside of a pandemic context, as well as on other traditionally stigmatized programs like SNAP or Temporary Assistance for Needy Families (cash assistance) to consider generalizability across the social safety net.

Third, while Studies 3 and 4 present evidence consistent with our theorized mechanism—namely that our interventions reduced stigma and, in particular, internalized stigma—there are other possible psychological mechanisms that could be explored. For instance, future studies should disentangle perceptions of stigma from beliefs about governments and government workers, and should assess the relationship between stigma and one's social identity, including self-esteem, beliefs about one's work ethic, and feelings of resilience and self-efficacy.

Finally, the intervention tested here solely targeted felt stigma among individual prospective beneficiaries. Future research should thus also explore methods of reducing societal stigma, especially among the frontline workers and landlords who are critical to the success of

government housing assistance programs. While targeting internalized stigma among low-income individuals may improve immediate take-up outcomes, a systems-wide approach is necessary to fully and enduringly destigmatize participation in government programs.

Chapter 2

Understanding and Reducing the Barriers to Landlord Participation in the Housing Choice Voucher Program

2.1 Introduction

Federal rental assistance programs are one of the largest and most effective anti-poverty programs in the US, lifting over three million people out of poverty every year (CBPP, 2021; Trisi, 2019). The largest rental assistance program, the Housing Choice Voucher (HCV) program, has been found to dramatically reduce homelessness and housing instability, and facilitate geographic and economic mobility (Fisher, 2015). Although a crucial part of the social safety net, this program is unique compared to other government benefits programs in that it requires participation from both landlords and low-income residents. Low-income individuals may face barriers to accessing the HCV program just as they do other government benefits. But unlike other benefits, the HCV program is chronically oversubscribed; most families that apply and are eligible wait years to receive a voucher (Acosta & Gartland, 2021).

One factor that influences these wait-times is landlords' willingness to accept tenants with vouchers. Landlord participation in the HCV program directly determines both how many units are available to voucher holders and where these units are located. Yet, despite the centrality of landlords to the success of HCV programs, there is little empirical evidence on effective methods of increasing their participation (Garboden et al., 2018). Using a large-scale survey (N = 1,088) that includes over 350 open-text responses, a field experiment (N = 13,419), and a survey experiment (N = 452), this paper documents the types of landlord (mis)perceptions that deter participation in the HCV program, and reports on the results of light-touch efforts to increase interest in the program.

A growing body of research on administrative burden categorizes three distinct costs that impact the *demand* for government assistance programs: learning costs, compliance costs, and psychological costs (Currie, 2004; Herd & Moynihan, 2019). Learning costs are the burdens of seeking out and collecting information about available resources and programs, and their eligibility and application requirements. Compliance costs arise from the process of applying for, using, and maintaining access to program benefits. This can include time spent waiting in line, onerous application procedures, in-person interviews, extensive documentation and verification of personal information, and so on. Psychological costs include the stigma, stress, frustration, or loss of personal autonomy that individuals may experience in the process of applying for or participating in a program. Extensive evidence has documented how these burdens affect take-up of social programs, especially among more vulnerable populations (Christensen et al., 2019; Heinrich, 2016; Herd & Moynihan, 2019).

I extend this framework to consider how administrative burdens can affect the *supply* of programs, specifically housing in the context of the HCV program. First, landlords must learn about HCV programs, their benefits, and associated eligibility and enrollment requirements (learning costs). Second, prospective landlords must be willing and able to navigate the application process, provide all required paperwork and documentation, and manage program requirements such as mandatory inspections (compliance costs). Qualitative research has found that many landlords choose not to participate in the HCV program because of cumbersome regulatory inspections, and the paperwork, bureaucracy, and lack of transparency involved in

applying for the program (Cunningham et al., 2018; NLIHC, 2018). Finally, landlords must overcome the stigma and stereotypes that are commonly associated with the HCV program, particularly around the tenants who participate (psychological costs). Landlords often view tenants with vouchers as worse-quality tenants than other renters in their market, and these perceptions can be a significant factor in their decisions to rent (or not) to voucher holders (Garboden et al., 2018; Cunningham et al., 2018; NLIHC, 2018). Although extant literature has qualitatively documented these barriers, there is limited empirical evidence on their relative impact on landlord decision-making, nor on effective strategies for reducing them.

This study helps close this gap by quantifying the relative role of different administrative barriers to landlord participation in the HCV program in Minneapolis, Minnesota, and by pilot testing the impact of light-touch behavioral communications on reducing these barriers in a large-scale field experiment. I find that psychological barriers in the form of pervasive stereotypes about tenants who use vouchers may have a larger impact on landlord decision-making than other barriers. Compliance hurdles, especially concerns about inspection and enrollment processes, also affect landlords' willingness to participate in the HCV program, but seemingly to a lesser extent. Light-touch outreach that aimed to isolate and reduce these barriers did not have a significant impact on landlord interest in the HCV program in a field experiment. Yet, in a subsequent survey experiment, the same outreach materials had a directional impact, suggesting the need for additional research to better understand the potential for light-touch strategies to increase landlord engagement.

This paper makes three main contributions. First, it contributes to ongoing housing policy conversations on the role of landlords and landlord-tenant relationships in shaping HCV program outcomes, as well as housing stability more broadly (e.g., Rosen & Garboden, 2022). While the logistical and compliance barriers to participating in the HCV program have been well-documented, this research suggests beliefs about tenants who use vouchers may be more influential in shaping landlords' decisions. Second, this research directly contributes to the evidence base on how administrative burdens affect the *supply* of social programs, a relatively understudied dimension of the social safety net and administrative burden framework. Conceptualizing the supply of housing in the HCV program as an administrative burden problem opens the door for future empirical research on isolating and reducing these barriers. Last, these findings contribute to the broader literature on behavioral public policy. A large body of research has examined the impact of light-touch behavioral interventions on motivating behaviors from voting to school attendance to saving for retirement (Gerber & Rogers, 2009; Karlan et al., 2016; Lasky-Fink et al., 2021). But research on the effectiveness of these types of interventions in the context of the social safety net focuses almost exclusively on demand-side policy solutions, rarely targets stigma or stereotypes, and has yielded mixed results (e.g., Finkelstein & Notowidigdo, 2019; Linos et al., 2022; Remler & Glied, 2003). Focusing on the providers of services may be another channel through which behavioral interventions can affect policy outcomes.

2.2 Background and Context

Over the past fifty years, the supply of public housing in the US has fallen (Rice, 2016). In its place, the HCV program has become the largest housing program, serving over two million households each year (Acosta & Gartland, 2021). The HCV program is administered by city- and county-based Public Housing Authorities (PHAs) across the country, and provides extremely

low-income families with assistance in the form of a voucher (i.e., subsidy) to afford rental homes on the private market. By allowing families to rent a unit of their choice in the private market, HCVs can also facilitate geographic and economic mobility by enabling participants to move to higher opportunity neighborhoods—those with lower crime and unemployment, and better schools (Fisher, 2015; Galvez, 2010). Prior studies have shown that children who move into high opportunity neighborhoods are significantly more likely to complete college and go on to earn higher wages than those who remained in lower opportunity neighborhoods (Chetty et al., 2016). However, the HCV program is only successful if landlords—especially those in higher opportunity neighborhoods—are willing to rent to tenants with vouchers.

Overall, the program has struggled to attract and retain landlords, and participation rates have been steadily declining over recent years. Out of over 10 million landlords across the country, just 700,000 are currently participating in an HCV program (HUD, 2019a). More consequentially, just 18% of voucher-affordable units in metropolitan areas can be found in higher opportunity neighborhoods (Mazzara & Knudsen, 2019). Especially in tight housing markets, landlords that are willing to rent to tenants with vouchers are often clustered in segregated, high-poverty neighborhoods (Devine et al., 2003; DeLuca, Garboden, & Rosenblatt, 2013; Rosen, 2014). This can, in turn, reinforce the concentration of poverty in certain neighborhoods and exacerbate existing socioeconomic inequities (DeLuca, Garboden, & Rosenblatt, 2013).

Although there is broad consensus about the importance of engaging landlords in the HCV program, there is relatively little evidence on effective strategies for doing so. Cities and states across the country have implemented a number of policies to try and increase landlord participation, including laws against source of income (SOI) discrimination, which makes it illegal for landlords to refuse tenants with vouchers. But enforcement of such laws is difficult. In fact, widespread discrimination against voucher holders has been repeatedly documented, even in states where it is illegal to reject a tenant's application because they are enrolled in a federal housing program (Cunningham et al., 2018; Demsas, 2021; Hangen & O'Brien, 2022; Wiltz, 2018). Other efforts have focused on financially incentivizing landlord participation (Cunningham, 2016), offering insurance against damages caused by tenants with vouchers (HUD, 2019b), widespread outreach efforts (HUD, n.d.), and increasing the maximum allowable rent (Duerig, 2022). With a few notable exceptions (e.g., Bergman et al., 2023; Collinson & Ganong, 2018; Ellen, O'Regan, & Harwood, 2022), most of these strategies have not been empirically tested. Housing policymakers have consistently called for additional research on methods of engaging landlords and the US Department of Housing and Urban Development has made this one of their top priorities for the next five years (HUD, 2022).

This study was conducted in partnership with the Minneapolis Public Housing Authority (MPHA), which administers the federal HCV program in addition to other affordable housing programs for the City of Minneapolis. Minneapolis has a population of approximately 425,000 people and, across all of its programs, MPHA provides affordable housing to around 26,000 low-income residents—about 6% of the city's population. Additionally, Minneapolis requires all rental properties in the city to have a license, and all rental license data are publicly available. At the time of the study, there were 22,823 active rental licenses in Minneapolis, representing approximately 15,000 unique landlords (including property management firms or other limited liability companies). Yet, only 1,042 landlords (7%) were participating in the HCV program.

Like many PHAs, MPHA employs a wide range of approaches to engage new landlords in the HCV program, from community presentations to virtual workshops. For the present study,

MPHA was particularly interested in increasing landlord interest in the program as a first stage to increasing actual participation. Thus, this is the primary focus of and outcome for this research.

2.3 Methods

I examine the role of administrative burdens in shaping Minneapolis landlords' decisions to participate in the HCV program in three ways: (1) a large-scale survey; (2) a pilot field experiment; and (3) a survey experiment.

Large-scale survey

In March 2022, I worked with MPHA to develop and conduct an online survey among all active landlords in Minneapolis (N ~ 15,000). The survey was sent by the City of Minneapolis to an email list of all active rental license holders, and responses were collected over a two-week period. The email invitation that accompanied the survey framed it as an opportunity to learn about landlords' experiences and perspectives in order to inform MPHA and city outreach efforts. To minimize response bias, neither the email nor the survey landing page mentioned the HCV program.

The roughly 10-minute survey aimed to quantify the relative role of different barriers to participation in the HCV program, with a particular focus on measuring informational, logistical, and psychological costs. The survey also allowed for open-text responses. Survey questions were organized along the three dimensions of administrative burden, as shown in Table 2.1. First, respondents were asked about their familiarity and experience with the HCV program (learning costs). Second, the survey measured respondents' beliefs about the process of participation (compliance costs), including their perceptions of how long the process takes; how many landlords fail the required inspection; and the extent to which they believe the process is too long or too difficult to be worthwhile. Third, respondents were asked about their perceptions of tenants who use vouchers (psychological costs). Specifically, I measured the extent to which respondents hold common stereotypes about tenants with vouchers (e.g., tenants with vouchers are not responsible or hard working), as well as their concerns about renting to voucher holders, which may stem directly from these stereotypes. Finally, to supplement these findings, I measured how landlords view their role in the community and their perceptions of MPHA. Respondents were also asked for basic demographic information including race, ethnicity, gender, age, experience as a Minneapolis landlord, and number of units owned.

Questions that used scales of agreement are quantified using two approaches. First, I compare the average level agreement with the measures corresponding with each dimension of administrative burden. Second, I transform each measure of agreement into a categorical indicator with three levels reflecting a positive response (somewhat agree, agree, or strongly agree), a neutral response (neither agree nor disagree), or a negative response (somewhat disagree, disagree, or strongly disagree). This allows for comparisons of the percent of respondents who agreed with each survey measure. Using either method does not change the findings.

For survey questions that assessed respondents' perceptions of tenants with vouchers using a 1 to 10 scale, I similarly analyze the mean score across measures and, for each, construct a categorical indicator with three levels reflecting a positive response (6-10), a neutral response (5), and a negative response (1-4).

The analysis reported in this paper focuses on the subset of respondents who completed the survey, but results do not differ meaningfully when looking instead at the full sample of respondents who started the survey, but did not complete it.

Table 2.1. Survey questions

Survey measure	Scale
Learning costs	
Prior to today, had you ever heard of the section 8/Housing Choice Voucher program?	1 = Yes; 0 = No or unsure
Compliance costs	
To what extent do you agree or disagree with the following statement: The inspection and approval process required to rent to a voucher holder is too difficult to make it worthwhile.	1 = Strongly disagree; 7 = Strongly agree
To what extent do you agree or disagree with the following statement: The inspection and approval process required to rent to a voucher holder takes too long to make it worthwhile.	1 = Strongly disagree; 7 = Strongly agree
To what extent do you agree or disagree with the following statement: In order to rent to a tenant with a section 8/Housing Choice Voucher, I would have to hold my unit vacant for longer than I would if I rented to a private market tenant.	1 = Strongly disagree; 7 = Strongly agree
In order to rent to a voucher holder and receive payment from MPHA, property owners must complete paperwork and pass an inspection. How long do you think this process takes, including MPHA approving the paperwork and the unit passing the inspection?	Less than a week; 1-2 weeks; 3-4 weeks; 5-6 weeks; 7-8 weeks; More than 8 weeks
Voucher payments are typically backdated to the date a lease is signed, but sometimes it takes time for payments to be made. How long do you think it would take to receive initial payment from MPHA after meeting these requirements and signing a lease with a tenant with a section 8/Housing Choice Voucher?	Less than a week; 1-2 weeks; 3-4 weeks; 5-6 weeks; 7-8 weeks; More than 8 weeks
If you had to guess, what percentage of property owners do you think fail the inspection the first time?	0-100%
Psychological Costs	
To what extent do you agree or disagree with the following statement: I am worried about damages to my unit if I rent to a voucher holder.	1 = Strongly disagree; 7 = Strongly agree
How well do you think each of the following phrases describe tenants who use section 8/Housing Choice Vouchers? Trustworthy; Responsible; Respectful; Hard working; Knowledgeable about maintaining an apartment	1 = Not at all; 10 = Very well
If you had to guess, what percentage of people who use section 8/Housing Choice Vouchers do so as a result of their own personal failings (as opposed to primarily the result of circumstances that are beyond their control)?	0-100%
To what extent do you agree or disagree with the following statement: People who receive section 8/Housing Choice Vouchers deserve a safe and stable place to live.	1 = Strongly disagree; 7 = Strongly agree
To what extent do you agree or disagree with the following statement: People who use section 8/Housing Choice Vouchers should not be ashamed.	1 = Strongly disagree; 7 = Strongly agree

Other	
To what extent do you agree or disagree with the following statement: I have a responsibility to help tenants with section 8/Housing Choice Vouchers find a safe and stable place to live.	1 = Strongly disagree; 7 = Strongly agree
To what extent do you agree or disagree with the following statement: I can make a positive impact in my community by renting to a tenant with a section 8/Housing Choice Voucher.	1 = Strongly disagree; 7 = Strongly agree
To what extent do you agree or disagree with the following statement: Landlords who rent to section 8/Housing Choice Voucher tenants improve the Minneapolis community.	1 = Strongly disagree; 7 = Strongly agree
To what extent do you agree or disagree with the following statement: More landlords should accept tenants who use a section 8/Housing Choice Voucher.	1 = Strongly disagree; 7 = Strongly agree

Field experiment

Building on the results from the initial survey, I worked with MPHA to co-design and test three different mail-based outreach messages aimed at increasing engagement among non-participating landlords in a pilot field experiment. This study was pre-registered on the AEA RCT Registry (AEARCTR-0008686).

The experimental universe was drawn from the city-wide rental license registry, which includes the address of every rental property in Minneapolis that has an active license, and the associated property owner name and address. I excluded all short-term rental properties (and their landlords), as short-term rentals are not eligible for the HCV program. I also matched rental license data with administrative data from MPHA in order to exclude landlords who were already renting to a tenant with a voucher at the time of the study. Thus, all landlords in the experimental universe had active rental licenses and were not actively participating in the HCV program. While all landlords in the experimental universe had addresses in the rental registry, there was no way to ensure that the address was where the landlord themselves lived (as opposed to the address of a property management company or some other business entity).

Landlords can own more than one property and often set up different entities to manage their properties. To ensure that landlords with multiple addresses in the database received the same outreach message, I clustered the randomization by landlord name, landlord address, and rental property address. The randomization was also stratified by modal neighborhood for each cluster and cluster size. Small strata (fewer than three landlord addresses) were randomized together. The final experimental universe included 12,715 clusters, representing 13,419 landlord addresses and 19,543 rental property addresses.[2]

In a stratified randomization, each cluster was randomly assigned with equal probability to one of three experimental conditions. Landlords assigned to the *Status Quo* group received a mailer that provided clear and simple information about the HCV program, adapted from

[2] There are fewer clusters than unique landlord addresses and unique rental property addresses because some landlords have multiple properties and some landlord names are associated with multiple owner addresses. Thus, clustering by all three variables results in clusters that are linked by rental address, landlord address, and/or landlord name such that a unique landlord (based on their name) may have multiple addresses within a single cluster.

MPHA's status quo language at the time. The message emphasized that landlords who rent to tenants with vouchers make a difference in their community. Landlords assigned to the *Process* group received a mailer that provided the same information about the HCV program as in the *Status Quo* group but with specific language adjustments to correct misperceptions about the process of participation. Specifically, mailer language emphasized that 60% of landlords pass the required inspection on the first try and that approval typically takes one business day once all information is received. Finally, landlords assigned to the *Destigmatizing* group received a mailer that provided the same information as in the *Status Quo* group, but with language that aimed to correct misperceptions about tenants who use vouchers. Language emphasized that tenants with vouchers are responsible, hard-working, and long-term tenants—they stay for an average of 7 years. Each mailer also included a real quote from a Minneapolis landlord that aligned with the experimental condition. See Appendix B for all treatment materials. Each landlord address in the final experimental universe was sent a mailer corresponding with their treatment assignment. All mailers were sent in June 2022.

All three mailers included the same call to action: a link (and QR code) to an online interest form through which landlords could request more information about the HCV program, let MPHA know that they had a vacant unit, or sign up to attend a workshop to learn more. The primary outcome of interest was registered interest through this form in the four weeks after mailing. Although the interest form asked for landlords' contact info and rental property address, there was no way to ensure that landlords would provide the same information that was used in constructing the experimental universe. As such, each mailer included a unique URL linked to the interest form. This allowed me to evaluate differences in submission rates across conditions without relying on linking interest form data to the experimental universe.

Survey experiment

In September 2022, I fielded a second survey to all active landlords in Minneapolis (N ~15,000). Again, the survey was sent directly by the City of Minneapolis, and responses were collected over a two-week period. The approximately five-minute survey was primarily intended to assess landlords' knowledge of different renter protection ordinances and preferences for communicating with the city, in order to inform future outreach efforts. But because the survey took place after the pilot field experiment, I also embedded an implementation check and a related survey experiment.

At the end of the survey, all respondents were first asked if they recalled receiving a postcard from MPHA with information about the HCV program in the prior three months. This question aimed to assess whether the field experiment mailers reached landlords as intended. This was a concern for two reasons. First, it was not possible to validate the landlord addresses used in the field experiment prior to the intervention. Second, the mailers were sent by a third-party vendor. While quality assurance processes were in place, it was not possible to fully monitor implementation fidelity during the experiment.

Survey response data cannot be linked to the randomization universe, nor to the list of emails that was used to send out the survey, since we could not ask for personally identifiable information in the survey itself or have access to this information to send the survey. That said, the samples for the field experiment and survey were drawn from the same primary universe: all landlords with an active rental license in Minneapolis. Based on the publicly available data, 79% of landlords in the randomized universe had an email address on file in the rental registry. Thus,

a majority of landlords in Minneapolis should have received both a survey and a mailer as part of this research.

Then, in a survey experiment, all respondents were randomly assigned to see one of the three mailers from the field experiment. After being prompted to read the mailer closely, they were asked whether they would like to complete the interest form—if they responded affirmatively, they were directed to the actual MPHA interest form. There is no way to know what mailer survey respondents received as part of the field experiment. But assuming there is a uniform distribution of prior treatment, this should not bias the results of the survey experiment.

The primary outcome of interest is a binary indicator of interest in the HCV program after seeing the mailer. I evaluate average differences in interest by experimental condition via a covariate-adjusted linear model, controlling for respondent age, gender, race (White or non-White), ethnicity (Hispanic or not Hispanic), and prior experience with the HCV program.

2.4 Results

Large-Scale Survey: Quantifying the Barriers to Participation

In total 1,088 landlords started the initial survey and 797 (73%) submitted it. As shown in Table 2.2, approximately 71% of respondents were White; 54% were male; and the modal respondent had been a Minneapolis property owner for over 10 years. A majority of respondents—62%—own just one or two units; 25% own 3-9 units; and just 13% own 10 or more units.

Table 2.2. Survey respondent characteristics

	All responses	Submitted responses
N	**1088**	**797**
Race		
White	53.6%	71.1%
Black/African American	1.4%	1.5%
Asian	1.9%	2.5%
Other	3.0%	4.1%
Multiracial	2.2%	2.9%
Prefer not to answer/missing	37.9%	17.8%
Ethnicity		
Hispanic	1.8%	2.5%
Not Hispanic	56.5%	74.8%
Prefer not to answer/missing	41.6%	22.7%
Gender		
Male	41.1%	54.1%
Female	24.3%	32.0%
Nonbinary	1.6%	2.1%
Other	0.4%	0.5%
Prefer not to answer/missing	32.7%	11.3%
Age		
Mean age (SD)	49 years (14.3)	49 years (14.3)
Length of time as Minneapolis property owner		
< 1 year	5.6%	7.3%
1-2 years	8.6%	11.3%
3-4 years	10.9%	14.1%
5-6 years	8.5%	10.9%
7-8 years	6.0%	7.5%
9-10 years	3.8%	5.0%
More than 10 years	32.9%	42.3%
Prefer not to answer/missing	23.9%	1.6%
Number of units owned		
1-2 units	44.0%	56.5%
3-9 units	17.9%	24.0%
10 or more units	9.5%	12.9%
Prefer not to answer/missing	28.6%	6.7%

Table 2.3 presents results from the survey, organized by each dimension of administrative burden. To start, 89% of respondents had heard of the HCV program, and 24% were currently renting or had previously rented to a tenant with a voucher. While this suggests that program awareness is not a consequential barrier for the majority of this population, it is possible that landlords face other learning costs that were not measured directly in the survey, including a lack of understanding or misperceptions of program eligibility criteria, benefits, or enrollment processes.

Next, among respondents who had heard of the program, 50% reported that the process of participation was too difficult and time consuming to make it worthwhile. Many of the open-ended responses underscored this. For example, one respondent said: "*As I am a new landlord, the license approval process itself was involved enough that I chose not to undergo the*

additional steps for the HCV program. I was worried about extra inspection requirements that would make it harder for me to rent out the unit. I was interested in finding a tenant on a short timeline and securing rent in a timely manner." In reality, the inspection and approval process takes an average of one to two weeks, but 78% of respondents believed that it takes three or more weeks. At the same time, respondents' beliefs about the percentage of landlords that fail the inspection on their first attempt were correct on average: 40% (see Table 2.3). This suggests that some—but not all—of the perceived compliance barriers to HCV program participation are being driven by misperceptions or incorrect beliefs about the enrollment process.

Finally, a large majority of respondents—70%—reported concerns about property damage. Again, similar concerns arose in the open-ended comments: "*I haven't taken part in the HCV program in my 25 years as a landlord because I frequently hear from other landlord friends that more damage is done to their properties [with tenants with vouchers]. Not providing the money for the damage deposit seems to make renters less careful with the property. Damage is more concerning for me than not paying rent.*" Relatedly, a significant proportion of respondents voiced common stereotypes about tenants who use vouchers: 37% of respondents do not believe most tenants with vouchers are responsible, 25% do not believe they are hardworking, and 30% do not believe they are trustworthy. Reflecting broader concerns about damages, 52% of respondents also do not believe tenants with vouchers are "knowledgeable about maintaining an apartment."

In addition to measuring each administrative burden, I also examined how landlords view their role in the community, particularly as it relates to accepting low-income tenants with vouchers. Although 48% of respondents believed that landlords can have a positive impact in their community by renting to tenants with vouchers, only 41% believed that more landlords should accept tenants with HCVs and only 30% believed that they personally have a responsibility to rent to tenants with vouchers (see Table 2.3). These beliefs differed by respondents' experience with the HCV program: 36% of respondents with current or prior HCV program experience believed they have a responsibility to rent to tenants with vouchers, compared to just 28% of those without current or prior program experience. Relatedly, just 28% of landlords with prior HCV program experience said that more landlords should accept tenants with vouchers, versus 44% of those who do not have prior experience with the program. That said, this correlation should be interpreted with caution since landlords with prior HCV program experience likely made an intentional choice to not continue accepting tenants with vouchers, and thus may hold more negative views of the program than respondents who are currently participating or who have never participated for reasons not captured by this survey.

While many landlords in this sample do not believe it is their responsibility to provide safe and affordable housing for low-income residents, this is not to say that they do not care about the well-being of low-income residents. Indeed, 90% of respondents agreed that tenants who use vouchers deserve a safe place to live. Rather, this speaks to how landlords view their role, compared to the role of other potential stakeholders. Open-ended comments reflected this: "*It should not be the landlord's/owner's responsibility to provide housing for section 8 voucher holders. The county/state should take the money they would be giving to landlords/owners and provide a safe and reasonable housing community that is set up and designed for them.*"

Overall, these findings suggest that psychological hurdles, particularly beliefs about the characteristics of tenants who use vouchers, are at least as consequential as compliance barriers, and maybe more so. This extends existing research that has documented the existence of compliance and psychological barriers to landlord participation in the HCV program, but has not

directly measured their impact or compared their relative role. In this survey, 45% of respondents reported being more concerned about damages incurred by renting to tenants with vouchers than about the HCV program inspection and approval process; another 29% were equally concerned about both. Concerns about damages may stem directly from stereotypes about the types of tenants who participate in the voucher program. Open-ended comments appear to support this association: "*The program is important. The problem is the renters. There are plenty of lousy non HCV tenants who expect the world and fail to care for the property. But HCV tenants seem to feel more entitled, less responsible, and willing to stretch the HCV rules.*"

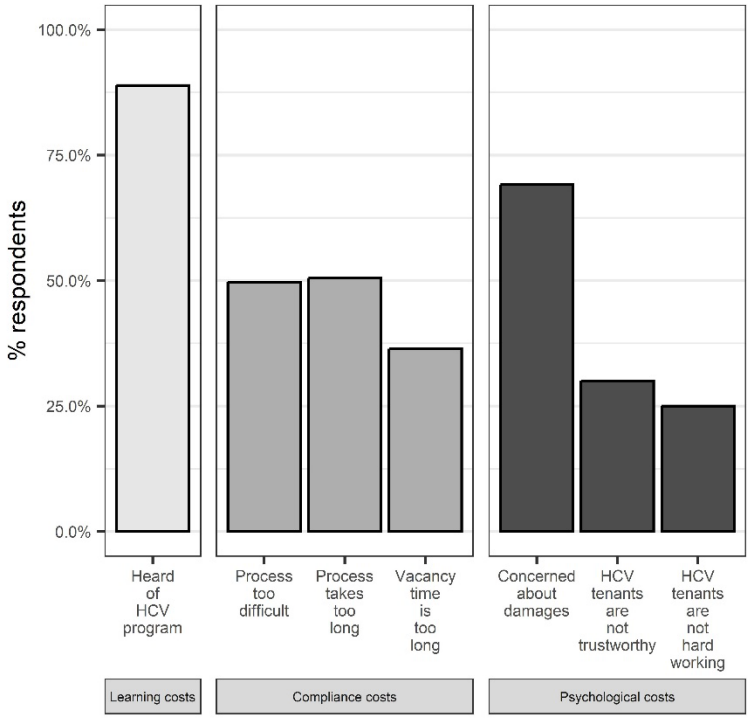
While the results of this survey should not be taken as representative of the beliefs of all Minneapolis landlords, they point to potential avenues for intervention. Namely, correcting landlords' misperceptions about the application and inspection process, and reducing the stigma associated with voucher holders may both be promising avenues for increasing landlord engagement. At the same time, these findings suggest that status-quo messaging that emphasizes the role landlords' play in providing safe and affordable housing may be less effective. The pilot field experiment tested these hypotheses directly.

Table 2.3. Survey results

	Response
Learning costs	
Had heard of HCV program [N = 1,034]	88.9%
Compliance costs	
Agree with: Inspection and approval process is too difficult [N = 716]	49.7%
Agree with: Inspection and approval process takes too long [N = 717]	50.5%
Agree with: Renting to voucher holder requires holding unit vacant for longer [N = 717]	36.4%
Est. length of time inspection and approval process takes > 3 weeks [N = 696]	77.9%
Est. length of time required to receive first payment from MPHA > 4 weeks [N = 702]	42.6%
Est. % landlords who fail inspection first time (SD) [N = 710]	41.5% (25.2)
Psychological costs	
Agree with: Concerned about damages with voucher holders [N = 719]	69.1%
Voucher holders are: trustworthy (SD) [N = 680]	5.4 (1.9)
Voucher holders are: responsible (SD) [N = 675]	5.0 (2.0)
Voucher holders are: respectful (SD) [N = 680]	5.6 (1.9)
Voucher holders are: hard working (SD) [N = 679]	5.7 (2.1)
Voucher holders are: knowledgeable about maintaining an apartment (SD) [N = 662]	4.4 (2.0)
% of voucher holders who use HCVs as a result of their own failings (SD) [N = 703]	37.2% (27.0)
Agree with: Voucher holders deserve a safe and stable place to live [N = 719]	91.0%
Agree with: Voucher holders should not be ashamed [N = 717]	82.9%
Landlords' role in community	
Agree with: I have responsibility to help tenants with vouchers [N = 718]	30.5%
Agree with: I can make positive impact in my community by renting to a tenant with a voucher [N = 719]	48.5%
Agree with: Landlords who rent to tenants with vouchers improve community [N = 715]	49.2%
Agree with: More landlords should accept tenants with vouchers [N = 718]	41.5%

Notes: Sample limited to respondents who completed the survey (N = 797). Because all questions were voluntary, the total number of responses differs by question. Results do not meaningfully change when including partial responses. See Table 2.1 for exact question language and scales for each question.

Figure 2.1. Survey results, by administrative burden dimension



Notes: Bars represent the raw percent of respondents that answered affirmatively to each survey question (see Table 2.1 and 2.3).

Field experiment

Overall, just 16 landlords (0.1%) responded to the interest form after receiving a mailer: 6 in the *Status Quo* group, compared to 6 in the *Process* group ($z = .04$, $p = .97$) and 4 in the *Destigmatizing* group ($z = .04$, $p = .48$). These engagement rates are lower than anticipated, even for light-touch interventions (e.g., DellaVigna & Linos, 2022). It is possible that the barriers to participation are too high to be moved by light-touch outreach, but similar interventions have yielded higher engagement rates in other, ostensibly more burdensome, contexts (e.g., Lasky-Fink, Li, & Doherty, 2022). It is also possible that the mailers did not reach landlords as intended. To try to better understand whether the low engagement rates were driven by a true lack of interest or by unintended implementation complications, I conducted a follow-up survey experiment among active landlords.

Survey Experiment

The final phase of this research was a second survey of all active landlords in Minneapolis. In total, 655 landlords started the survey and 496 (76%) submitted it in a two-week period in September 2022.

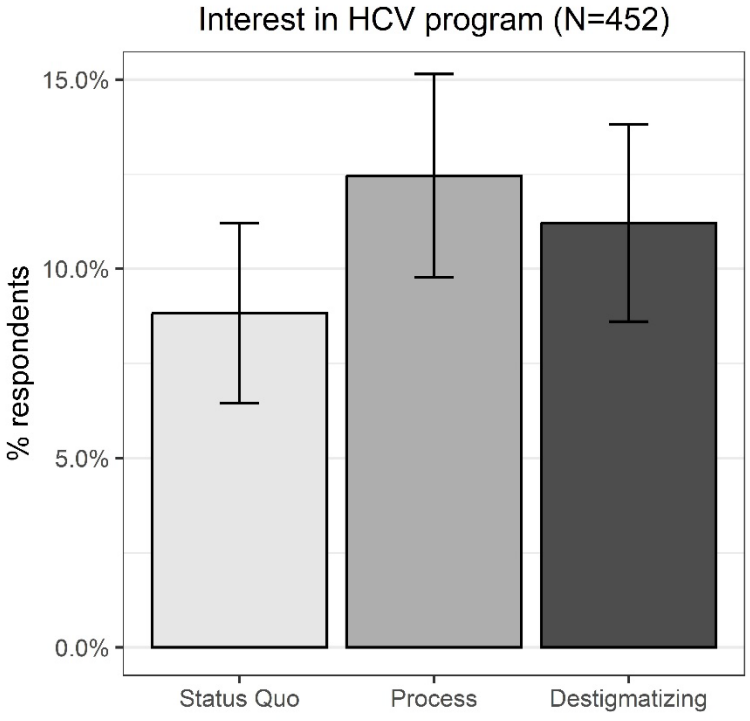
The first outcome of interest is whether survey participants recalled receiving a mailer that was sent as part of the field experiment. In total, just 9% of respondents ($N = 455$) recalled receiving a mailer from MPHA in the months prior to the survey. Not all respondents would

have received a mailer (see Methods), nor would we expect perfect recall among those who did. Nevertheless, even with these caveats, this is a lower recall rate than anticipated. For example, the average recall rate for a Facebook ad—an arguably less salient medium than a government mailer—is 18% (McGaff, 2018). This suggests that many landlords may not have received a mailer as intended during the field experiment.

The second outcome of interest comes from the embedded survey experiment. In total, 452 respondents were randomized to see one of the three mailers used in the field experiment, and answered the relevant outcome question. Overall, 8.8% of respondents who were shown the *Status Quo* mailer completed the interest form, compared to 12.5% of respondents who were shown the *Process* mailer ($t = 1.01$, $p = .31$, 95% CI[-0.03, 0.11]) and 11.2% of respondents who were shown the *Destigmatizing* mailer ($t = 0.67$, $p = .50$, 95% CI[-0.05, 0.09]).

Although survey respondents are not representative of all Minneapolis landlords, these results suggest that baseline interest in the HCV program may be higher than found in the field experiment. Additionally, while we are underpowered to detect differences smaller than 11 pp between conditions in this study, these results suggest that different framings may yield small gains in engagement. Future studies should continue to test the impact of different outreach messages with larger samples in real-world contexts.

Figure 2.2. Survey experiment results



Notes: Regression-adjusted proportion of survey respondents who expressed interest in learning more about the HCV program, by experimental condition. Estimates come from covariate-adjusted linear model, controlling for respondent age, gender, race (White or non-White), ethnicity (Hispanic or not Hispanic), and experience renting to a tenant with a voucher. Error bars reflect +/- 1 standard error.

2.5 Discussion

This study presents results from a large survey, a pilot field experiment, and a survey experiment on the barriers landlords face to participating in the HCV program in Minneapolis, and on the impact of a light-touch behavioral intervention aimed at reducing these barriers. Survey results suggest that stereotypes about tenants with vouchers may be one of the largest barriers to landlord participation, even more so than perceived compliance hurdles such as burdensome inspection processes and lengthy approval timelines. Although outreach aimed at reducing these barriers did not significantly affect landlord interest, results from a subsequent survey experiment suggest that light-touch interventions may still hold promise in this context.

This research has a few important limitations that suggest directions for future work. First, while this research offers some of the first empirical evidence on the relative importance of different administrative barriers to landlord participation in the HCV program, I cannot determine the extent to which these findings are generalizable across contexts or even within the Minneapolis context. These findings challenge existing narratives that focus primarily on the role of compliance hurdles in deterring landlord engagement. While I find that compliance barriers are consequential, in this sample they were not the primary barrier. At the same time, existing evidence suggests that the factors that drive participation and nonparticipation in the HCV program may differ meaningfully by both geography and landlord characteristics (Garboden et al., 2018). Future research should focus on measuring these barriers across contexts and in bigger samples to better understand the generalizability of the present findings, and heterogeneity by landlord characteristics.

Second, although the pilot experiment yielded null effects, it is unknown whether this is due to an implementation failure, ineffective modality, or ineffective messaging. Additional studies should test similar interventions, delivered via multiple modalities, in order to understand the potential for light-touch interventions to influence landlord interest in the program. Additionally, there may be important heterogeneity in the types of barriers different landlords face, which could influence the effectiveness of outreach. For instance, property management companies may face—or perceive—different barriers to participation than individual landlords who own just a few units. Exploring these differences is a critical first step toward designing targeted interventions aimed at reducing barriers to participation.

Finally, the findings presented here highlight large systemic barriers that, even in the best case scenario, cannot be solved by light-touch interventions. Future studies should examine the role of higher-touch interventions, including leveraging community or social networks, to encourage landlord participation in the HCV program. Existing research in this area has shown promise (Bergman et al., 2023), but much is left to understand about how to best target—and reduce—consequential barriers to landlord engagement.

Chapter 3

Increasing Take-up of CalFresh Among Californian College Students: The Impact of Messaging and Modality

3.1 Introduction

Recent studies have found that 20 to over 50 percent of US college students are food insecure (Coleman-Jensen et al., 2022; Freudenberg, Goldrick-Rab, & Poppendieck, 2019; Morris et al., 2016). Not only is food insecurity associated with myriad adverse health outcomes, including diabetes, obesity, and depression, but it has also been shown to negatively affect academic achievement and persistence (Bruening et al., 2017; Martinez et al., 2020; Maroto, Snelling, & Linck, 2015). Yet, just 20 to 30 percent of food insecure college students participate in the Supplemental Nutrition Assistance Program (SNAP)—the largest food benefits program and one of the most effective anti-poverty programs in the US—in part because traditional eligibility criteria exclude most students (GAO, 2018). However, as part of the Federal Government's response to the COVID-19 pandemic, SNAP eligibility was temporarily simplified and expanded (Federal Student Aid, 2021). As a consequence, millions of low-income college students became newly eligible for SNAP benefits in 2020-2021. This offered an opportunity to test the impact of targeted informational outreach—both the message and modality—on benefits take-up among a population that may face uniquely high barriers to participation.

I partnered with the California Student Aid Commission (CSAC), the California Department of Social Services (CDSS), and the California Policy Lab (CPL) to conduct a large-scale field experiment ($N = 275{,}977$) aimed at increasing take-up of CalFresh, California's SNAP program, among likely eligible college students. In a mass outreach campaign, I tested the impact of communication modality, as well as the impact of different messages, on applications for CalFresh. I found that simplified messaging increased application rates by 0.2 percentage points (pp), or 7%, relative to the status quo message, but additional language tweaks aimed at reducing potential psychological costs associated with CalFresh participation had no impact beyond the simplified message. At the same time, I found that multimodal outreach (email and postcard) nearly doubled application rates compared to outreach conducted via email alone.

This research makes three main contributions. First, it builds on and extends the existing evidence base on the role of administrative burdens in the social safety net. Although a growing literature explores how administrative barriers affect access to government programs for low-income households, empirical evidence on reducing these barriers has thus far been mixed (e.g., Finkelstein & Notowidigdo, 2019; Linos et al., 2022). Second, these findings directly contribute to the broad literature on the use of light-touch interventions to encourage welfare-enhancing behavior by extending our understanding of what works beyond linguistic tweaks. Behavioral science has become increasingly influential in the design and implementation of information interventions and has yielded promising results across a range of policy domains (Dai et al., 2021; Gerber & Rogers, 2009; Milkman et al., 2011), but there are few empirical studies on the impact of communication modality. This study offers one of the first tests of communication modality on a real-world, consequential behavior. Finally, these findings speak to both academics and practitioners focused on understanding and alleviating food insecurity among

college students by demonstrating the potential of targeted interventions to connect college students to available resources.

3.2 Current Literature

The social safety net in the US provides critical supports for low-income Americans that have been shown to effectively mitigate the effects of poverty on a broad scale. SNAP, in particular, has been shown to have long-term health and economic benefits, especially for young children (Bronchetti, Christensen, & Hoynes, 2019; Hoynes, Schanzenbach, & Almond, 2016). Yet, an estimated 20 percent of individuals who are eligible for SNAP do not participate (Vigil, 2022). There is limited evidence on participation rates among college students specifically, in part because complicated eligibility criteria make it difficult to determine how many college students qualify for SNAP benefits. In California, estimates range from 289,000 to 561,000 college students who are eligible for CalFresh benefits, but not participating (Johnson, 2020). Related research documents similarly large take-up gaps for financial aid among college students (Bettinger et al., 2012; Bird et al., 2021; Page, Castleman, & Meyer, 2020). While there are important differences between the design and administration of financial aid and CalFresh, the barriers students face to participation may be similar.

To categorize these barriers, I draw on a growing literature on administrative burden, which documents learning, compliance, and psychological costs that can contribute to so-called "take-up gaps," especially among the most vulnerable (Christensen et al., 2019; Currie, 2004; Herd & Moynihan, 2019). Learning costs are those associated with learning about a program, its eligibility criteria, and relevant application requirements and processes. In the context of SNAP, students must first learn that the program exists and that they are eligible, and then must seek out information about enrollment requirements. Some evidence suggests that learning costs were a significant barrier for many college students prior to the pandemic (GAO, 2018). Students who became newly eligible for SNAP during the pandemic likely faced even higher barriers as they simultaneously had to learn about the program and their new eligibility status.

Compliance barriers are those associated with applying for, enrolling in, and maintaining access to a program. This can involve lengthy and burdensome application processes, including in-person interviews, extensive verification and documentation requirements, and even drug testing in some states. Such burdens impose a "time-tax" on prospective beneficiaries; those who lack the resources to overcome this tax may miss out on benefits for which they are eligible (Lowrey, 2021). For instance, the complexity of the Free Application for Federal Student Aid (FAFSA) form has been shown to deter students from applying for financial aid (Dynarski & Scott-Clayton, 2008; Page, Castleman, & Meyer, 2020). Although California has made strides in simplifying the application process for CalFresh (Code for America, 2019), there are still many hurdles that prospective beneficiaries must navigate.

Finally, psychological barriers include the stigma, loss of autonomy, and threat to one's self-worth or identity that can be associated with applying for or participating in benefit programs. The existence of poverty and welfare stigma has been well-documented in both qualitative and survey-based research (Baumberg, 2015; Stuber & Kronebusch, 2004; Nichols, 2020). People living in poverty, and especially those who benefit from government assistance, are often stereotyped as lazy, undeserving, and even morally inferior (Celhay, Meyer, & Mittag, 2022; Lauter, 2016; Mead, 2019; Stuber & Schlesinger, 2006). These stereotypes are both racialized and gendered, suggesting that the effects of stigma may fall disproportionately on

systemically marginalized populations (Brown-Iannuzzi et al., 2017; Federico, 2004; Gilens, 1999; Soss, Fording, & Schram, 2011). Prior to deciding to participate in a program, prospective beneficiaries, including students, must thus overcome the stigma and potential threats to their self-image and identity that are often associated with being a beneficiary of government assistance. It is unclear whether or how psychological costs may differ for newly eligible individuals relative to the broader population of prospective beneficiaries.

A large body of research tests behaviorally-informed methods of reducing these barriers and increasing take-up, but has yielded mixed results. For instance, reducing learning costs by providing clear and simple information about program benefits has been found to increase take-up in some contexts (Bhargava & Manoli, 2015; Finkelstein & Notowidigdo, 2019), but not in others (Linos et al., 2022). Meanwhile, simplifying program requirements, pre-filling application materials, and providing assistance with application processes have been shown to effectively increase enrollment in a range of programs, including FAFSA (Bettinger et al., 2012), health insurance (Collins et al., 2016), Supplemental Security Income and Social Security Disability Insurance (Deshpande & Li, 2019), and SNAP (Finkelstein & Notowidigdo, 2019; Schanzenbach, 2009).

Efforts to reduce stigma via written communications have primarily focused on testing the impact of reframing how policies and programs are described, which has been found to have outsized effects on recipient decision-making in other contexts. In the context of the social safety net, however, empirical evidence is both limited and mixed. Bhargava and Manoli (2015) found that adjusting how the EITC was framed in government letters to target one potential source of stigma did not meaningfully increase take-up, although the authors acknowledge that the EITC is not a highly stigmatized government program. In fact, some research suggests the EITC is not typically seen as "welfare" (Halpern-Meekin et al., 2015). Two other studies find more promising results. Schanzenbach (2009) found some evidence that reframing SNAP as a "benefit transfer" instead of "food stamps" increased interest in learning about the program by about 30 percent. Similarly, De La Rosa et al. (2021) found that reframing informational outreach to induce psychological ownership (e.g., "your stimulus payment" instead of "a stimulus payment") increased interest in benefits programs including SNAP and the EITC.

While implementing behavioral field experiments of this nature all involve important decisions about the modality of communication (e.g., text messages, emails, mailers, postcards), most empirical studies examine the efficacy of a single communication method. There is limited evidence on how the modality of communication influences the effectiveness of light-touch communications (Kappes et al., 2021). Taken together, this suggests a need for more empirical research on when, why, and for whom light-touch behavioral interventions are most effective, as well as the most effective way of delivering such interventions.

3.3 Setting and Methods

As of 2019, SNAP provided 38 million low-income individuals and households across the US with benefits that can be spent on groceries (Hall & Nchako, 2022). Yet, under traditional eligibility rules, most college students—regardless of their income—are not eligible to participate. To be eligible for SNAP benefits, full-time students must qualify for a specific exemption. For instance, if a college student participates in a state- or federally-funded work study program, cares for a child under the age of six, or is a single parent caring for a child under the age of twelve, they may be eligible for SNAP benefits. Most college students do not meet

any of the exemption criteria, and many of those who are eligible mistakenly conclude that they are not, in part because of how the rules are written and communicated (Freudenberg, Goldrick-Rab, & Poppendieck, 2019). Even for those who know they are eligible, proving eligibility can be complicated and onerous.

In 2020, as part of the Federal Government's efforts to mitigate the economic consequences of the COVID-19 pandemic, SNAP eligibility criteria were simplified and expanded such that any student eligible for work-study programs (regardless of participation) or who had an expected family contribution (EFC) of $0 on their FAFSA form was temporarily eligible for SNAP benefits. In California, students could receive up to $234 per month to spend on groceries. But because most college students were previously ineligible, state social services agencies faced the herculean task of having to find and inform students of their new eligibility status.

In 2021, CDSS, which administers the state-wide CalFresh program, and CSAC, which administers the state's financial aid programs, conducted two mass outreach campaigns to encourage likely eligible students to apply for CalFresh benefits under the temporary eligibility expansion. The first mass outreach campaign was conducted in February 2021 (Palos Castellanos et al., 2022). For the second campaign, conducted in May 2021, I collaborated with CDSS and CSAC to evaluate the impact of different messages and modalities of outreach on take-up of CalFresh in a large-scale randomized experiment.

Treatment Design

The baseline (*Status Quo*) treatment was an email developed by CSAC that informed recipients that student eligibility for CalFresh benefits had been temporarily expanded due to the COVID-19 pandemic. It outlined that, based on their FAFSA information, recipients may be eligible for benefits under this expansion. The message also included a link to the CalFresh application form, instructions for verifying eligibility, and a list of resources should recipients have additional questions. In all, the message included 300 words, was written at a college reading level, and required careful reading to find the application link.

Starting with the *Status Quo* message, I worked with CDSS and CSAC to co-design three modified communications to evaluate the impact of simplification (T1); destigmatizing language (T2); and language targeting misperceptions of scarcity (T3). T1 was a simplified email adapted from the *Status Quo* message. A large body of behavioral science and communication literature demonstrates that simplification can have a large impact on behavior (Bergman, Lasky-Fink, & Rogers, 2020; Kling et al., 2012). Even when processes themselves cannot be simplified, making content clear and easy to understand can reduce learning barriers and information frictions that may otherwise deter action (Bhargava & Manoli, 2015; DellaVigna & Linos, 2022; Lasky-Fink et al., 2021). Thus, T1 included the same information as the *Status Quo* message, but with less text, simplified formatting, and a lower reading level. The top of the *Simplified* message (T1) stated clearly that recipients may be eligible for CalFresh benefits, and the application link was highlighted by a blue box. The main message was simplified to only include information pertinent to the actual application process—including visual instructions for how to confirm eligibility in the application portal. Less critical information, such as an explanation of why recipients were receiving the email and links to additional resources, was included at the bottom of the message, offset from the main call to action.

The *Destigmatizing* (T2) message aimed to reduce the internalized stigma that can be associated with participation in government assistance programs. Internalized stigma occurs when beneficiaries or prospective beneficiaries of government assistance internalize the negative stereotypes and beliefs held by society (Bos et al., 2013; Fox et al., 2018). This may lead prospective beneficiaries to not participate in government assistance programs to avoid the sense of shame in being identified as part of a stigmatized group or to preserve their identity as an outgroup member. Prior evidence from similar contexts suggests that internalized stigma may be movable, even in the presence of pervasive societal stigma (Lasky-Fink & Linos, 2022). The *Destigmatizing* (T2) message used the simplified language from T1, but with subtle language changes to target potential sources of internalized stigma associated with program participation. For instance, the message emphasized that the pandemic hit many students hard, and "it's okay to need help."

The *Resources* (T3) message also used the simplified language from T1, but included language that targeted perceptions of scarcity, another psychological cost that is closely related to stigma. Preliminary qualitative research conducted by CSAC revealed that some students did not apply for CalFresh because of a belief that they were not "needy enough" to need assistance, and they did not want to take benefits from those who needed them more. In other words, prospective beneficiaries may be deterred from participation due to misperceptions about who the program is intended for and how one's own identity aligns (or not) with these beliefs. In an effort to correct these misperceptions, the *Resources* (T3) message included language emphasizing that CalFresh benefits can help *all* eligible students.

For each of the three messages (T1, T2, T3), we developed email and mail-based (postcard) versions with identical language in order to empirically test the impact of communication modality. All treatment letters are shown in Appendix C (Figures C1-C8).

Experimental Design and Sample

The experimental universe was drawn from CSAC's administrative records and included 285,325 California college students who had $0 EFC reported on their 2020-2021 FAFSA and were thus likely eligible for CalFresh under the temporary eligibility expansion. In a stratified randomization, all students were randomly assigned with equal probability to one of seven conditions corresponding with message and modality, as shown in Table 3.1.

Table 3.1. Experimental Design

Condition	Message	Modality	N	Description
1	Status Quo	Email	38,890	Baseline email designed by CSAC
2	T1 - Simplified	Email	39,501	Simplified language, shorter message, and clear call to action
3	T1 - Simplified	Email + Postcard	39,578	
4	T2 - Destigmatizing	Email	39,471	Simplified language, plus language targeting potential sources of internalized stigma by emphasizing that the "pandemic has hit many students hard" and "it's okay to need help."
5	T2 - Destigmatizing	Email + Postcard	39,504	
6	T3 - Resources	Email	39,517	Simplified language, plus language targeting misperceptions about who the program is intended for by emphasizing that "there are enough benefits to help everyone who is eligible."
7	T3 - Resources	Email + Postcard	39,516	

The randomization was stratified by county and award status, an indicator for whether the student had received a financial aid award in the past year, which we considered a proxy for being an active student. Small strata (< 10) were randomized together.

All messages—both emails and postcards—directed recipients to GetCalFresh.org, the simplified CalFresh application platform developed by Code for America as a contractor to CDSS. However, students could apply for CalFresh directly through CDSS as well. We could not measure applications through CDSS as part of this study.

Outcome Measures and Data

The primary outcome of interest was applications for CalFresh that were submitted via GetCalFresh.org in the six weeks following the intervention (June 1 to July 13, 2021). As a robustness check, I also looked at outcomes over an eight-week period (June 1 to July 30, 2021). This dataset included individual-level records on CalFresh applications submitted through the simplified platform, including demographics, household composition and housing circumstances, information on student eligibility criteria, and information on income and expenses.

CalFresh application data and the CSAC data used to conduct the randomization were de-identified to protect student privacy and comply with legal restrictions around the use of student data. Datasets from CSAC and GetCalFresh were linked through a hashing process developed by CPL that allows for linkage on hashed identifiers without access to the underlying personally identifiable information.

Empirical Strategy

The analytic universe excluded 9,348 DREAM Act students (undocumented and nonresident students) who were randomized as part of the experiment, but for whom I could not receive outcome data because of data privacy rules. These students were balanced evenly across conditions ($\chi^2(6) = 4.05$, $p = .67$). After exclusions, the final analytic sample consisted of 275,977 students, balanced evenly across available covariates, including county, grant status, and award status (see Table 3.2).

In an intent-to-treat analysis, I first evaluated the average effect of condition assignment on applications via the following linear probability model:

$$(1)\ Y_{is} = \propto + \sum_{j} \beta_j treat_{isj} + \gamma_s + \varepsilon_{is}$$

where Y_{is} is an indicator for whether student *i* in stratum *s* submitted an application for CalFresh via GetCalFresh.org in the outcome period; *treat* is an indicator for assignment to treatment arm *j*, and β_j is the average effect of treatment arm *j* relative to the Status Quo condition; and μ_s is a vector of stratum fixed effects.

I then evaluated the average effect of assignment to a multimodal condition (email and postcard), relative to an email only condition via the following linear probability model:

$$(2)\ Y_{is} = \propto + \beta_1 multimodal_{is} + \gamma_s + \varepsilon_{is}$$

where Y_{is} is an indicator for whether student *i* in stratum *s* submitted an application for CalFresh via GetCalFresh.org in the outcome period; *multimodal* is an indicator for assignment to one of the multimodal conditions (conditions 3, 5, 7), and β_1 is the average effect of multimodal communication relative to email only communication; and μ_s is a vector of stratum fixed effects.

As secondary analysis, I also evaluated equation (2) separately by message condition (condition 2 vs. 3; condition 4 vs. 5; condition 6 vs. 7).

Table 3.2. Balance of analytic universe

	Status Quo	Simplified (T1)		Destigmatizing (T2)		Resources (T3)		p-value
	Email only	Email only	Email + PC	Email only	Email + PC	Email only	Email + PC	
N	38890	39501	39578	39471	39504	39517	39516	
Grant status	12433 (32.0%)	12531 (31.7%)	12559 (31.7%)	12397 (31.4%)	12428 (31.5%)	12558 (31.8%)	12501 (31.6%)	0.67
Award status	20998 (54.0%)	21252 (53.8%)	21359 (54.0%)	21192 (53.7%)	21216 (53.7%)	21271 (53.8%)	21256 (53.8%)	0.97

Notes: Grant status is an indicator for whether the student was a Cal Grant recipient, and award status is whether the student's award had been issued, which we considered a proxy for being an active student. The other available covariate was county, which was balanced across condition ($p = 1.00$) but is not shown here due to data privacy restrictions.

3.4 Results

Effect of messages

Among students who only received email outreach, the *Simplified* (T1) message yielded a small, but significant, increase in CalFresh applications relative to the *Status Quo* message. Approximately 2.7% of students who received the *Status Quo* email applied for CalFresh in the six weeks after receiving the communication, compared to 2.9% of students who received the *Simplified* (T1) email (t = 2.12, p = .03, 95% CI[0.0001-0.0048]). As shown in Table 3.3, results are similar when looking at outcomes over an eight-week period instead.

Table 3.3. Effect of treatment assignment on CalFresh applications

	(1) 6-week outcome period	(2) 8-week outcome period
Simplified (T1) – Email	0.0025**	0.0025**
	(0.0012)	(0.0012)
Simplified (T1) – Email + PC	0.0229***	0.0230***
	(0.0014)	(0.0014)
Destigmatizing (T2) – Email	0.0023*	0.0022*
	(0.0012)	(0.0012)
Destigmatizing (T2) – Email + PC	0.0204***	0.0207***
	(0.0013)	(0.0014)
Resources (T3) – Email	0.0023*	0.0021*
	(0.0012)	(0.0012)
Resources (T3) – Email + PC	0.0233***	0.0236***
	(0.0014)	(0.0014)
Observations	275,977	275,977
R-squared	0.003	0.003
Status Quo mean	0.0268	0.0293

Notes: Estimates of equation 1 on CalFresh applications during the six-week outcome period (column 1) and eight-week outcome period (column 2). All specifications include stratum fixed effects. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 3.4 presents estimates of the effect of each message relative to the *Simplified* (T1) message. On average, there was no difference in application rates between students assigned to the *Simplified* (T1), *Destigmatizing* (T2), or *Resources* (T3) conditions. When pooled across modality, 3.9% of students assigned to receive the *Simplified* (T1) message applied for CalFresh in the six-week outcome period, compared to 3.8% of students assigned to receive the *Destigmatizing* (T2) message and 3.9% of students assigned to receive the *Resources* (T3) message (joint F(2, 236969) = 1.44, p = .24).

By modality, there was no difference in application rates by message (T1 vs. T2 vs. T3) among students who only received emails (joint F(2, 118371) = 0.03, p = .98). However, among students assigned to receive both emails and postcards, students assigned to receive the *Destigmatizing* (T2) message were marginally less likely to apply in the six-week outcome period than those assigned to receive either the *Simplified* (T1) message (t = -1.64, p =.10, 95%

CI[-0.0055, 0.0004]) or the *Resources* (T3) message (t = -1.93, p = .05, 95% CI[-0.0059, 0.0000]). As shown in Appendix C (Table C1), this effect disappears when looking at applications over eight weeks.

Table 3.4. Effect of messaging on CalFresh applications

	(1) Full sample	(2) Email only	(3) Email + PC
Destigmatizing (T2)	-0.0014	-0.0002	-0.0025
	(0.0010)	(0.0012)	(0.0015)
Resources (T3)	0.0001	-0.0002	0.0005
	(0.0010)	(0.0012)	(0.0015)
Observations	237,087	118,489	118,598
R-squared	0.001	0.001	0.001
Simplified (T1) mean	0.0395	0.0293	0.0496

Notes: Estimates of the effect of assignment to the Destigmatizing or Resources message conditions on CalFresh applications during the six-week outcome period, relative to the Simplified message conditions. All specifications include stratum fixed effects. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Effect of Modality

Students who were assigned to receive both an email and postcard—regardless of treatment message—were 2 percentage points (pp), or 68 percent, more likely to apply for CalFresh in the six-week outcome period than students who were assigned to receive only an email (t = 24.99, p < .001, 95% CI[0.018, 0.021]). Overall, 2.9% of students assigned to one of the three email only conditions applied for CalFresh compared to 4.9% of those assigned to one of the three email and postcard conditions. As shown in Table 3.5, a similar pattern can be seen within each message group.

Table 3.5. Effect of communication modality on CalFresh applications

	(1) Full sample	(2) Simplified (T1)	(3) Destigmatizing (T2)	(4) Resources (T3)
Email + PC	0.0198***	0.0204***	0.0181***	0.0211***
	(0.0008)	(0.0014)	(0.0014)	(0.0014)
Observations	237,087	79,079	78,975	79,033
R-squared	0.003	0.004	0.004	0.004
Email mean	0.0291	0.0293	0.0290	0.0290

Notes: Estimates of equation 2 on CalFresh applications during the six-week outcome period. All specifications include stratum fixed effects. Columns 2-4 reflect subgroup of students assigned to each messaging condition, respectively. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

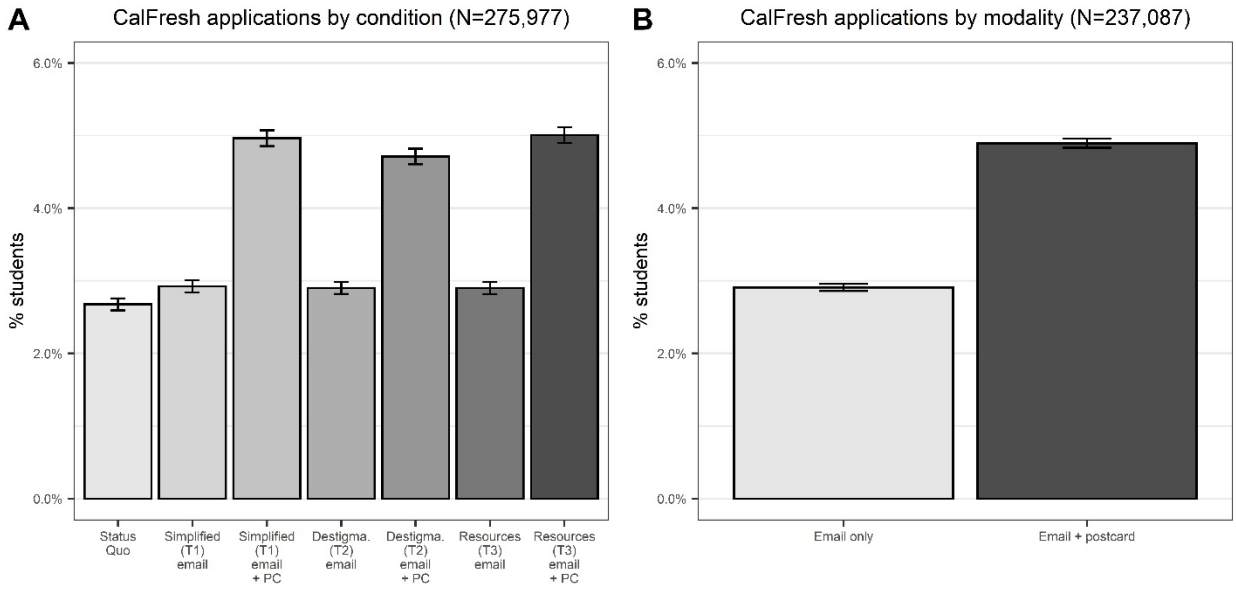
Robustness Checks

As shown in Appendix C, Tables C1-C2, results do not differ meaningfully when evaluating applications over an eight-week outcome period, as opposed to a six-week outcome period.

As a robustness check, I also evaluated each primary model excluding students who were already enrolled in CalFresh prior to the intervention, based on CDSS case data that was received after the study (N = 53,521), and students whose email addresses or postal addresses were flagged as invalid during the course of the study (N = 5,875). Of note, these exclusions were not distributed evenly by condition. Bounce backs were higher for mail than email, thus leading to larger proportions of students being excluded from the multimodal conditions ($\chi^2(6) = 1100$, $p <$ .001). It is unclear why the proportion of students who were already enrolled in CalFresh prior to the study was not distributed evenly across conditions ($\chi^2(6) = 22.03$, $p = .001$), but differences by condition were small in magnitude (1-2 pp).

As would be expected, overall application rates are higher when these students are excluded from the analysis, but otherwise, the results do not differ meaningfully from what is reported here (see Appendix C, Tables C3-C5).

Figure 3.1. Results



Notes: Regression-adjusted percent of students who applied for CalFresh via GetCalFresh.org in the six weeks following outreach (June 1-July 13, 2021) by experimental condition (A) and by modality of outreach (B). Estimates come from equation (1) and (2), respectively. Students assigned to the Status Quo condition are excluded from (B) to allow for a direct comparison of email vs. multimodal outreach. Error bars reflect +/- 1 standard error.

3.5 Discussion

Food insecurity rates among college students in the US have reached unprecedented levels, with potentially serious and far-reaching consequences on health, education, and economic outcomes. In 2021, the Federal Government temporarily expanded student eligibility for SNAP benefits in an effort to mitigate the economic consequences of the COVID-19 pandemic. However, for these benefits to effectively reduce food insecurity, eligible students must be able to access them. In a large-scale field experiment, I found that simplified messaging increased applications for CalFresh among likely eligible college students, relative to a status quo message. At the same time, messages that targeted potential psychological barriers had no additional impact over and above the simplified message. Additionally, multimodal communication (email and postcard) nearly doubled application rates compared to outreach via email alone.

These findings extend existing research on the role of administrative burdens in the context of the social safety net, and on the potential for light-touch interventions to reduce these barriers. The simplified message increased CalFresh applications by about 7% relative to the *Status Quo* message, which is in line with evidence on the average effects of behavioral interventions (DellaVigna & Linos, 2022). Communication modality had a much larger impact on program applications—nearly 36%—although the design of this experiment does not allow for a direct comparison of email versus mail-based outreach. Nevertheless, the overall take-up rate in this population was still relatively low–just 5%. While not all students included in the study sample were eligible for benefits, there remains a substantial take-up gap that will require a combination of light-touch and higher-touch methods to fully close.

This research also has several important limitations that suggest areas for further study. First, I am unable to disentangle why the *Destigmatizing* (T2) and *Resources* (T3) messages did not affect CalFresh applications relative to the *Simplified* (T1) message. It is possible that information costs were the most consequential barrier among this population, thus minimizing the effect of any additional language adjustments over and above simplification. But it is also possible that the tested messages did not effectively reduce psychological costs or that the psychological barriers targeted in T2 and T3 are not the most consequential among college students. Additional research is needed to fully understand the mechanisms underlying these results in order to inform future interventions.

Second, while multimodal outreach outperformed digital outreach in this study, both the mechanisms and the generalizability of these findings are unclear. I cannot disentangle whether multimodal outreach was more effective because of the modality of communication or because recipients received the same message twice (once via email and once via postcard), which may have functioned as a reminder. Relatedly, it is not possible to extrapolate from this research to determine the conditions under which mail-based or multimodal outreach will outperform digital outreach. Given the prevalence of mass outreach campaigns, future studies should empirically test different communication modalities in a range of contexts.

Third, this analysis is limited by data availability. Specifically, I was only able to examine the impact of outreach on program applications submitted through GetCalFresh.org. This is the call to action that was included in all outreach materials and, overall, 65% of all CalFresh applications came through GetCalFresh.org in 2020 (Code for America, 2021). Yet, it is still possible that some students applied directly through CDSS, which would not be captured in our analysis. Additionally, I am unable to measure actual program enrollment. While

GetCalFresh.org screens out applicants who do not meet basic eligibility criteria, it is unknown whether students who applied during the study were ultimately determined to be eligible to enroll in the program. Future research should examine the impact of similar outreach on the final behavioral outcome of program enrollment.

Appendix A: Supplemental Materials for Chapter 1

Supplemental Methods

Field Experiments

Study 2

Sample construction

Denver County is divided into 78 distinct neighborhoods and 144 census tracts. We identified 56 neighborhoods and 106 census tracts with populations at high risk of displacement through a four-step process that used publicly available data.

First, Denver Economic Development and Opportunity's division of Neighborhood Equity and Stabilization (NEST) identified ten neighborhoods as being at high risk of involuntary displacement due to rapid socio-economic changes (City and County of Denver (a), n.d.). All ten of these neighborhoods were included at the request of the County.

Second, Denver County also tracked vulnerability to displacement for all 78 neighborhoods. A neighborhood's vulnerability score was ranked on a scale of 0 to 3, where 3 indicates the highest level of vulnerability (City and County of Denver (b), n.d.). Scores were calculated based on a neighborhood's average educational attainment, rental occupancy, and median household income. All neighborhoods with a vulnerability score greater than 0 were included in the sample universe.

Third, the Urban Institute's Emergency Rental Assistance Priority Index estimated the risk of housing instability and homelessness by census tract (Urban Institute, 2020). Their Rental Assistance Priority Index was a weighted measure of three subindexes: housing instability, impact from COVID-19, and equity. Higher total index values indicated that a census tract was in higher need of rental assistance. The 70 highest priority census tracts in Denver County were included in the sample universe. This threshold was decided upon based on budget and resource availability constraints that limited the total number of residents that could be contacted.

Fourth, we used publicly available data from the Eviction Lab to rank each census tract in Denver County by four key predictors of vulnerability: percent of non-White residents; percent of renter households; percent of cost-burdened renters; and poverty rate (Eviction Lab, 2016). At the census-tract level, we created an equal-weighted composite rank such that the highest-ranked census tracts were those with the highest proportions of non-White, renter, cost-burdened, and poor households. The 70 highest ranked census tracts were included in the sample universe. Again, this threshold reflected budget and resource constraints.

Outcomes

In addition to the three primary outcomes described in the main paper—application requests, application submissions, and assistance received—we also obtained Denver County Court administrative data on evictions during our outcome period.

On September 4, 2020, the Centers for Disease Control (CDC) used its authority under the Public Health Service Act to issue a national eviction moratorium in order to reduce the potential for transmission of COVID-19 that can occur as displaced people double-up with

friends or family, become homeless, or turn to shelters. Initially the moratorium was set to expire on January 31, 2021. However, the moratorium was extended during our study implementation period, which hindered our ability to evaluate this outcome. While some evictions still occurred during this period, the rate was so low it is not possible to analyze—or interpret—the effect of our intervention on evictions. As a result, we do not report these results. As detailed in our pre-registered analysis plan, we anticipated this challenge ahead of time and noted our intent to only conduct an exploratory analysis of evictions if the moratorium was extended.

Deviation from analysis plan

We deviate from our pre-registered analysis plan for Study 2 by analyzing our primary outcomes via OLS models instead of logistic models. Because the overall prevalence of application requests and submissions was so low, many neighborhoods had no positive outcomes. Thus, there was significant collinearity in covariate-adjusted logistic models, leading to hundreds of dropped observations. As a result, our main manuscript reports results from covariate-adjusted linear models that include the full analytic sample. In the Supplemental Tables section, we also report results from our pre-registered models, excluding collinear neighborhoods.

Online Experiments

Standard MTurk Participant Qualifications

All studies reported utilized the same minimum qualifications for recruiting MTurk participants. In order to participate, a MTurk worker must:

1. Be located in the United States;
2. Have an approval rating of at least 95%;
3. Have not participated in prior surveys as part of this study;
4. Consent to participate;
5. And pass an initial attention check.

Only MTurk workers who met all five criteria were eligible to participate in any study reported in this paper.

Exclusion Criteria

For each online experiment, we excluded responses that met the following criteria:

- Duplicate responses based on worker ID and IP address
- Participants who failed second attention check included at the end of the survey
- Responses flagged by Qualtrics as likely fraudulent
- Responses that were not internally consistent on two household income questions: All studies included an initial screener question to ensure that we only recruited participants whose household income was less than $50,000 per year. At the end of each survey, we again asked household income. Responses from participants who provided different

answers to the screener question and the income question at the end of the survey were excluded.
- Participants who completed the survey in less than 30 seconds (pilot study) or 45 seconds (Studies 3 and 4)

All exclusion criteria were pre-registered.

Pilot Study

Participants

Participants were Amazon MTurk workers whose reported annual household income was under $50,000 and who were recruited to complete a 1-minute online survey for which they were paid $0.35 each. Standard participant qualifications were applied. A total of 676 participants (mean age = 38.3 years, *SD* = 12.0; 42.5% female) passed the attention check and completed the study. Data quality exclusions were balanced evenly across treatment conditions ($\chi^2(2) = 4.30$, $p = .51$). After all exclusions, our final analytic sample consisted of 493 participants (mean age = 39.2 years, *SD* = 12.6; 44.6% female).

Procedures

After passing an initial attention check, all participants were randomly assigned via the survey platform to one of six conditions, each associated with a different stigmatized means-tested program or attribute: (1) rental assistance; (2) Medicaid; (3) Social Security Disability Insurance; (4) Supplemental Nutrition Assistance Program; (5) obesity; (6) mental illness. Participants were then asked to what extent they agreed or disagreed with the following eight statements (presented in random order) about the program or attribute corresponding with their condition assignment:

1. *Shame [IS]:* I would be ashamed [*X*].
2. *Down [AS]:* Most people would look down on me if I [*X*].
3. *Judge [AS]:* If I [*X*], others would judge me.
4. *Less [IS]:* I would think less of myself if I [*X*].
5. *Stereotype [AS]:* If someone were to find out I [*X*], they would think I [*common stereotype*].
6. *Fault [AS]:* Most people would think it was my fault if I [*X*].
7. *Deal [IS]:* I would rather deal with my problems myself than [*X*].
8. *Inferior [IS]:* If I were [*X*], I would feel inferior.

Agreement for each question was measured on a 1-7 scale in which a 1 reflected "strongly disagree" and a 7 reflected "strongly agree." In each question [*X*] reflected the participant's randomly assigned program or attribute. For instance, participants assigned to the rental assistance condition were shown statements such as: "I would be ashamed if I applied for rental assistance" and "I would rather deal with my problems myself than apply for rental assistance." Meanwhile, participants assigned to the obesity condition were shown statements such as: "I would be ashamed if I were obese" and "I would rather deal with my problems myself than accept help if I were obese."

The [*common stereotype*] in Question 5 was as follows:

- For all conditions associated with a means-tested program: "If someone were to find out I applied for [*program name*], they would think I lack a work ethic.
- For obesity: "If someone were to find out I were obese, they would think I was lazy."
- For mental illness: "If someone were to find out I had a mental illness, they would think I was weak."

Questions denoted with [IS] constitute a measure of internalized stigma, while questions denoted with [AS] constitute a measure of anticipated stigma.

Analysis

Prior to collecting any outcome data, we pre-registered an analysis plan on OSF (https://osf.io/surhm/). All participants were asked eight stigma measures, four about anticipated stigma and four about internalized stigma. Each was measured on a 7-point scale in which a 7 reflects high stigma and a 1 reflects low stigma. As our primary outcomes, we constructed three indices: overall stigma, anticipated stigma, and internalized stigma. Each was calculated as the average of their respective stigma measures. We evaluated differences in the stigma associated with each program and attribute via a covariate-adjusted OLS model that includes controls for gender, age, college education, race/ethnicity, income, party affiliation.

Study 3

Procedures

In Study 3, all participants who passed an initial attention check were randomly assigned to one of two conditions with equal probability: *Information Only* or *Information + Stigma*. Participants were then shown the postcard from Study 2 that corresponded with their condition assignment. Thereafter, all participants were asked the following nine questions:

To what extent do you agree or disagree with each of the following statements:

1. *Shame [IS]:* I would be ashamed to apply for the rental assistance program.
2. *Down [AS]:* Most people would look down on me if I applied for the rental assistance program.
3. *Judge [AS]:* If I applied for the rental assistance program, others would judge me.
4. *Less [IS]:* I would think less of myself if I applied for the rental assistance program.
5. *Stereotype [AS]:* If someone were to find out I applied for the rental assistance program, they would think I lack a work ethic.
6. *Fault [AS]:* Most people would think it was my fault if I needed to apply for the rental assistance program.
7. *Deal [IS]:* I would rather deal with my problems myself than apply for the rental assistance program.
8. *Inferior [IS]:* If I were to apply for the rental assistance program, I would feel inferior.

9. *Apply:* If you were eligible, how likely would you be to apply for the rental assistance program after receiving this postcard? [Scale of 1-7, where 7 = Extremely likely]
10. *Easy:* How easy do you think it would be to apply for the rental assistance program on a scale from 1 to 10, where 10 = extremely difficult? [1-10 scale]

Questions 1-8 were presented in a random order and each measured on a 1-7 scale in which a 1 reflected "strongly disagree" and a 7 reflected "strongly agree." Questions denoted with [IS] constitute a measure of internalized stigma, while questions denoted with [AS] constitute a measure of anticipated stigma.

Study 4

Procedures

In Study 4, all participants who passed an initial attention check were randomly assigned to one of two conditions with equal probability: *Information Only* or *Information + Stigma*. Participants were then shown the postcard from Study 2 that corresponded with their condition assignment. Thereafter, all participants were asked the following four questions:

1. *Easy:* How easy do you think it would be to apply for the rental assistance program on a scale from 1 to 10, where 10 = extremely difficult? [1-10 scale]
2. *Receive:* If you were to apply for the rental assistance program, how likely do you think it is that you would receive money? [1-5 scale, 5 = Very likely]
3. *Credible:* To what extent do you agree or disagree with the following statement: This postcard is from a credible source. [1-5 scale, 5 = Strongly agree]
4. *Comprehension:* This postcard is advertising a program that offers which of the following services: *[answer choices presented in random order]*
    a. Temporary rent and utility assistance
    b. Eviction legal assistance
    c. Long-term housing assistance
    d. Housing choice voucher assistance
    e. Rental search assistance

Supplemental Tables

Table A1. Study 2: Balance of randomized universe

Level	Control	Information Only	Info + Stigma	p-value
N	12066	25389	25260	
Excluded address	38 (0.3%)	74 (0.3%)	74 (0.3%)	0.92
Apartment building	6320 (52.4%)	13611 (53.6%)	13513 (53.5%)	0.07
DEMOGRAPHICS (CENSUS TRACT)				
Eviction rate, median (IQR)	1.48 (1.1, 2.67)	1.45 (1.02, 2.67)	1.45 (1.1, 2.67)	0.25
% cost burdened, median (IQR)	.58 (.48, .68)	.58 (.48, .68)	.58 (.48, .67)	0.16
% below poverty line, median (IQR)	.15 (.11, .20)	.15 (.11, .20)	.15 (.11, .20)	0.55
% White, median (IQR)	70.3 (33.8, 80.6)	71.7 (33.8, 80.6)	70.3 (33.8, 80.6)	0.12
NONPROFIT				
1	5440 (45.1%)	11694 (46.1%)	11690 (46.3%)	0.06
2	4899 (40.6%)	10164 (40.0%)	10192 (40.3%)	
3	1727 (14.3%)	3531 (13.9%)	3378 (13.4%)	
NEIGHBORHOOD				
ATHMAR PARK	108 (0.9%)	246 (1.0%)	246 (1.0%)	1.00
BAKER	183 (1.5%)	359 (1.4%)	376 (1.5%)	
BARNUM	92 (0.8%)	180 (0.7%)	189 (0.7%)	
BARNUM WEST	65 (0.5%)	134 (0.5%)	127 (0.5%)	
BEAR VALLEY	84 (0.7%)	164 (0.6%)	173 (0.7%)	
CAPITOL HILL	966 (8.0%)	2200 (8.7%)	2200 (8.7%)	
CBD	133 (1.1%)	259 (1.0%)	273 (1.1%)	
CHAFFEE PARK	58 (0.5%)	119 (0.5%)	114 (0.5%)	
CHEESMAN PARK	530 (4.4%)	1207 (4.8%)	1208 (4.8%)	
CITY PARK	267 (2.2%)	549 (2.2%)	524 (2.1%)	
CITY PARK WEST	326 (2.7%)	636 (2.5%)	669 (2.6%)	
CIVIC CENTER	45 (0.4%)	93 (0.4%)	88 (0.3%)	
CLAYTON	67 (0.6%)	137 (0.5%)	129 (0.5%)	
COLE	97 (0.8%)	199 (0.8%)	189 (0.7%)	
COLLEGE VIEW - SOUTH PLATTE	143 (1.2%)	280 (1.1%)	293 (1.2%)	
CONGRESS PARK	1105 (9.2%)	2516 (9.9%)	2516 (10.0%)	
DIA	44 (0.4%)	87 (0.3%)	91 (0.4%)	
EAST COLFAX	452 (3.7%)	926 (3.6%)	881 (3.5%)	
ELYRIA SWANSEA	127 (1.1%)	289 (1.1%)	289 (1.1%)	
FIVE POINTS	228 (1.9%)	451 (1.8%)	473 (1.9%)	
GATEWAY - GREEN VALLEY RANCH	269 (2.2%)	552 (2.2%)	526 (2.1%)	
GLOBEVILLE	81 (0.7%)	183 (0.7%)	183 (0.7%)	
GOLDSMITH	62 (0.5%)	121 (0.5%)	128 (0.5%)	
HALE	754 (6.2%)	1535 (6.0%)	1479 (5.9%)	
HAMPDEN	243 (2.0%)	499 (2.0%)	473 (1.9%)	
HARVEY PARK	150 (1.2%)	294 (1.2%)	308 (1.2%)	
HARVEY PARK SOUTH	61 (0.5%)	125 (0.5%)	118 (0.5%)	
HIGHLAND	429 (3.6%)	837 (3.3%)	879 (3.5%)	
JEFFERSON PARK	179 (1.5%)	348 (1.4%)	367 (1.5%)	
LINCOLN PARK	121 (1.0%)	237 (0.9%)	248 (1.0%)	
MAR LEE	157 (1.3%)	355 (1.4%)	355 (1.4%)	
MONTBELLO	302 (2.5%)	589 (2.3%)	620 (2.5%)	
NORTH CAPITOL HILL	238 (2.0%)	464 (1.8%)	487 (1.9%)	
NORTH PARK HILL	75 (0.6%)	154 (0.6%)	147 (0.6%)	
NORTHEAST PARK HILL	229 (1.9%)	469 (1.8%)	446 (1.8%)	

OVERLAND	46 (0.4%)	95 (0.4%)	90 (0.4%)
REGIS	146 (1.2%)	300 (1.2%)	285 (1.1%)
RUBY HILL	147 (1.2%)	301 (1.2%)	286 (1.1%)
SKYLAND	73 (0.6%)	150 (0.6%)	144 (0.6%)
SLOAN LAKE	117 (1.0%)	264 (1.0%)	264 (1.0%)
SOUTHMOOR PARK	6 (<1%)	12 (<1%)	12 (<1%)
SPEER	586 (4.9%)	1274 (5.0%)	1248 (4.9%)
SUN VALLEY	73 (0.6%)	143 (0.6%)	150 (0.6%)
SUNNYSIDE	194 (1.6%)	377 (1.5%)	397 (1.6%)
UNION STATION	174 (1.4%)	357 (1.4%)	339 (1.3%)
UNIVERSITY	268 (2.2%)	610 (2.4%)	610 (2.4%)
UNIVERSITY HILLS	35 (0.3%)	68 (0.3%)	72 (0.3%)
UNIVERSITY PARK	187 (1.5%)	384 (1.5%)	365 (1.4%)
VALVERDE	36 (0.3%)	83 (0.3%)	83 (0.3%)
VILLA PARK	165 (1.4%)	323 (1.3%)	339 (1.3%)
VIRGINIA VILLAGE	171 (1.4%)	351 (1.4%)	333 (1.3%)
WASHINGTON PARK WEST	174 (1.4%)	357 (1.4%)	340 (1.3%)
WASHINGTON VIRGINIA VALE	114 (0.9%)	233 (0.9%)	221 (0.9%)
WEST COLFAX	442 (3.7%)	1007 (4.0%)	1007 (4.0%)
WESTWOOD	220 (1.8%)	453 (1.8%)	432 (1.7%)
WINDSOR	222 (1.8%)	454 (1.8%)	431 (1.7%)

Notes: Includes 186 addresses that were randomized, but later found to be duplicates and excluded from the final analytic universe.

Table A2. Study 2 results: application requests, full analytic universe

	(1)	(2)	(3)	(4)
	Full treatment		Pooled treatment	
	Logistic	OLS	Logistic	OLS
Treatment pooled			0.5013***	0.0041***
			(0.1228)	(0.0009)
Information Only	0.4555***	0.0036***		
	(0.1310)	(0.0010)		
Information + Stigma	0.5453***	0.0045***		
	(0.1300)	(0.0010)		
Percent rent burdened	-0.0175	-0.0002	-0.0175	-0.0002
	(0.0146)	(0.0002)	(0.0146)	(0.0002)
Poverty rate	-0.0181	-0.0002*	-0.0181	-0.0002*
	(0.0099)	(0.0001)	(0.0099)	(0.0001)
Percent non-White	2.5426*	0.0200*	2.5494*	0.0200*
	(1.2530)	(0.0084)	(1.2530)	(0.0084)
Observations	61,658	62,528	61,658	62,528
R-squared		0.0102		0.0102
Control mean	0.00643	0.00631	0.00643	0.00631

Notes: Estimates of the average effect of treatment assignment on application requests in the eight weeks following the mailing date. One observation is excluded from OLS models due to missing covariate data. Additional observations excluded from logistic models due to collinearity of neighborhoods and outcome (see Supplemental Methods). Additional controls not shown include neighborhood, nonprofit organization, and an indicator for whether the address was part of an apartment building. Robust standard errors in parentheses. *** p<0.001, ** p<0.01, * p<0.05

Table A3. Study 2 results: application requests, nonprofit #2

	(1)	(2)	(3)	(4)
	Full treatment		Pooled treatment	
	Logistic	OLS	Logistic	OLS
Treatment pooled			0.5077**	0.0060***
			(0.1606)	(0.0016)
Information Only	0.4204*	0.0047**		
	(0.1723)	(0.0018)		
Information + Stigma	0.5886***	0.0072***		
	(0.1694)	(0.0018)		
Percent rent burdened	0.0154	0.0003	0.0154	0.0003
	(0.0233)	(0.0003)	(0.0233)	(0.0003)
Poverty rate	-0.0249	-0.0004*	-0.0247	-0.0004*
	(0.0134)	(0.0002)	(0.0134)	(0.0002)
Percent non-White	3.6291	0.0712	3.6296	0.0713
	(2.2495)	(0.0374)	(2.2530)	(0.0374)
Observations	24,564	25,229	24,564	25,229
R-squared		0.0118		0.0117
Control mean	0.00944	0.00914	0.00944	0.00914

Notes: Estimates of the average effect of treatment assignment on application requests in the eight weeks following the mailing date. Sample is all addresses associated with the only administering nonprofit organization that tracked all incoming requests (*N* = 25,229). Observations excluded from logistic models due to collinearity of neighborhoods and outcome (see Supplemental Methods). Additional controls not shown include neighborhood, nonprofit organization, and an indicator for whether the address was part of an apartment building. Robust standard errors in parentheses. *** p<0.001, ** p<0.01, * p<0.05

Table A4. Study 2 results: submitted applications, full analytic universe

	(1)	(2)	(3)	(4)
	Full treatment		Pooled treatment	
	Logistic	OLS	Logistic	OLS
Treatment pooled			0.2659	0.0016*
			(0.1367)	(0.0008)
Information Only	0.2134	0.0013		
	(0.1479)	(0.0008)		
Information + Stigma	0.3160*	0.0020*		
	(0.1461)	(0.0009)		
Percent rent burdened	-0.0047	0.0000	-0.0046	0.0000
	(0.0167)	(0.0001)	(0.0167)	(0.0001)
Poverty rate	-0.0124	-0.0001	-0.0125	-0.0001
	(0.0127)	(0.0001)	(0.0127)	(0.0001)
Percent non-White	4.3100***	0.0332***	4.3160***	0.0333***
	(1.1562)	(0.0096)	(1.1561)	(0.0096)
Observations	60,394	62,528	60,394	62,528
R-squared		0.0047		0.0047
Control mean	0.00550	0.00530	0.00550	0.00530

Notes: Estimates of the average effect of treatment assignment on application submissions in the eight weeks following the mailing date. One observation is excluded from OLS models due to missing covariate data. Additional observations excluded from logistic models due to collinearity of neighborhoods and outcome (see Supplemental Methods). Additional controls not shown include neighborhood, nonprofit organization, and an indicator for whether the address was part of an apartment building. Robust standard errors in parentheses. *** p<0.001, ** p<0.01, * p<0.05

Table A5. Study 2 results: Assistance received prior to April 2021

	(1)	(2)	(3)	(4)
	Full treatment		Pooled treatment	
	Logistic	OLS	Logistic	OLS
Treatment pooled			0.5483**	0.0021***
			(0.1834)	(0.0006)
Information Only	0.5036**	0.0019**		
	(0.1950)	(0.0006)		
Information + Stigma	0.5914**	0.0023***		
	(0.1934)	(0.0007)		
Percent rent burdened	-0.0043	-0.0000	-0.0041	-0.0000
	(0.0217)	(0.0001)	(0.0217)	(0.0001)
Poverty rate	-0.0299	-0.0002*	-0.0300	-0.0002*
	(0.0158)	(0.0001)	(0.0158)	(0.0001)
Percent non-White	5.2955***	0.0319***	5.3003***	0.0319***
	(1.3737)	(0.0089)	(1.3743)	(0.0089)
Observations	53,197	62,528	53,197	62,528
R-squared		0.0041		0.0041
Control mean	0.00334	0.00284	0.00334	0.00284

Notes: Estimates of the average effect of treatment assignment on receipt of rental assistance funds following the mailing date. One observation is excluded from OLS models due to missing covariate data. Additional observations excluded from logistic models due to collinearity of neighborhoods and outcome (see Supplemental Methods). Additional controls not shown include neighborhood, nonprofit organization, and an indicator for whether the address was part of an apartment building. Robust standard errors in parentheses. *** p<0.001, ** p<0.01, * p<0.05

Table A6. Study 2: Distribution of submitted applications, by race

	AI/AN/NH/PI	Asian	Black	Multi	White	Total
Control	2	2	1	1	16	22
	9.1%	9.1%	4.6%	4.6%	72.7%	100%
Information Only	3	1	11	0	48	63
	4.8%	1.6%	17.5%	0%	76.2%	100%
Info + Stigma	3	3	14	2	31	53
	5.7%	5.7%	26.4%	3.8%	58.5%	100%
Total	8	6	26	3	95	138
	5.8%	4.4%	18.1%	2.2%	68.8%	100%

Notes: Racial distribution of submitted applications that included race data. AI/AN/NH/PI reflects American Indian; Alaskan Native; Native Hawaiian; Pacific Islander

Table A7. Study 2: Distribution of submitted applications, by ethnicity

	Not Hispanic	Hispanic	Total
Control	18	13	31
	58.1%	41.9%	100%
Information Only	44	38	82
	53.7%	46.3%	100%
Info + Stigma	39	43	82
	47.6%	52.4%	100%
Total	101	94	138
	51.8%	48.2%	100%

Notes: Racial distribution of submitted applications that included race data. AI/AN/NH/PI reflects American Indian; Alaskan Native; Native Hawaiian; Pacific Islander

Table A8. Study 2: Missingness of race and ethnicity among submitted applications, by treatment

	Total N applied	N/% missing race	N missing ethnicity
Control	64	42	33
		65.6%	51.6%
Information Only	166	103	84
		62.1%	50.6%
Info + Stigma	183	130	101
		71.0%	55.2%
Total	413	275	218
		66.6%	52.8%

Notes: Columns indicate the number and percent of submitted applications that were missing race or ethnicity data, by treatment condition. Missingness across conditions is not significant for either race ($\chi^2(2) = 3.19$, $p = .20$) or ethnicity ($\chi^2(2) = 0.78$, $p = .68$).

Table A9. Study 2: Pre-registered test of sharp null hypothesis of no treatment effect

	Effect/ Test statistic	p-value
Application requests (Nonprofit #2)		
Info Only vs. Control	0.0047	.01
Info + Stigma vs. Control	0.0072	<.001
Info Only vs. Info + Stigma	0.0025	.16
Treatment pooled vs. Control	0.0060	.002
Joint significance of assignment to either treatment condition (F-test)	7.67	<.001
Application submissions		
Info Only vs. Control	0.0013	.14
Info + Stigma vs. Control	0.002	.03
Info Only vs. Info + Stigma	0.0007	.34
Treatment pooled vs. Control	0.0016	.05
Joint significance of assignment to either treatment condition (F-test)	4.69	0.01

Notes: Results from Fisher's randomization inference test of sharp null hypothesis for each pairwise comparison and both primary outcomes.

Table A10. Pilot study results

	(1)	(2)	(3)
VARIABLES	Stigma Index	AS Index	IS index
Medicaid	-0.669**	-0.528*	-0.809**
	(0.221)	(0.223)	(0.251)
SNAP	-0.313	-0.212	-0.414
	(0.220)	(0.227)	(0.251)
SSDI	-0.405	-0.446*	-0.363
	(0.211)	(0.217)	(0.242)
Obesity	0.699***	0.747***	0.651**
	(0.196)	(0.200)	(0.224)
Mental Illness	-0.176	-0.220	-0.132
	(0.207)	(0.218)	(0.230)
Observations	493	493	493
R-squared	0.171	0.156	0.159
Mean for Rental Asst.	4.561	4.764	4.357

Notes: OLS estimates of differences between rental assistance (reference group) and other means-tested programs and stigmatized attributes. Column (1) is constructed as the average of all 8 stigma measures described in the Supplemental Methods; Column (2) is an average of all 4 anticipated stigma measures; and column (3) is an average of all 4 internalized stigma measures. Additional controls include income, age, gender, college education, race, and party. Robust standard errors in parentheses. *** p<0.001, ** p<0.01, * p<0.05

Table A11. Study 3 results, indices

	(1) Stigma Index	(2) AS Index	(3) IS Index
Info + Stigma	-0.239*	-0.178	-0.300*
	(0.113)	(0.116)	(0.127)
Observations	622	622	622
R-squared	0.205	0.180	0.195
Mean for Info Only	4.470	4.615	4.325

Notes: OLS estimates of average treatment effect on overall stigma, anticipated stigma, and internalized stigma in Study 3. Column (1) is constructed as the average of all 8 stigma measures; Column (2) is an average of all 4 anticipated stigma measures; and column (3) is an average of all 4 internalized stigma measures. Additional controls include income, age, gender, college education, race, party, prior experience with housing insecurity, and prior experience using rental assistance. Robust standard errors in parentheses. *** p<0.001, ** p<0.01, * p<0.05

Table A12. Study 3 results, by individual measure

	(1) Shame	(2) Down	(3) Judge	(4) Less	(5) Stereotype	(6) Fault	(7) Deal	(8) Inferior	(9) Apply	(10) Easy
Info + Stigma	-0.385**	-0.166	-0.140	-0.355*	-0.188	-0.217	-0.225	-0.236	0.080	0.201
	(0.147)	(0.130)	(0.132)	(0.146)	(0.136)	(0.136)	(0.139)	(0.148)	(0.127)	(0.188)
Observations	622	622	622	622	622	622	622	622	622	622
R-squared	0.158	0.170	0.146	0.207	0.159	0.119	0.135	0.151	0.121	0.061
Mean for Info Only	4.171	4.541	4.734	4.305	4.579	4.605	4.498	4.327	5.169	5.551

Notes: OLS estimates of average treatment effect on each individual stigma measure in Study 3, as well as likelihood of applying (column (9)) and perceived difficulty of applying (column (10)). See Supplemental Methods for question text. Additional controls include income, age, gender, college education, race, party, prior experience with housing insecurity, and prior experience using rental assistance. Robust standard errors in parentheses. *** p<0.001, ** p<0.01, * p<0.05

Table A13. Study 4 results

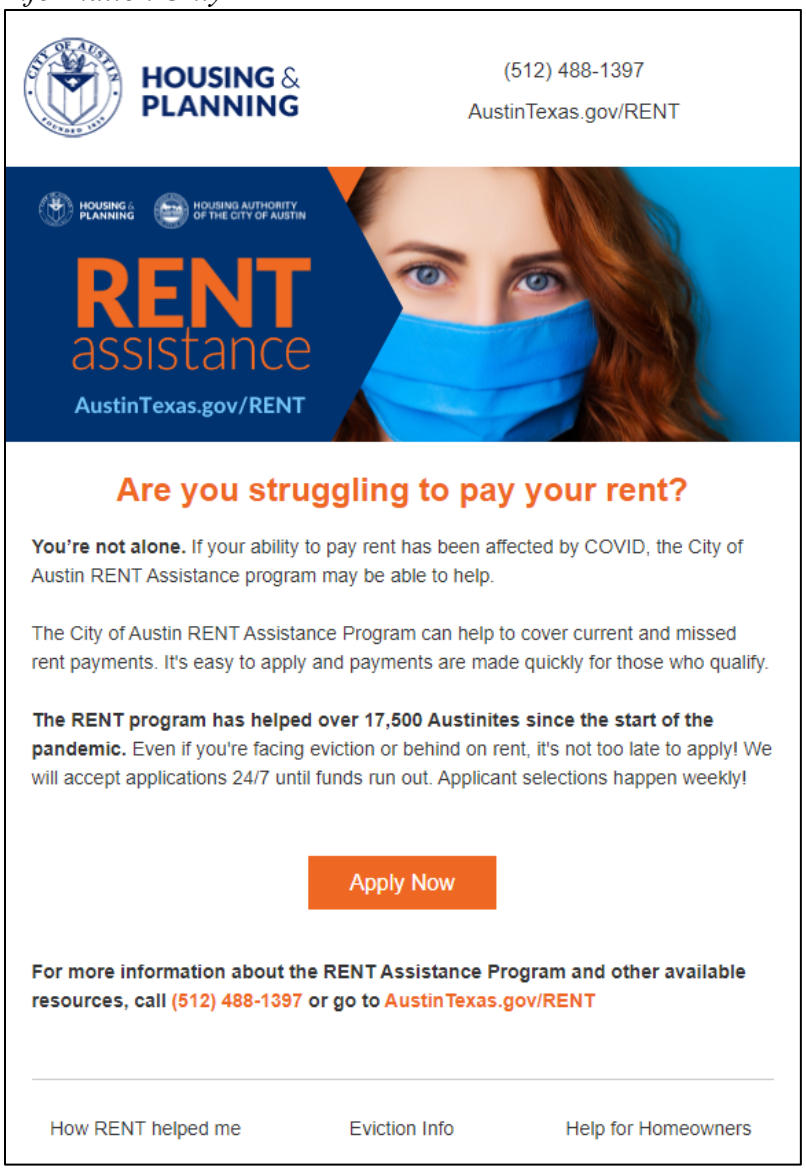
	(1) Easy	(2) Receive	(3) Credible	(4) Comprehension
Info + Stigma	-0.124	0.106	-0.177*	0.242
	(0.188)	(0.091)	(0.089)	(0.232)
Observations	636	636	636	628
R-squared	0.043	0.166	0.132	
Mean for Info Only	5.709	3.053	3.396	0.769

Notes: Estimates of average treatment effect on each outcome in Study 4. See Supplemental Methods for question text. Columns (1)-(3) reflect OLS estimates; Column (4) reflects logistic estimates. Eight observations excluded from model (4) due to collinearity between gender and the outcome. Additional controls include income, age, gender, college education, race, party, prior experience with housing insecurity, and prior experience using rental assistance. Robust standard errors in parentheses. *** p<0.001, ** p<0.01, * p<0.05

Supplemental Figures

Figure A1. Study 1 emails

Information Only email



Information + Stigma email

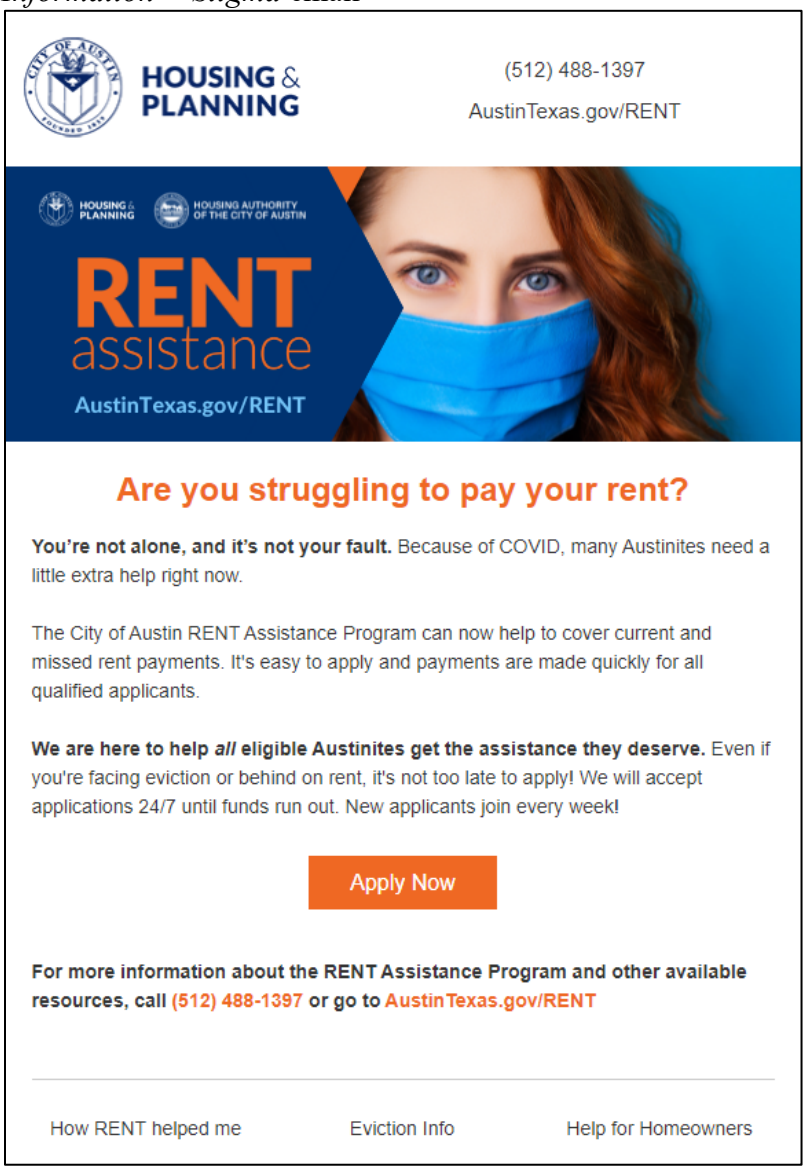
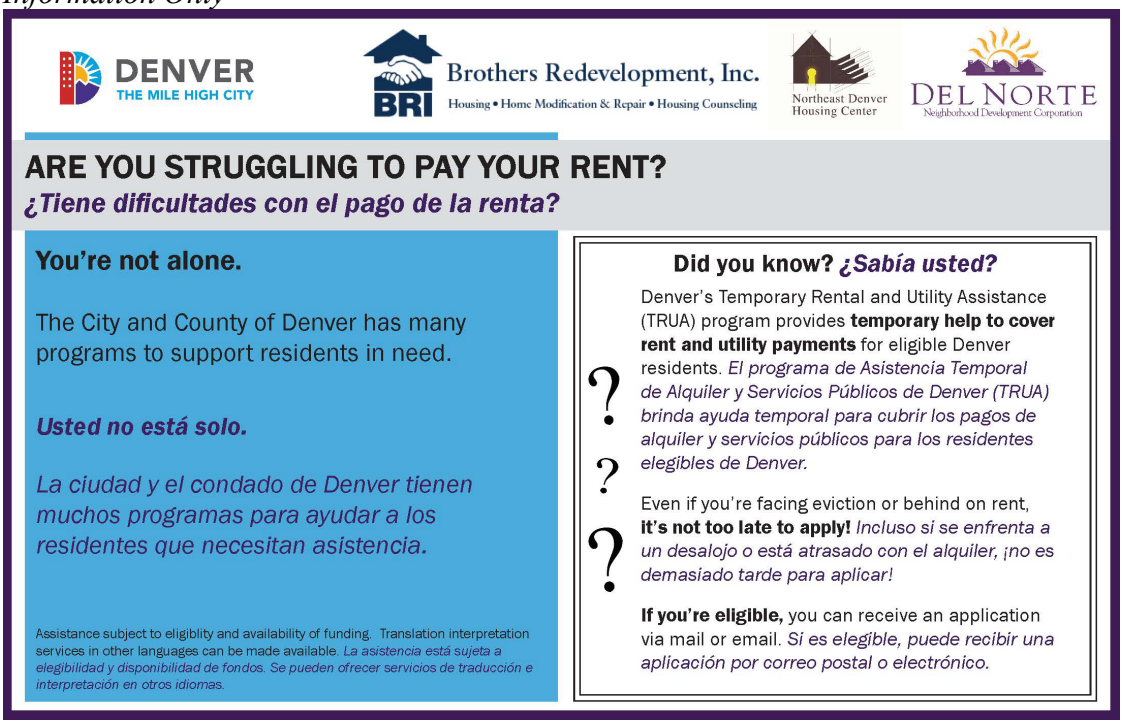


Figure A2. Study 2 postcards - front

Information Only



Information + *Stigma* (red boxes highlight language changes)

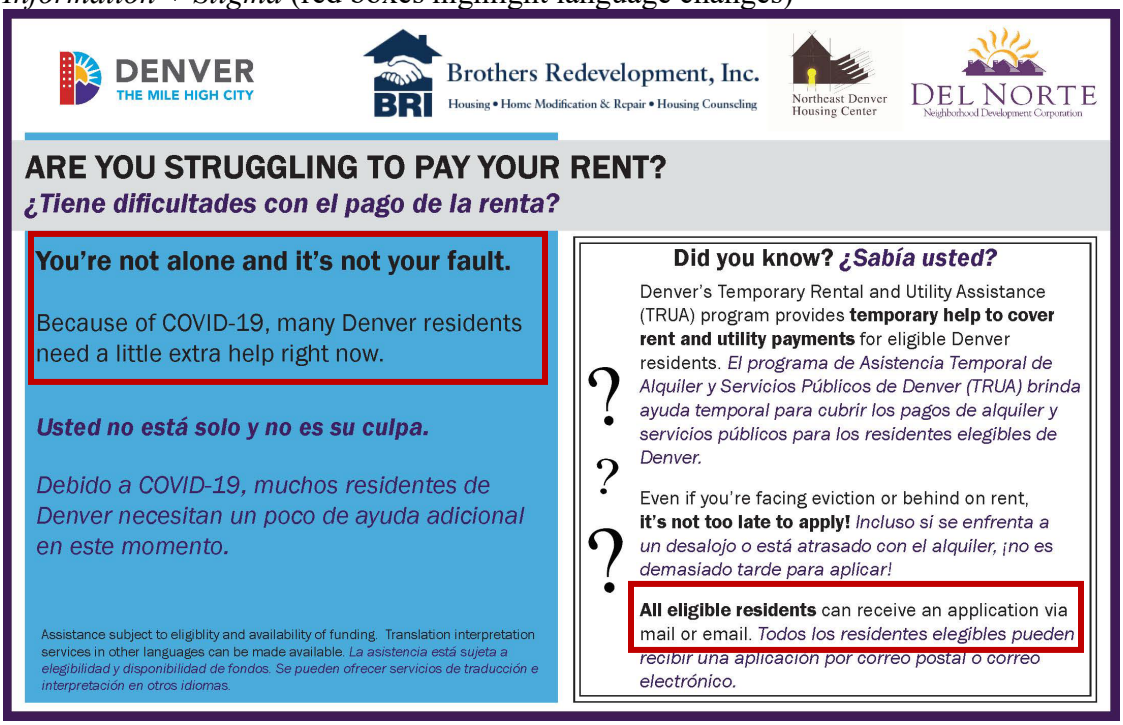
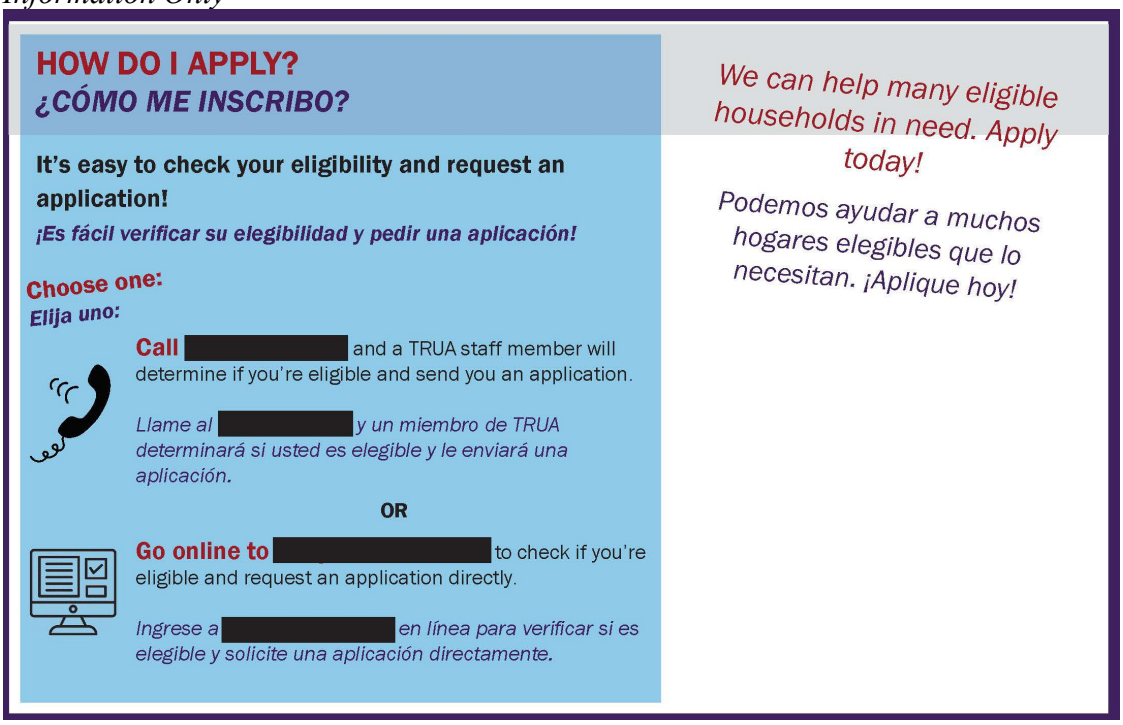
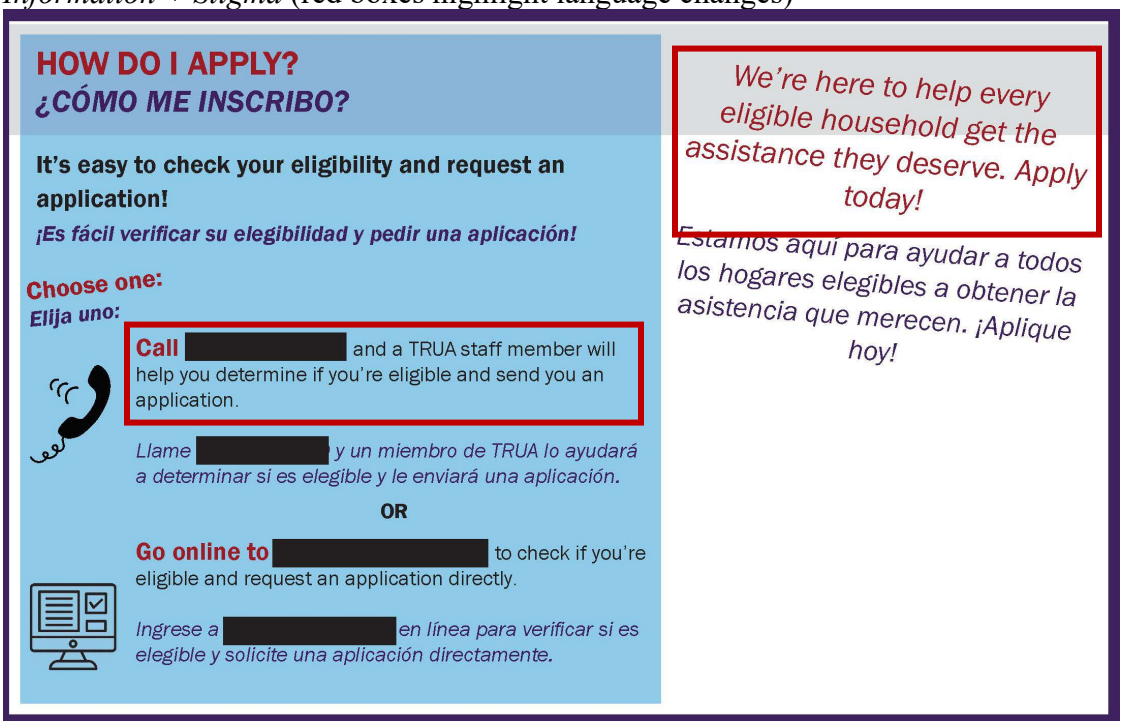


Figure A3. Study 2 postcards - back

Information Only



Information + Stigma (red boxes highlight language changes)

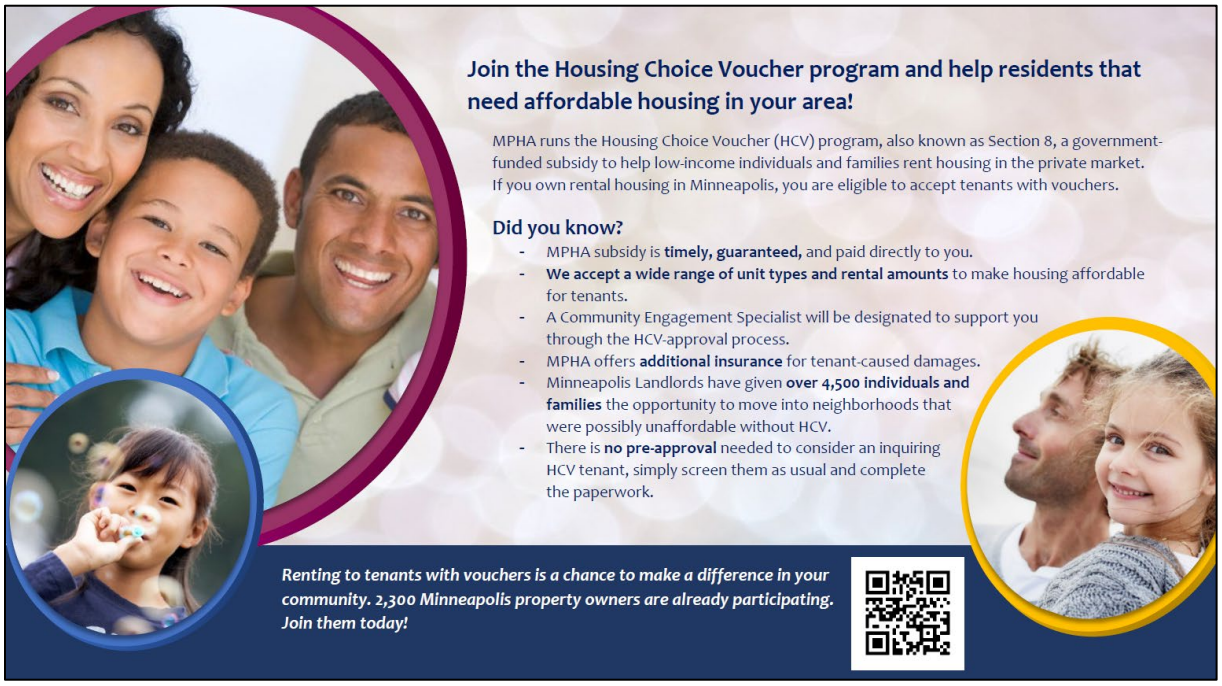


Appendix B: Supplemental Materials for Chapter 2

Supplemental Figures

Figure B1. Status quo mailer

Front



Back

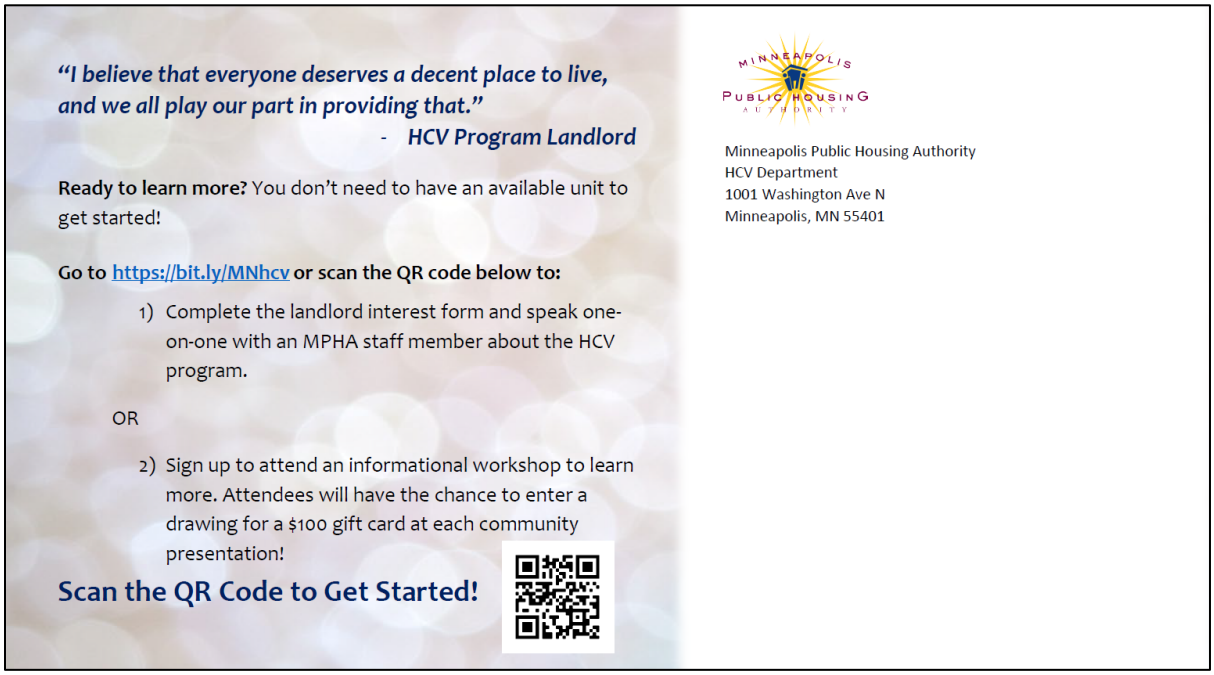


Figure B2. Process mailer

Front

Back

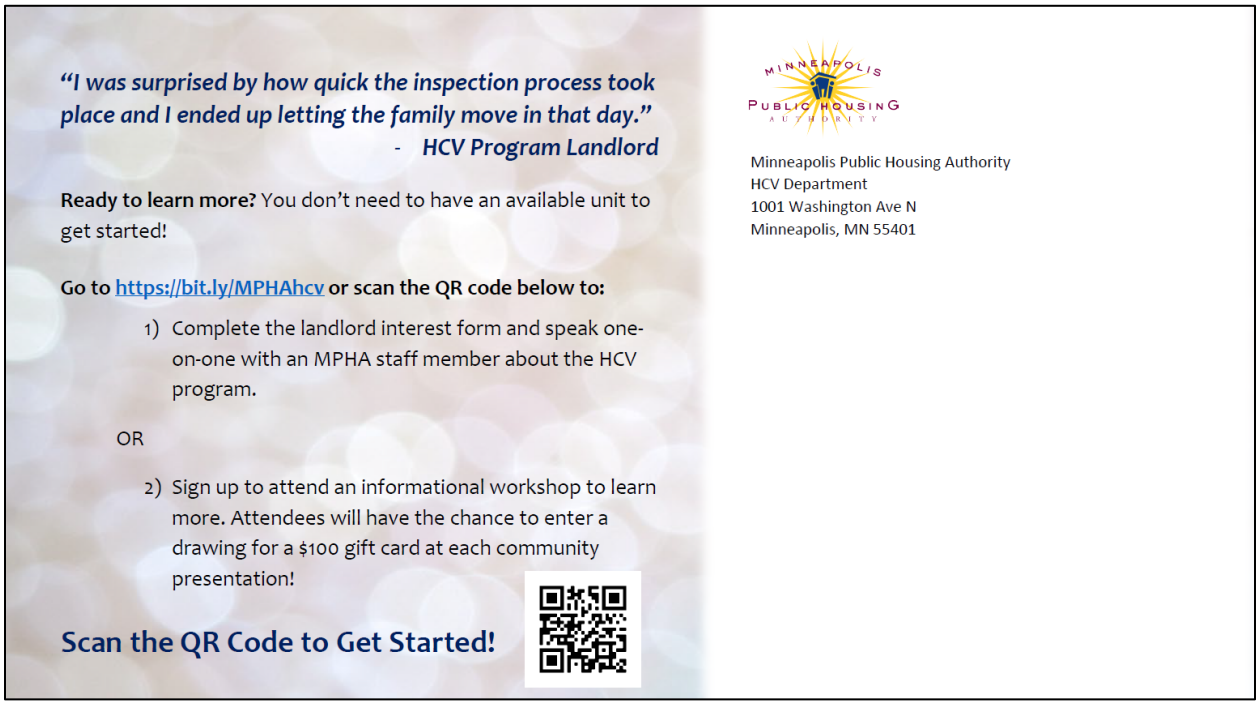
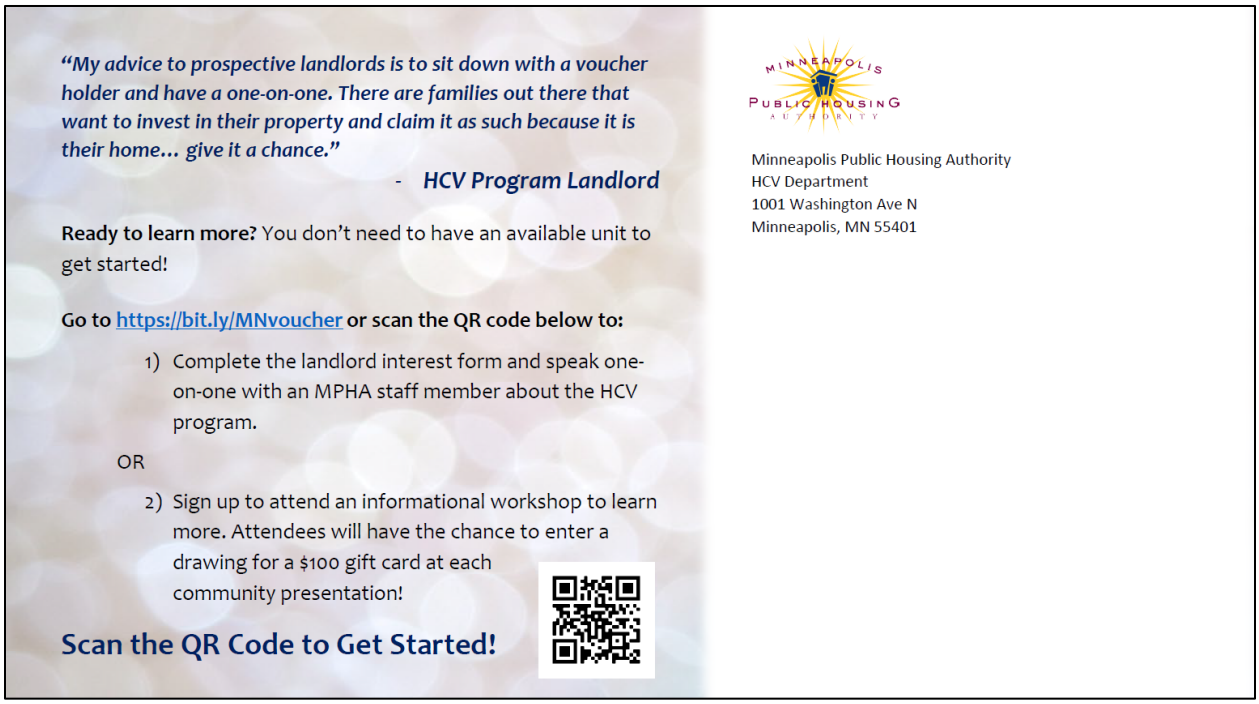


Figure B3. Destigmatizing mailer

Front

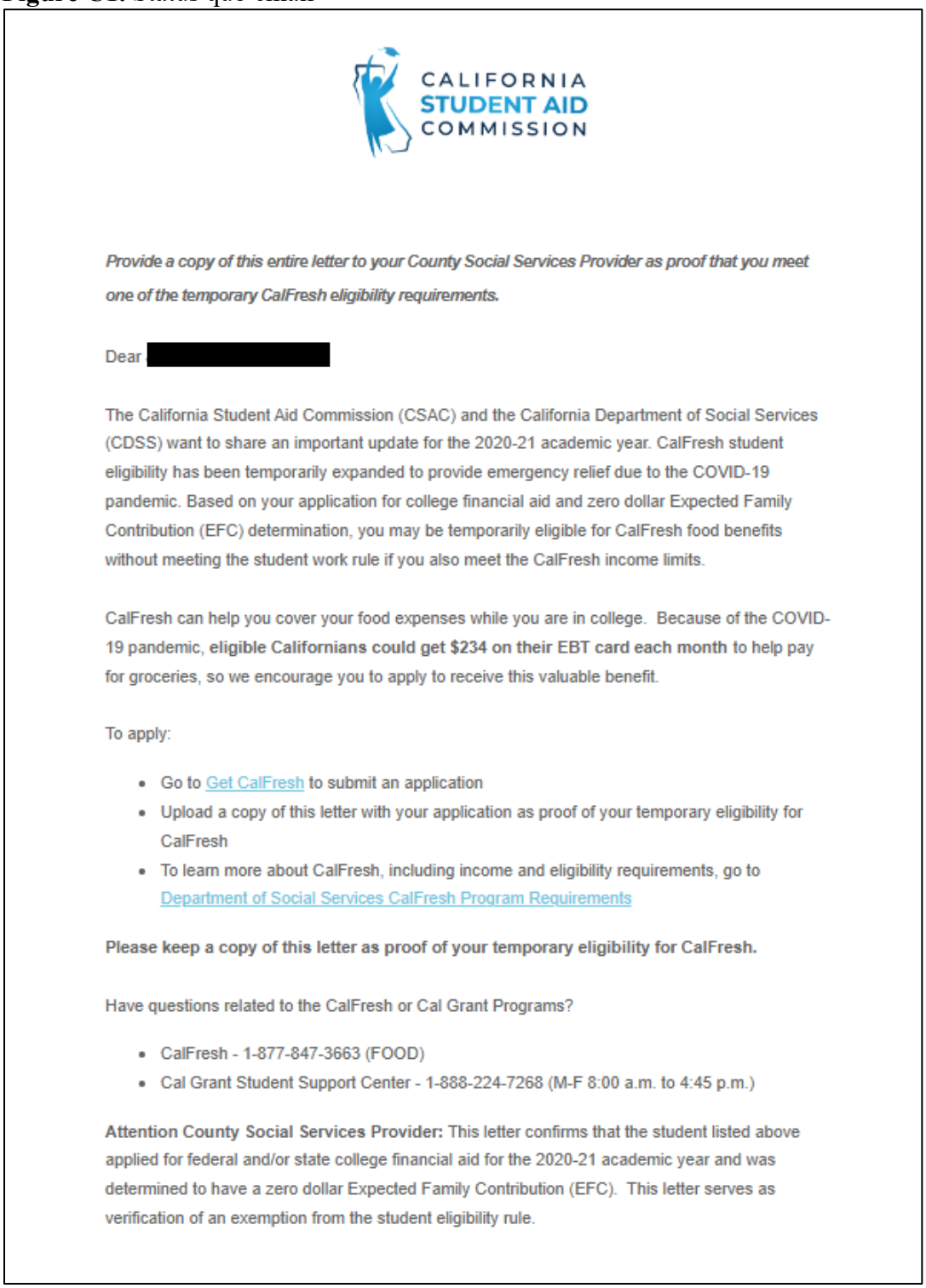
Back



Appendix C: Supplemental Materials for Chapter 3

Supplemental Materials

Figure C1. Status quo email



CALIFORNIA STUDENT AID COMMISSION

Provide a copy of this entire letter to your County Social Services Provider as proof that you meet one of the temporary CalFresh eligibility requirements.

Dear [redacted]

The California Student Aid Commission (CSAC) and the California Department of Social Services (CDSS) want to share an important update for the 2020-21 academic year. CalFresh student eligibility has been temporarily expanded to provide emergency relief due to the COVID-19 pandemic. Based on your application for college financial aid and zero dollar Expected Family Contribution (EFC) determination, you may be temporarily eligible for CalFresh food benefits without meeting the student work rule if you also meet the CalFresh income limits.

CalFresh can help you cover your food expenses while you are in college. Because of the COVID-19 pandemic, **eligible Californians could get $234 on their EBT card each month** to help pay for groceries, so we encourage you to apply to receive this valuable benefit.

To apply:

- Go to Get CalFresh to submit an application
- Upload a copy of this letter with your application as proof of your temporary eligibility for CalFresh
- To learn more about CalFresh, including income and eligibility requirements, go to Department of Social Services CalFresh Program Requirements

Please keep a copy of this letter as proof of your temporary eligibility for CalFresh.

Have questions related to the CalFresh or Cal Grant Programs?

- CalFresh - 1-877-847-3663 (FOOD)
- Cal Grant Student Support Center - 1-888-224-7268 (M-F 8:00 a.m. to 4:45 p.m.)

Attention County Social Services Provider: This letter confirms that the student listed above applied for federal and/or state college financial aid for the 2020-21 academic year and was determined to have a zero dollar Expected Family Contribution (EFC). This letter serves as verification of an exemption from the student eligibility rule.

Figure C2. Simplified email (T1)

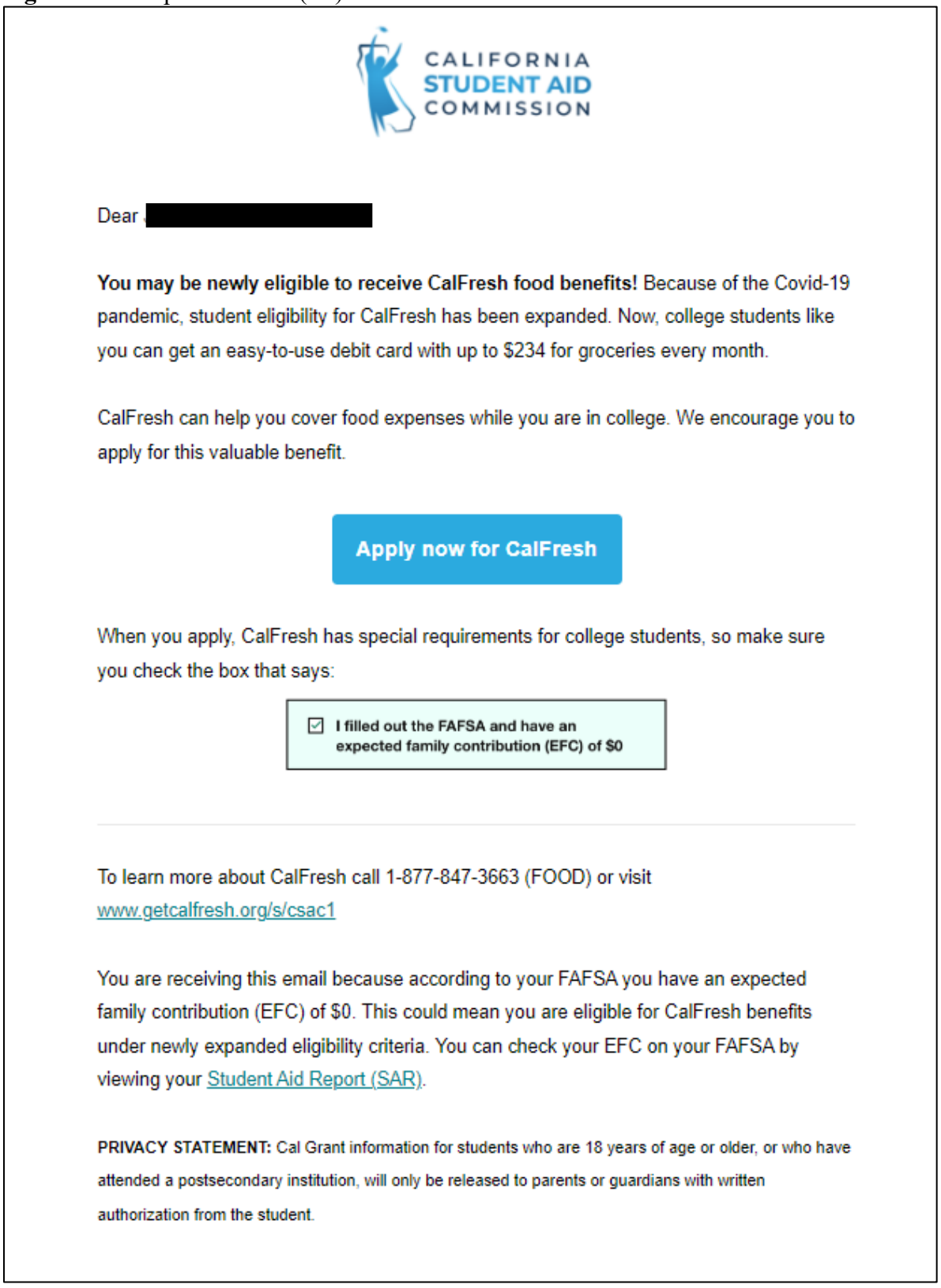
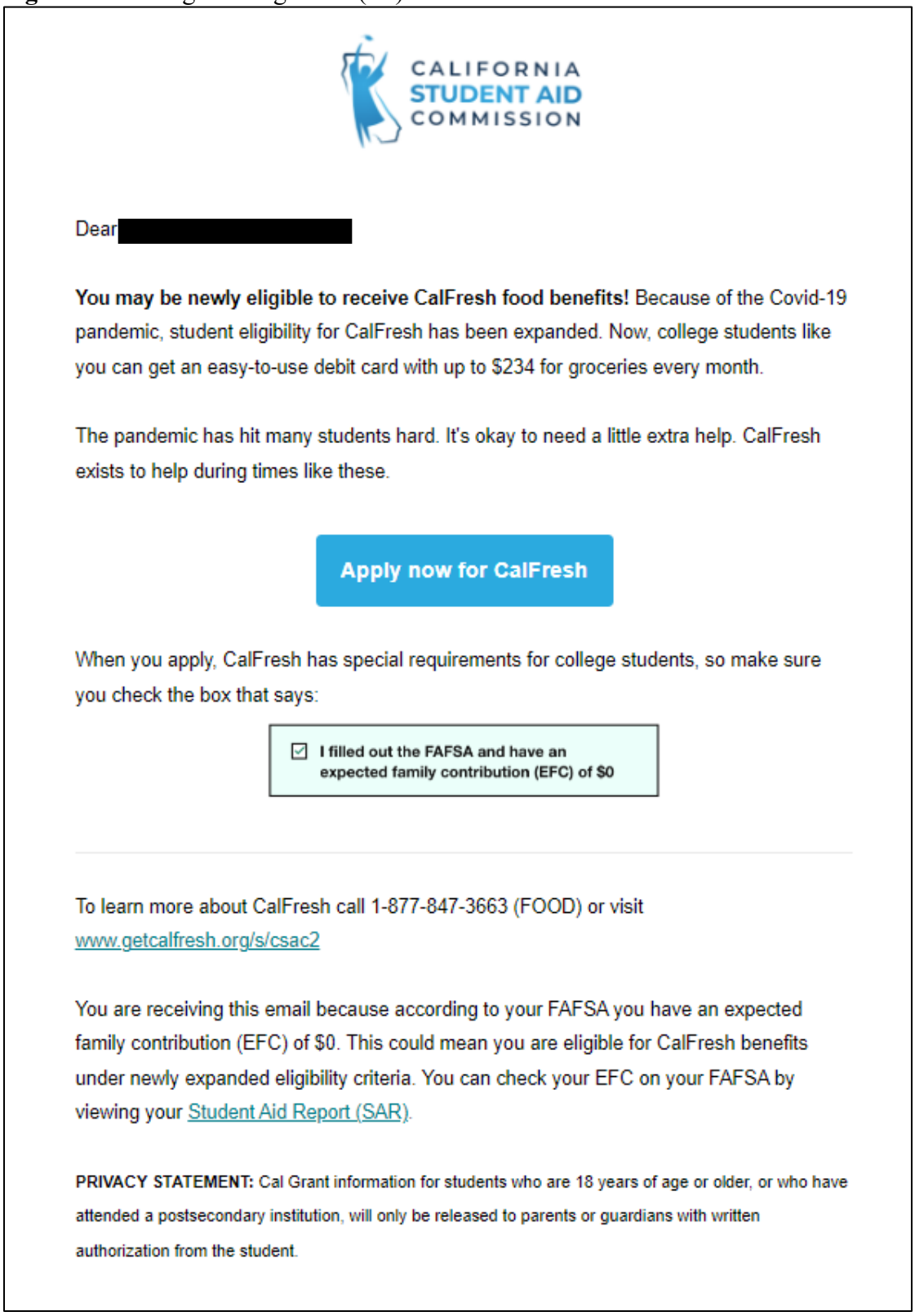


Figure C3. Destigmatizing email (T2)



CALIFORNIA STUDENT AID COMMISSION

Dear [redacted]

You may be newly eligible to receive CalFresh food benefits! Because of the Covid-19 pandemic, student eligibility for CalFresh has been expanded. Now, college students like you can get an easy-to-use debit card with up to $234 for groceries every month.

The pandemic has hit many students hard. It's okay to need a little extra help. CalFresh exists to help during times like these.

Apply now for CalFresh

When you apply, CalFresh has special requirements for college students, so make sure you check the box that says:

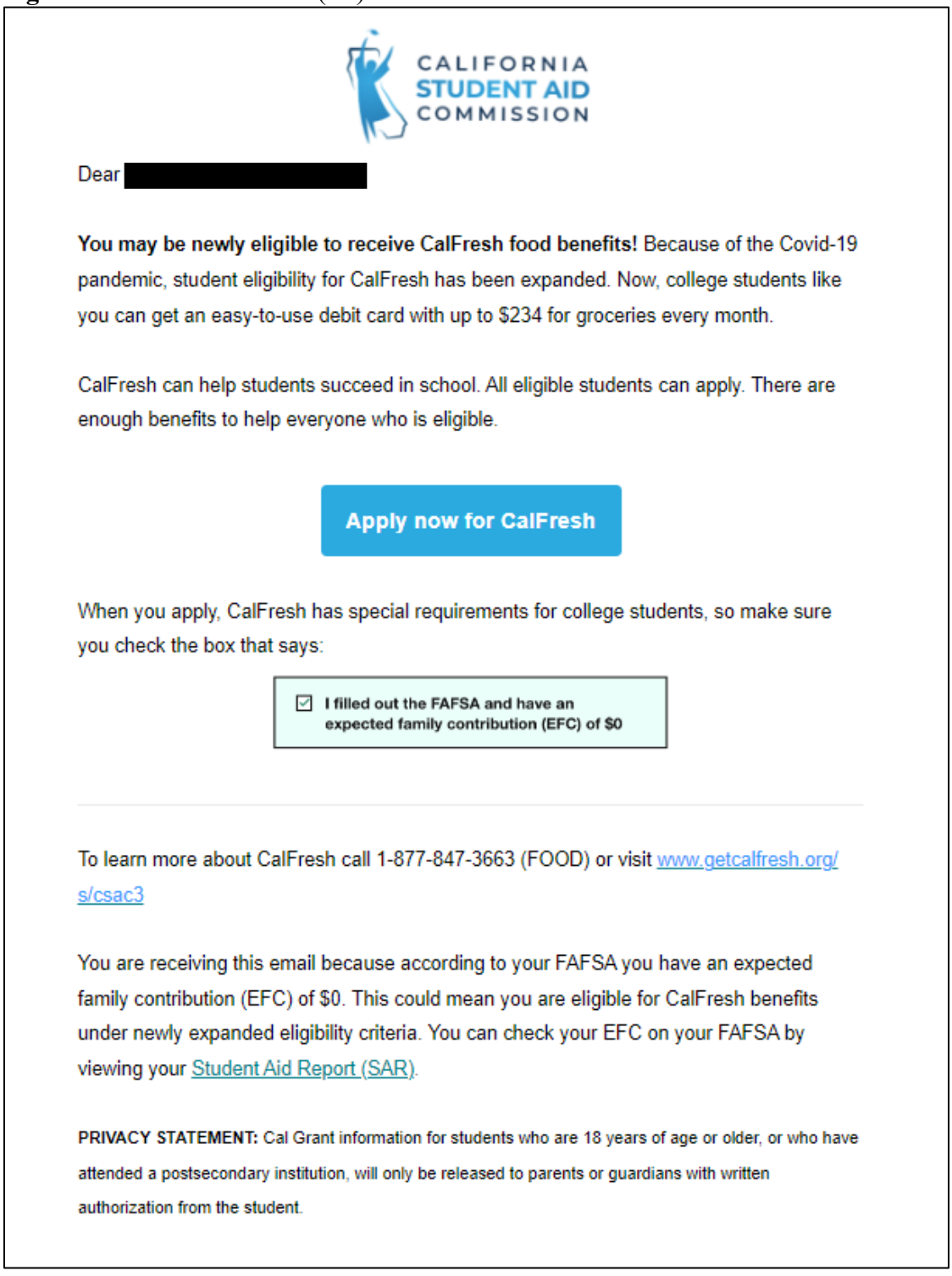
☑ **I filled out the FAFSA and have an expected family contribution (EFC) of $0**

To learn more about CalFresh call 1-877-847-3663 (FOOD) or visit www.getcalfresh.org/s/csac2

You are receiving this email because according to your FAFSA you have an expected family contribution (EFC) of $0. This could mean you are eligible for CalFresh benefits under newly expanded eligibility criteria. You can check your EFC on your FAFSA by viewing your Student Aid Report (SAR).

PRIVACY STATEMENT: Cal Grant information for students who are 18 years of age or older, or who have attended a postsecondary institution, will only be released to parents or guardians with written authorization from the student.

Figure C4. Resources email (T3)



CALIFORNIA STUDENT AID COMMISSION

Dear [redacted]

You may be newly eligible to receive CalFresh food benefits! Because of the Covid-19 pandemic, student eligibility for CalFresh has been expanded. Now, college students like you can get an easy-to-use debit card with up to $234 for groceries every month.

CalFresh can help students succeed in school. All eligible students can apply. There are enough benefits to help everyone who is eligible.

Apply now for CalFresh

When you apply, CalFresh has special requirements for college students, so make sure you check the box that says:

☑ **I filled out the FAFSA and have an expected family contribution (EFC) of $0**

To learn more about CalFresh call 1-877-847-3663 (FOOD) or visit www.getcalfresh.org/s/csac3

You are receiving this email because according to your FAFSA you have an expected family contribution (EFC) of $0. This could mean you are eligible for CalFresh benefits under newly expanded eligibility criteria. You can check your EFC on your FAFSA by viewing your Student Aid Report (SAR).

PRIVACY STATEMENT: Cal Grant information for students who are 18 years of age or older, or who have attended a postsecondary institution, will only be released to parents or guardians with written authorization from the student.

Figure C5. Postcard Front: All conditions (T1, T2, T3)

Figure C6. Postcard Back: Simplified (T1)

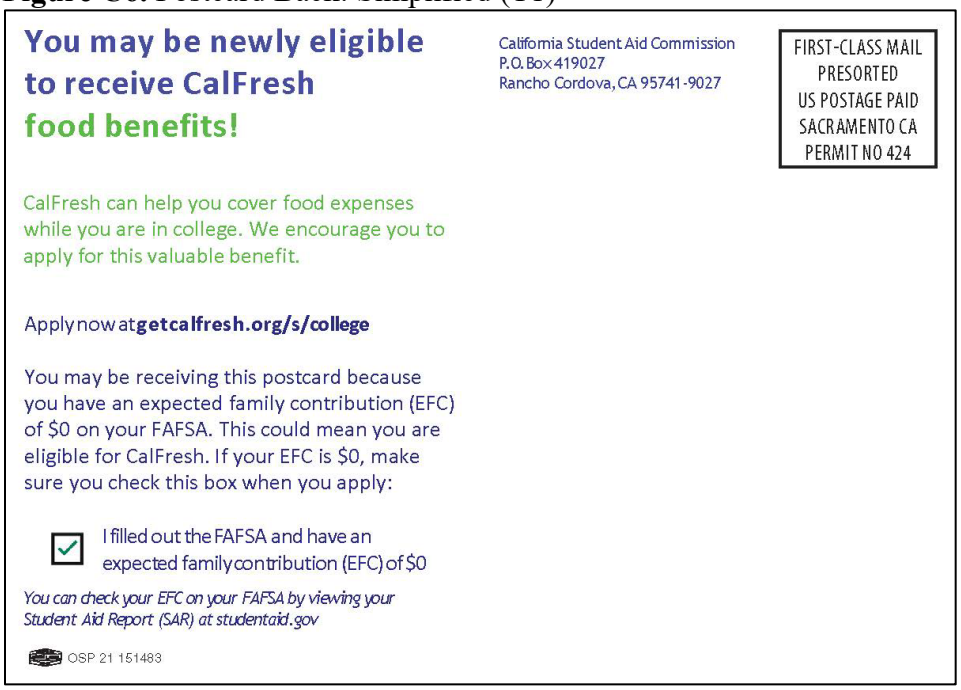


Figure C7. Postcard Back: Destigmatizing (T2)

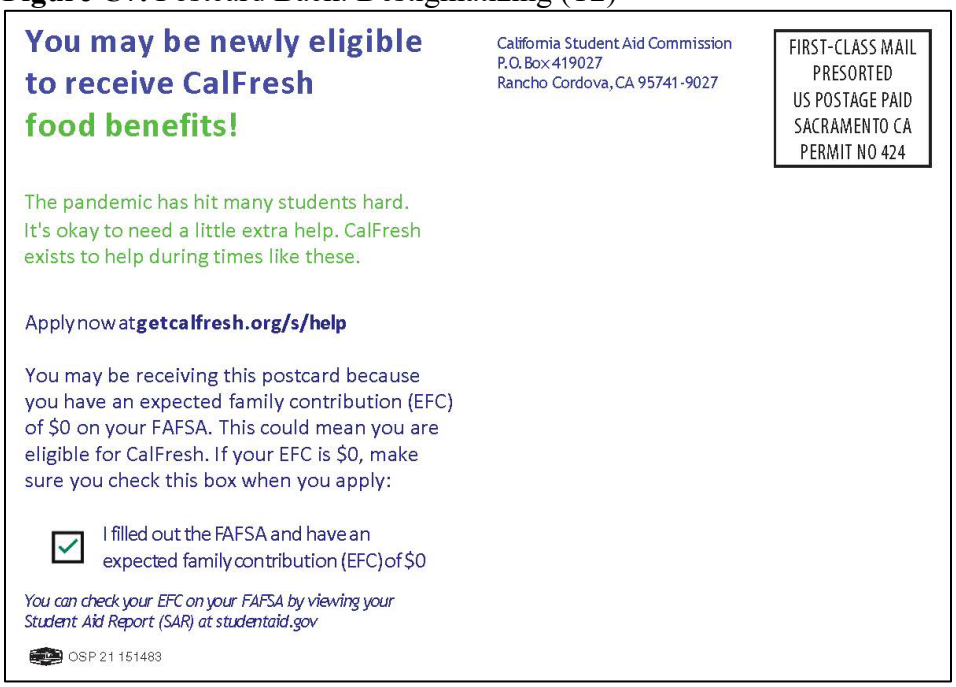
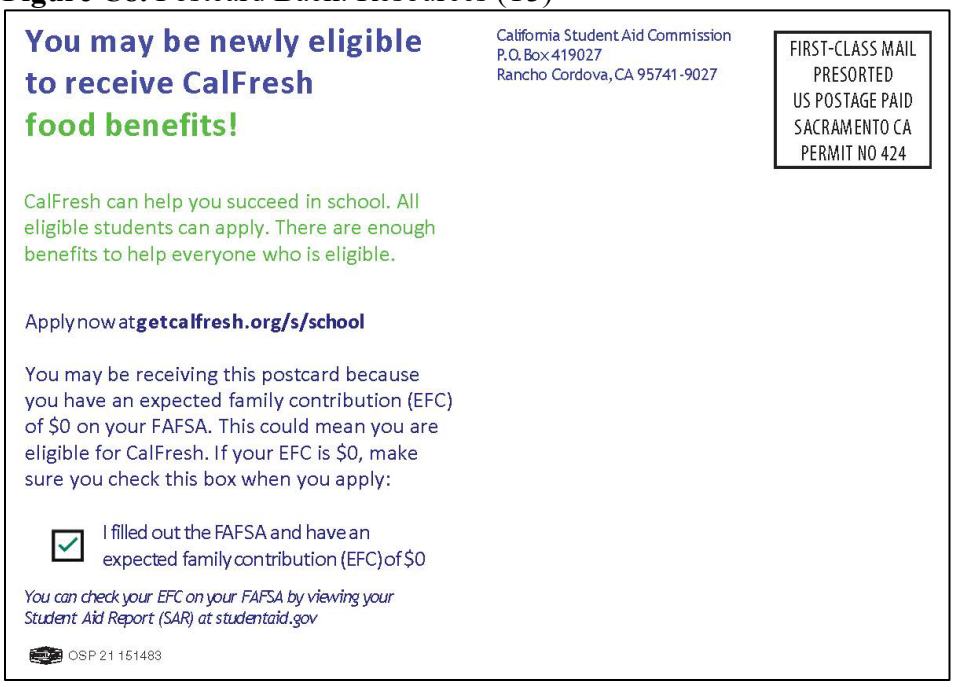


Figure C8. Postcard Back: Resources (T3)



Supplemental Tables

Table C1. Effect of messaging on CalFresh applications, 8-week outcome period

	(1) Full sample	(2) Email only	(3) Email + PC
Destigmatizing (T2)	-0.0013	-0.0023	-0.0003
	(0.0010)	(0.0016)	(0.0012)
Resources (T3)	0.0001	0.0006	-0.0004
	(0.0010)	(0.0016)	(0.0012)
Observations	237,087	118,598	118,489
R-squared	0.001	0.001	0.001
Simplified (T1) mean	0.0421	0.0523	0.0318

Notes: Estimates of the effect of assignment to the Destigmatizing or Resources message conditions on CalFresh applications during the eight-week outcome period, relative to the Simplified message conditions. All specifications include stratum fixed effects. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table C2. Effect of communication modality on CalFresh applications, 8-week outcome period

	(1) Full sample	(2) Simplified (T1)	(3) Destigmatizing (T2)	(4) Resources (T3)
Email + PC	0.0202***	0.0205***	0.0185***	0.0215***
	(0.0008)	(0.0014)	(0.0014)	(0.0014)
Observations	237,087	79,079	78,975	79,033
R-squared	0.003	0.004	0.004	0.004
Email mean	0.0316	0.0319	0.0315	0.0314

Notes: Estimates of equation 2 on CalFresh applications during the eight-week outcome period. All specifications include stratum fixed effects. Columns 2-4 reflect subgroup of students assigned to each messaging condition, respectively. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table C3. Effect of treatment assignment on CalFresh applications, robustness check

	(1) 6-week outcome period	(2) 8-week outcome period
Simplified (T1) – Email	0.0027*	0.0027*
	(0.0014)	(0.0015)
Simplified (T1) – Email + PC	0.0268***	0.0269***
	(0.0016)	(0.0017)
Destigmatizing (T2) – Email	0.0026*	0.0025*
	(0.0014)	(0.0015)
Destigmatizing (T2) – Email + PC	0.0240***	0.0244***
	(0.0016)	(0.0017)
Resources (T3) – Email	0.0022	0.0019
	(0.0014)	(0.0015)
Resources (T3) – Email + PC	0.0263***	0.0270***
	(0.0016)	(0.0017)
Observations	218,137	218,137
R-squared	0.004	0.004
Status Quo mean	0.0307	0.0335

Notes: Estimates of equation 1 on CalFresh applications during the six-week outcome period (column 1) and eight-week outcome period (column 2), excluding students with invalid contact information and students who were enrolled in CalFresh prior to the intervention. All specifications include stratum fixed effects. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table C4. Effect of messaging on CalFresh applications, robustness check

	(1)	(2)	(3)	(4)	(1)	(2)
	Full sample		Email only		Email + PC	
	6-week	8-week	6-week	8-week	6-week	8-week
Destigmatizing (T2)	-0.0014	-0.0013	-0.0000	-0.0002	-0.0028	-0.0024
	(0.0012)	(0.0012)	(0.0014)	(0.0015)	(0.0018)	(0.0019)
Resources (T3)	-0.0005	-0.0003	-0.0005	-0.0008	-0.0005	0.0001
	(0.0012)	(0.0012)	(0.0014)	(0.0015)	(0.0019)	(0.0019)
Observations	187,186	187,186	94,086	94,086	93,100	93,100
R-squared	0.001	0.001	0.001	0.002	0.002	0.002
Simplified (T1) mean	0.0454	0.0482	0.0333	0.0362	0.0575	0.0603

Notes: Estimates of the effect of assignment to the Destigmatizing (T2) or Resources (T3) message conditions on CalFresh applications during the six- and eight-week outcome periods, relative to the Simplified message conditions, excluding students with invalid contact information and students who were enrolled in CalFresh prior to the intervention. All specifications include stratum fixed effects. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table C5. Effect of communication modality on CalFresh applications, robustness check

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Full sample		Simplified (T1)		Destigmatizing (T2)		Resources (T3)	
	6-week	8-week	6-week	8-week	6-week	8-week	6-week	8-week
Email + PC	0.0233***	0.0237***	0.0242***	0.0241***	0.0214***	0.0220***	0.0242***	0.0250***
	(0.0010)	(0.0010)	(0.0017)	(0.0017)	(0.0016)	(0.0017)	(0.0017)	(0.0017)
Observations	187,186	187,186	62,509	62,509	62,064	62,064	62,613	62,613
R-squared	0.004	0.004	0.005	0.005	0.005	0.005	0.005	0.005
Email mean	0.0332	0.0359	0.0334	0.0362	0.0333	0.0360	0.0328	0.0354

Notes: Estimates of equation 2 on CalFresh applications during the six- and eight-week outcome periods, excluding students with invalid contact information and students who were enrolled in CalFresh prior to the intervention. All specifications include stratum fixed effects. Columns 2-4 reflect subgroup of students assigned to each messaging condition, respectively. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

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