It's Not Your Fault: Reducing Stigma Increases Take-up of Government Programs

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Abstract

This paper examines the role of stigma, an often cited, but understudied barrier to take-up of safety net programs. In two field experiments (N = 117,073), we find that subtle changes to the framing of rental assistance—one highly stigmatized benefit—increased interest in the program by 36% compared to providing information only and increased completed program applications by about 11%, with potentially larger effects for renters of color. Two subsequent online experiments (N = 1,258) confirm that these effects are driven by a reduction in internalized stigma, as opposed to a change in how potential beneficiaries understand the program.

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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 pervasive 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., 2016; Feagin, 1975; Federico, 2004; Gilens, 1999; Kluegel & Smith, 1986; Moffit, 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 Geiger, 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 Nutritional 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 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 (see SOM). 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 de-stigmatizing 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 de-stigmatizing 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 de-stigmatizing 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 de-stigmatizing 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 (see, 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 (see, e.g., De La Rosa et al. 2021; Bird et al., 2021; Linos et al., 2020). 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 et al., 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.

I. Study 1: Austin, TX

A. 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 Fig. 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 Fig. 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.

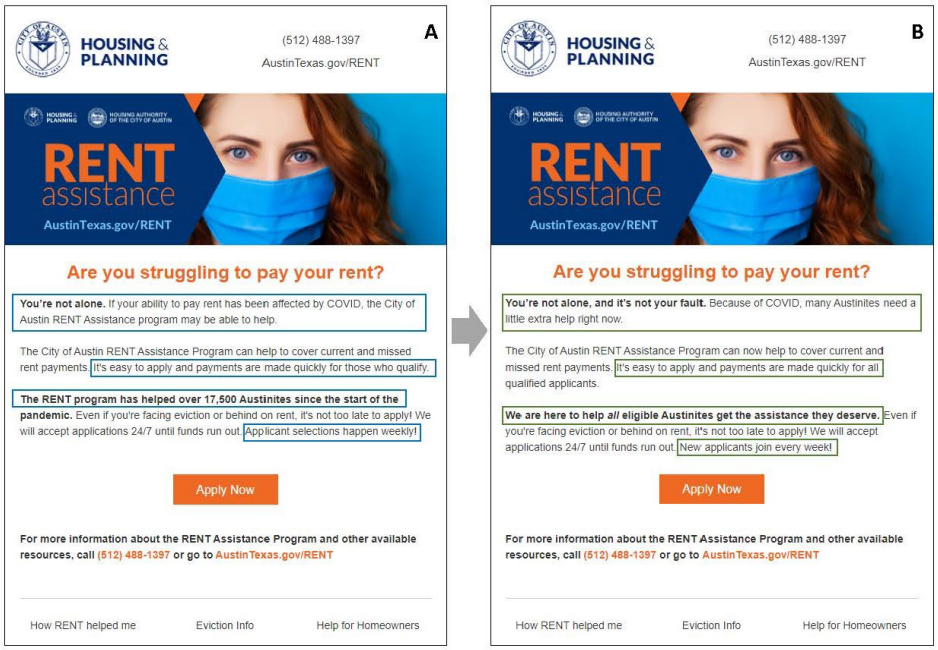
B. Results

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 de-stigmatizing 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. 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.

II. Study 2: Denver, CO

A. 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 the supplement.

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 SOM). 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.[1] 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 the SOM (Table S2).

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.

[1] At the time of this study, Denver County's rental assistance program was administered by three nonprofit agencies. Each nonprofit served a nonoverlapping group of zip codes within the county. Zip code boundaries do not align with neighborhood boundaries, making it possible to stratify on neighborhood-service area groups.

B. Results

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 + Stigm*a, 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 the SOM 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 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 ($N = 25{,}229$).[2] 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* condition ($p < .001$, 95% CI [0.28, 0.91]). On average, 0.9 percent of households in the *Control* condition requested an application during the eight weeks following the mail date (all means regression-adjusted; $SE = 0.14$),

[2] The SOM presents results for the full analytic universe (Table S2).

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]).

Results: 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 S4). As shown in Table 1, on average, 0.53 percent of households in the *Control* condition 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]).

Results: Assistance Received

Once renters submit an application for rental assistance, program staff must 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).

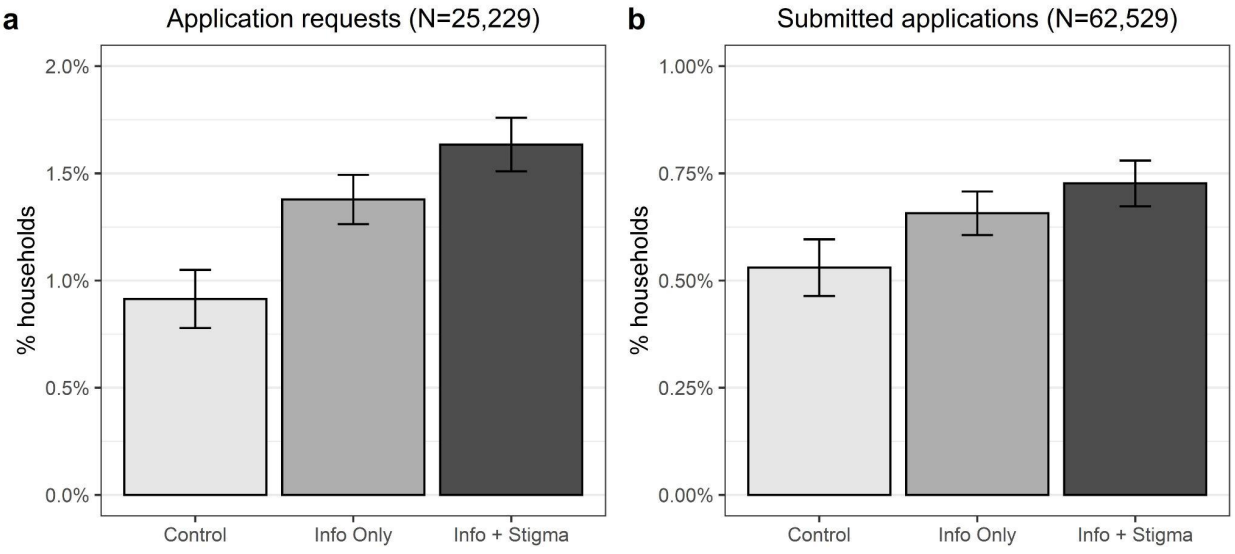
As shown in the SOM (Table S5), we find that the increase in take-up driven by the mailers also translated into an increase in funds received. Renters assigned to the *Information Only* group were 0.19 pp more likely to receive assistance between December 2020 and April 2021 than renters assigned to 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. Study 2 results

	Application Requests		Submitted Applications	
	Logistic	OLS	Logistic	OLS
VARIABLES	(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). The SOM reports results 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.

Figure 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 de-stigmatizing 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.

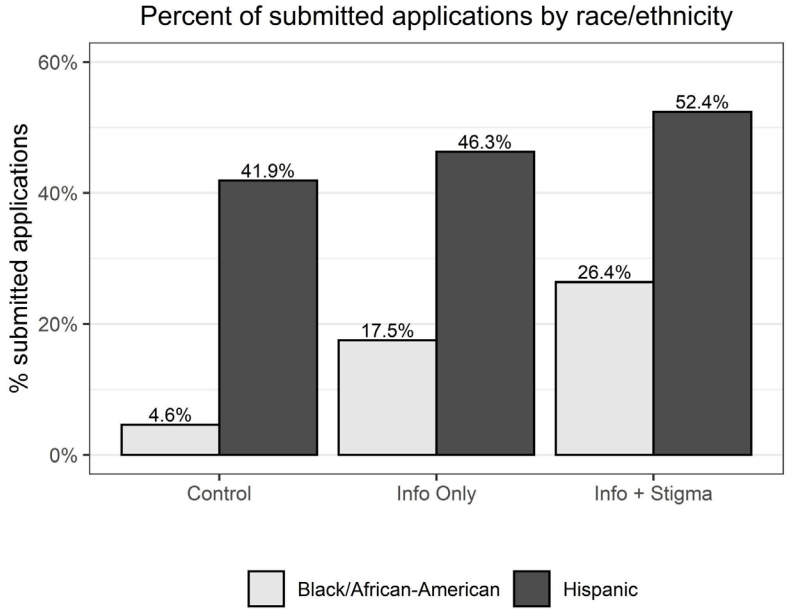
Results: Heterogeneous Effects by Race and Ethnicity

Given the racialized nature of stigma associated with government assistance (see, e.g., Cohen-Cole & Zanella, 2006), we also explore effects by race and ethnicity. Because household-level demographic information is only available for about one-third of renters who submitted applications, we can only explore variation in the raw distribution of completed applications in condition by race and ethnicity; 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 3, in the *Control* group, just 5% of submitted applications came from Black or African-American residents. In contrast, 17% of submitted applications from 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 point to potentially interesting and important directions for future study.

Figure 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.

III. 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).

A. 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 $0.50 each. After relevant data quality exclusions (see SOM), balanced evenly across treatment conditions ($\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 SOM 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 $0.30 each. Standard participant qualifications were applied (see SOM). After relevant data quality exclusions (see SOM), balanced evenly across treatment conditions ($\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 SOM 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."

B. Results

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 + Stigm*a 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 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]).

Results: 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 2. Study 3 and 4 results

	Study 3				Study 4			
VARIABLES	Overall stigma (1)	Anticipated stigma (2)	Internalized stigma (3)	Likelihood of applying (4)	Ease of applying (5)	Likelihood of receiving (6)	Credibility (7)	Comprehe-nsion (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.

IV. Discussion

Across four studies, we show that de-stigmatizing 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 are 1.5 to 3 times larger than the average effect of a behavioral intervention (DellaVigna and Linos, 2022). Moreover, the de-stigmatizing 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 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 de-stigmatize participation in government programs.

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