

Stigma in Welfare Programs

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Abstract

Stigma of welfare participation is important for policy and survey design, because it reduces program take-up and increases misreporting. Stigma is also relevant to the literature on social image concerns, yet empirical evidence is scant because stigma is difficult to empirically identify. We use a novel approach to studying stigma by examining the relationship between program participation in a recipient's local network and underreporting program participation in surveys. We find a robust negative relationship and provide evidence against explanations other than stigma. Stigma decreases when more peers engage in the stigmatized behavior and when such actions are less observable.

Key Words: Welfare Stigma, Program Participation, Social Networks, Social Image, Peer Effects.

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I. Introduction

Individual behavior often depends on the judgement of others. The literature on social image concerns (Bursztyn and Jensen, 2017) demonstrates the power of such judgement in shaping decisions, highlighting the importance of understanding how actions and beliefs of those around us affect our choices. A type of social image concern of long-standing interest for policy and research is welfare stigma.¹ Those eligible for government aid may not apply because of stigma (Moffitt 1983; Currie et al., 2001, Anders and Rafkin, 2022). This lack of program take-up has led many cities and states to invest significant resources to encourage participation. Stigma may also serve a useful purpose by inducing only the more deserving potential recipients to apply for benefits (Nichols and Zeckhauser 1982; Blumkin, Margalioth and Sadka, 2015). However, empirical evidence on the presence and nature of stigma, or social image concerns more generally, is scant. An important reason for this lack of evidence is that key concepts such as stigma or peer valuation cannot be measured and are difficult to isolate from other factors that may deter program participation, such as information or application costs (e.g. Bursztyn and Jensen 2017, p.134-135).

In this paper, we use a novel approach to study stigma in welfare programs. Following Moffitt (1983), the main approach of the literature uses program take-up and attributes the unexplained part of incomplete take-up to stigma. Our strategy differs from this approach in two ways: First, instead of incomplete take-up, we use failure to report program receipt in surveys as an indicator of stigma, which we observe by linking an accurate measure of

¹ Stigma could also arise from self-image concerns or embarrassment. Our results point to social image concerns, but we do not attempt to distinguish social and self-image concerns here, as we discuss in section V.

actual receipt to the survey data. Second, we only attribute the unexplained part of underreporting that is correlated with a predictor of stigma, program participation in the census tract of survey respondents, to stigma. This approach rests on the hypotheses that if there is stigma, it decreases with higher local participation and increases underreporting. There are clear precedents for these two hypotheses in the literature. There is a striking asymmetry towards underreporting, suggesting that respondents prefer not to report program receipt: Between a quarter and more than half of true recipients do not report program participation in our survey data, but only a small fraction of true non-recipients report receipt. The literature on survey design provides ample evidence of stigma leading to a failure to report negative information; see Celhay, Meyer and Mittag (2024) for a summary and initial evidence on stigma and misreporting. Several studies support our second hypothesis by arguing that the intensity of stigma or social norms depends on the number of people adhering to a social norm (Lindbeck, Nyberg, and Weibull, 1999) and how close these people are to the person under study (Bursztyn and Jensen, 2017).

A key challenge is to isolate stigma from other factors that are associated with both local participation and underreporting. Our approach, which we describe further in the next section, has several advantages. Studying misreporting avoids explanations that can easily be mistaken for stigma. Incomplete take-up may also be due to information and transaction costs or mismeasured receipt or eligibility. Examining how misreporting varies with a predictor of stigma (local program participation) further helps us to isolate it from these confounders. In addition, it allows us to examine how actions vary with their social desirability to these peers, which is another key determinant of social image concerns.

Thereby, we connect the literature on stigma and social image concerns and show how social image concerns can be studied in observational studies.

Unlike previous literature, we examine stigma of program reporting rather than take-up. These two types of stigma are likely to be similar, so our findings are informative about the presence and nature of stigma in program take-up. Stigma of reporting participation is also important as a source of survey error. It also seems likely that stigma of reporting makes people more reluctant to talk about program participation. This leads to lower information about available support, eligibility and program rules. Consequently, it amplifies transaction costs and information frictions, which are important barriers to take-up.

Our results provide strong evidence of stigma, as misreporting among true recipients is negatively associated with local program receipt. Several additional analyses provide evidence that this association identifies the effect of stigma: Our findings are robust to controlling for observable confounders. Instrumental Variable estimates using distance to the closest welfare office support the conclusion that omitted variables do not drive the relationship. The effects are stronger in the presence of interviewers (in-person or phone) compared to mail-back responses, where stigma should matter less. Finally, a falsification test suggests that our findings are not driven by overall survey accuracy being lower when program participation is higher. Taken together, these results provide robust evidence that welfare participation is associated with stigma, which is important for policy and survey design. We also document that stigma is stronger the less common participation is among peers and amplified in the presence of an interviewer. We thus characterize the nature of

stigma, which can help to improve policies, advance theories of welfare stigma and shed light on the nature of peer effects and social image concerns.

The next section introduces our empirical strategy. Section III describes the data. Section IV reports the results. Section V discusses our findings and concludes.

II. Empirical Strategy

The empirical challenge is that stigma is not observed, so its presence and nature need to be inferred from other variables. Using failure to report participation to study stigma rests on participation in welfare programs being considered negative information, so survey respondents may hide this information. The literature on survey methodology provides ample support for this hypothesis. See Celhay, Meyer and Mittag (2024) for discussion and references. Based on similar arguments, Bharadwaj, Pai and Suziedelyte (2017) measure mental health stigma by the extent of underreporting and DellaVigna et al. (2017) use non-response to study stigma of not voting.

As for take-up, stigma is just one potential cause of underreporting. Let M^* be a latent index that determines whether an individual fails to report program participation:

$$M^* = X\beta + S^*\gamma + \varepsilon$$

Where S^* is (unobserved) stigma. X and ε are observed and unobserved determinants of failure to report. The key empirical challenge is to isolate unobserved stigma from other unobserved factors.

Using failure to report receipt by true recipients rather than take-up is a first key advantage of our approach. Reporting of welfare programs should be more responsive to stigma than take-up, simply because not reporting receipt is far less costly than not collecting benefits (Bhargava and Manoli, 2015). This difference makes it easier to detect

effects of stigma. In addition, many other unobserved reasons for incomplete take-up are difficult to distinguish from stigma empirically. Incomplete take-up may also be due to applicants' lack of information about eligibility (e.g. Daponte, Sanders, and Taylor 1999, Banerjee et al. 2010), high transaction costs of applying (e.g. Currie et al. 2001, Ribar, Edelhoch, and Liu 2008) or both (e.g. Finkelstein and Notowidigdo, 2019). Surveys may also distort measures of take-up because both program eligibility (Scherpf, Newman and Prell, 2014) and receipt (Celhay, Meyer and Mittag, 2021) are measured with error. All of these effects can easily be mistaken for stigma in studies relying on program take-up. However, factors such as information about eligibility or transaction costs are unlikely to be systematically related to higher misreporting, as uncertain eligibility and high application costs are likely to make receipt more salient to those on the programs. Therefore, our approach is more likely to detect effects of stigma and less likely to erroneously attribute other unobserved factors that deter take-up to stigma.

In addition, we only attribute the unexplained covariance of underreporting with a predictor of stigma, local program participation, to stigma, rather than the entire unexplained part of take-up or underreporting. Therefore, we do not have to be concerned about all unobserved factors that predict misreporting, but only about those that also predict local participation rates. The set of confounders that affect two variables is necessarily smaller than the set of confounders that affect each variable, making the problem much more tractable, especially because misreporting and local area participation appear to be determined by quite different factors (see e.g. Celhay, Meyer and Mittag 2024 and Currie 2006).

Using local participation rests on the hypothesis that stigma, if present, decreases in the fraction of immediate neighbors who are recipients. There is a growing literature studying how social networks and interactions impact individual behavior (Becker 1974, Durlauf and Young 2001, Jackson 2014) including program take-up (Bertrand, Luttmer, and Mullainathan 2000; Aizer and Currie 2004; Kroft 2008; Dahl, Loken, Mogstad 2014; Alatas et al. 2016). Supporting our assumption, Lindbeck, Nyberg, and Weibull (1999, p.3), for example, argue that “the [intensity] of a social norm against living off other people’s work ... depends on the number of people adhering to it” so that “... living on transfers becomes relatively less embarrassing when more individuals do likewise.” Thus, stigma should be decreasing in local participation as the belief of being ostracized for welfare receipt depends on the extent that others within the relevant social network make similar choices. Both the literature on social image concerns (Bursztyn and Jensen 2017) and peer effects (Durlauf and Young 2001, Jackson 2014) also often find individuals to be more prone to take actions that are more common in their network, potentially because the intensity of social norms depends on the number of people adhering to them. We assume that individuals’ perception about the intensity of social norms is defined locally, rather than in the entire economy, because recipients have some knowledge of program participation by neighbors, possibly through interactions at the local store, program office or by word of mouth.²

² As these theories clearly point to stigma decreasing in the frequency of the action, we use participation rather than take-up, i.e. we include ineligible households in the denominator. Additional advantages of using participation are that individuals seem unlikely to know whether others are ineligible or do not take-up the program and that stigma likely increases in the number of ineligible non-recipients as well.

This approach is only feasible because we examine stigma in reporting rather than actual take-up. For take-up, an association between individual behavior (take-up) and group behavior (local participation) for reasons other than stigma could arise from the “reflection problem” (Manski, 1993). We avoid this problem because in our analysis group behavior (participation) is not an aggregated version of our dependent variable (reporting rather than participation). We study the effect of local receipt rates on reporting among recipients only. Thereby, we vary group behavior (local receipt rates) while holding fixed whether or not the individual takes the same action. Among those who receive the program, individual participation is constant and hence invariant to group behavior, which breaks the link between individual and group behavior.

An important concern that arises from conditioning on receipt is that we only examine true program recipients. While this is unavoidable, because there is no stigma of reporting among non-recipients, restricting the sample to recipients induces sample selection. However, this selection problem will make it harder for us to detect stigma. Our sample is indeed negatively selected in terms of stigma, which biases the mean of stigma in our sample downward. However, our hypothesis concerns the slope of the relationship between the local participation rate and misreporting, not the intercept. As we show in the appendix, conditioning on participation will bias this slope upward, making it harder for us to confirm our hypothesis that higher local participation reduces misreporting. Intuitively, stigma deters program participation. Thus, those who take-up the program, are negatively selected in the sense that they face lower unobserved stigma. Since local program participation increases take-up, our sample becomes less and less negatively selected as local participation increases. Thus, recipients in areas with higher local participation are less

negatively selected in terms of unobserved stigma. Hence, sample selection makes unobserved stigma in our sample rise with local participation. This mechanism works against the hypothesis that stigma decreases with local participation and will hence bias our estimates toward zero. Thus, our finding of the presence of stigma is stronger evidence of its presence. By virtue of a similar argument, common shocks to unobservable determinants of participation will make it harder for us to detect stigma.

In summary, we propose to study stigma by examining misreporting rather than take-up and focusing on its correlation with local participation rates rather than its entire residual. Therefore, our main specification is a Probit model,

$$\Pr(Y_{ijc} = 1|P_{jc}, X_{ijc}) = \Phi(\alpha + P_{jc}\delta + X_{ijc}'\beta), \quad (1)$$

where Y_{ijc} is an indicator equal to one if individual i who participates in program j and lives in census tract c fails to report program participation in the survey. We analyze SNAP as well as Temporary Assistance for Needy Families (TANF) or General Assistance (GA). We aggregate participation in either TANF or GA to participation in one program, TANF+GA, because they are in practice one program in New York. P_{jc} is participation in program j in census tract c and X_{ijc} is a vector of control variables. Our main interest is the coefficient δ , which measures the extent to which failure to report program participation varies with local participation. To interpret δ as an effect of stigma, we need to purge any confounders that predict both local receipt and underreporting. Several features of our approach make this condition plausible. First, we control for key confounders including household income. Our results are robust to including many additional potential confounders, which suggests that conditioning on a measure of household income removes omitted variable bias in equation (1).

Second, we provide evidence against unobserved confounders by instrumenting local participation using the distance to local welfare offices as our instrument. Instrument validity requires no effect on the probability that a true participant does not report participation, other than through its effect on local participation (conditional on covariates). Welfare offices are likely to be closer to people in need, potentially creating a spurious relation between distance and the dependent variables in studies of individual participation on outcomes such as employment and income.³ This line of reasoning is less convincing in our case, since our dependent variable measures misreporting and we condition on true receipt as well as measures of income and well-being. Thus, our strategy only suffers from bias if the government placed welfare offices closer to people whose unobserved characteristics make them more likely to *report* participation truthfully. The government seems unlikely to try that and even less likely to succeed, because there is little evidence of geographic variation in misreporting once conditioning on demographics (Meyer and Mittag 2019b, Mittag, 2019, Fox, Rothbaum, Shantz, 2022).

Rather, being further away from a DSS office likely reduces program participation through higher transaction costs, which is unlikely to affect reporting of receipt. The stability of our estimates across specifications including estimates that add county fixed effects are more consistent with a causal effect than a spurious relationship that somehow yields similar coefficients in different settings.

³ Note, however, that the scope for endogenous placement of welfare offices is limited, because they tend to be in government owned buildings in administrative centers of regions. In addition, the location of welfare offices rarely changes, so proximity to recipients would have to be based on neighborhood characteristics long ago.

In sum, we account for observed factors, so remaining concerns for validity would have to arise from something both unobserved and systematically predicted by the distance to the nearest welfare office. It is possible that a potential confounder, such as salience or information about the program may vary with both our instrument and reporting. To provide evidence against such a potential confounder, we show that our estimates are indeed larger when one would expect more stigma. A common precept in the survey methodology literature is that stigma should be lower in self-administered surveys. Substantial empirical evidence supports that stigma is stronger in the presence of an interviewer (Tourangeau and Smith 1996; Bradburn 2015; also see Fu et al. 1998, Brittingham, Tourangeau, and Kay 1998, Belli, Traugott, and Beckman 2001 and Karlan and Zinman 2008). Thus, if the effect we find is indeed due to stigma, it should be larger among households interviewed either by telephone or by face-to-face interviews than among those responding by mail. Conversely, if the relationship were driven by the placement of welfare offices or by spurious correlates of the distance to these offices, then their effect should also be present among the mail-back sample. Thereby, these analyses provide clear evidence against key concerns of IV validity.

Finally, we provide evidence against another potential explanation, that overall survey accuracy may vary with local participation, from a falsification test: we estimate the same model, but change the dependent variable to a measure of survey cooperation (whether or not an individual refuses to answer the question about their earnings). We also repeat the analysis using the CPS. That the findings are consistent across the two surveys provides further evidence against a spurious relationship. It is particularly encouraging since misreporting differs between the two surveys (Meyer and Mittag, 2021), so that spurious

relationships between geography and misreporting would likely differ between the two surveys. While each of these analyses by themselves could in principle be affected by factors we cannot control for, we argue below that each factor that may affect one specific result is incompatible with at least one of our other results.

III. Data

To implement our empirical strategy, we need to observe whether a true recipient household fails to report program participation. We also need to measure participation in the network or immediate neighborhood of the household. Finally, we need the distance to the nearest welfare office. Linking survey data to administrative records provides us with these measures.

As survey data, we use the 2008-2012 American Community Survey (ACS) and the Annual Social and Economic Supplement of the Current Population Survey (CPS) for calendar years 2007-2012. We link both surveys to high quality administrative records of all monthly payments for SNAP and TANF+GA in New York State for 2007-2012. The records stem from actual payments and are thus subject to various validity checks. Linking monthly records at the individual level allows us to exactly match survey concepts such as the household definition and reference period. Meyer and Mittag (2017, 2019a, 2021) and Celhay, Meyer, and Mittag (2021, 2024) also use these data and discuss why they are sufficiently accurate to provide a measure of underreporting. Most importantly, high linkage rates and a close correspondence of total dollars received according to estimates from the linked data and official aggregate spending provide evidence against substantial error in our data. Note that we do not need the data to be entirely error free, but only need

the probability of a true non-recipient household to be erroneously linked to a payment record not to decrease substantially with local participation.

To measure underreporting, we use the linked samples of households that actually participate in SNAP (79,707 households in the ACS and 2,763 in the CPS), and in TANF+GA (4,595 households in the ACS and 727 in the CPS) during the reference periods of the surveys, excluding item non-respondents. Appendix Table 1 presents summary statistics. 25 percent of these true recipient households do not report SNAP participation in the ACS (36 percent in the CPS) and 59 percent do not report TANF+GA participation in both surveys. See Celhay, Meyer and Mittag (2021) for analyses of survey error in these sources. For our ACS sample, they find that program recipients who do not report receipt are more likely to be married parents, Black or Hispanic, male, higher income, and in a household with an employed member. Appendix Table 2 reproduces their Tables with the determinants of failure to report by true recipients. More generally, the literature has emphasized income, employment, race and ethnicity as well as reported receipt of other programs as key predictors of underreporting program receipt.

Our administrative records contain all cases and their addresses, which allows us to measure local participation and distances to the nearest welfare office. We use the census tract⁴ of a household as its local network, so we measure local participation by the ratio of the number of recipients in each census tract according to the administrative data in each year from 2008 to 2012 to the total number of living units according to the 2010 Census. Tracking office locations back to 2007, we compute the linear distance from the center of

⁴ Census tracts contain about 4,000 individuals. We use census tracts rather than ZIP codes (Aizer and Currie 2004) or wider areas such as PUMAs or MSAs (Bertrand, Luttmer, and Mullainathan 2000), because census tracts are smaller and vary less in their size.

the census block of each household to local offices of the Department of Social Services (DSS), where SNAP and TANF+GA applications are filed and cases are managed. To construct a distance measure at the census tract level, we computed the recipient-population weighted average of the distances to the nearest DSS of the blocks in each tract.

IV. Results

A. Basic results

We estimate equation (1) for both programs in each survey. Table 1 reports results for SNAP (Panel A) and TANF+GA (Panel B) in the ACS. Table 2 repeats the analyses for the CPS. We focus on SNAP in the ACS below. The other results are similar and further support our hypotheses, but our findings are less conclusive for TANF+GA than for SNAP. This difference arises partly from statistical reasons. SNAP is a much larger program, so the effects are more precisely estimated. In addition, usage of SNAP benefits is easier to observe than receipt of cash assistance from TANF+GA. Thus, awareness of participation by peers may be lower, making local participation a noisier measure of stigma for TANF+GA than for SNAP. The estimated effects are larger in the CPS than in the ACS, but the CPS only uses in-person and telephone interviews. Therefore, the CPS results should be compared to those for non-mail interviews in the ACS, which are very similar (see Table 1 column 5).

We start with the bivariate relationship between failure to report receipt and local participation in column 1 of Tables 1 and 2, which is likely biased due to the omission of confounding variables like income. Income is a strong predictor of misreporting program participation (e.g., Bollinger and David 1997) and households with lower income are more likely to live in areas with higher participation. Indeed, controlling for household income

in column 2 cuts the estimate in half. The estimate changes little with the inclusion of a rich set of controls in column 3. These controls include demographic characteristics (such as household composition, gender and race) and receipt patterns (time since last receipt, receipt duration and amounts) that have been found to predict misreporting (Meyer, Mittag and Goerge, 2022, Celhay, Meyer and Mittag, 2024), as well as geographic controls (rural, county fixed effects). Despite the controls predicting our outcome variable, our coefficient of interest remains remarkably stable for both programs in both surveys, which provides evidence that controlling for household income removes (most of) the omitted variable bias in the bivariate relationship (Altonji, Elder and Taber, 2005, Oster 2019).

Focusing on the results in column (3), for SNAP in the ACS, a 10-percentage point increase in local participation leads to a 0.9-percentage point decline in the conditional probability of misreporting. For TANF+GA, the effect is slightly larger with a 3.2 percentage point reduction. As expected, the effects of a 10-percentage point increase in local participation in the CPS are larger at 1.5 percentage points for SNAP and 7.2 percentage points for TANF+GA. While the effect implies sizeable variation in stigma and its effects between neighborhoods, the reduction in misreporting is small relative to the false negative rate and the importance of other determinants of misreporting, such as recall errors (Celhay, Meyer and Mittag 2024). More importantly, the increase in misreporting provides evidence that stigma is higher in areas with low local program participation and that stigma deters the reporting of program receipt.

B. IV estimates using distance to Social Service Offices

We have shown that the decline in underreporting with local participation is not driven by observables. Yet, rather than stigma, the effects we find above may arise from unobserved

factors related to both misreporting and local participation, such as a “welfare culture” or concerns about being investigated for fraudulent receipt. Recall may also vary with local participation potentially because interviewers probe more for program receipt in areas with high participation. Note, however, that the invariance of our results to the inclusion of many covariates requires unobserved confounders to be orthogonal to our covariates, which seems unlikely in the alternative explanations above. Next, we present IV estimates as evidence that the effects are not due to omitted variables. Our instrument is the distance from each census tract to the closest DSS office, which is strong with F-statistics between 120 and 218 in the ACS (and 14 and 194 in the CPS, see Appendix Table 3).

Columns 4 of Table 1 and 2 show the results of IV Probits.⁵ In the ACS, a 10-percentage point increase in local SNAP participation reduces the probability of underreporting by 1.5 percentage points. The reduction of 0.9 percentage points for TANF+GA is insignificant due to a large standard error. The results for the CPS are insignificant, but well-aligned with our other results for SNAP and too noisy to be informative for TANF+GA. Compared to the regular Probit estimates, the IV Probit estimates are approximately 1.5 times larger for SNAP, but smaller for TANF+GA. Hausman tests of equality of the Probit and IV Probit estimates do not reject that the coefficients are equal in either case, with p-values of 0.4 for SNAP and 0.8 for TANF+GA in the ACS and even higher p-values in the CPS. Consequently, the IV estimates do not provide evidence of endogeneity. Thus, they further support that we isolate stigma.

⁵ Linear IV models yield similar results, so functional form assumptions appear unimportant.

C. Interview Method

Our main specification and the IV results provide evidence against both observed and unobserved factors other than stigma that may vary with local participation and misreporting. Our instrument does not vary within census tract, so we can rule out unobserved confounders that vary between individuals, but not between tracts. However, our IV estimates could be biased if survey reporting is more accurate closer to DSS offices for reasons other than stigma. Past work has found most geographic variation in misreporting to be explained by observed characteristics, but factors such as information about the program, program confusion, its salience or how much it is discussed among neighbors may also vary both with the distance to DSS offices (and hence local participation) and misreporting. We next use variation within geographic areas to provide evidence against such explanations. We do so by showing that the effect is stronger in the presence of an interviewer where stigma should be more relevant. Specifically, we use the ACS to repeat our analyses separately for households that were surveyed by mail and those surveyed by an interviewer either by telephone or face-to-face.

An important caveat is that interview mode is not assigned randomly. Only households that fail to send back a mail-back form are contacted by telephone and ultimately visited for an interview. Thus, reporting may differ by survey mode due to selection. Indeed, receipt and misreporting are higher for both programs in the non-mail sample. The non-mail sample is also a bit more urban and slightly poorer, leading to slightly lower distances to welfare offices and higher average program amounts (but no meaningful difference in duration of receipt). In addition, there are some small differences in variables associated with non-response (more employment, less education, younger, more children). We control

for these and all other covariates included in the previous regressions. Sample selection could still bias our coefficient of interest, but if our results in the non-mail sample were driven by selection, then the effect would have to have the opposite sign in the mail-back sample and cancel in the aggregate. Columns 3, 5 and 6 of Table 1 clearly show that neither is the case. In fact, the point estimate for the mail-back sample is negative as well, so selection cannot explain the negative effects we find in the non-mail sample.

For SNAP, we find exactly the patterns we expect, both in our main specification in columns 5 and 6 and in the IV approach in columns 7 and 8 of Panel A of Table 1. We find no effects in mail interviews, but in the presence of interviewers, the effects are significant and larger than in the full sample. For TANF+GA in Panel B of Table 1, we also find a significant and large effect when an interviewer is present in our main specification. The effect for mail interviews is not significant and smaller, as expected, but contrary to SNAP, it is not close to zero either. The results from the IV approach for TANF+GA are so noisy that they provide little evidence overall.

D. Falsification test for differences in overall response accuracy

In addition to evidence against observable and unobservable confounders, the results by survey mode provide evidence against any remaining confounding factors unless they are only relevant in the presence of an interviewer. For example, one would expect most individual and neighborhood characteristics as well as any factors potentially caused by being closer to a DSS office, such as salience or information effects, to affect mail-in interviews as well. However, a remaining concern is that local participation may be associated with survey cooperation and reporting behavior in general. This hypothesis is hard to reconcile with the fact that we find effects to be muted in the absence of

interviewers and would thus require high welfare receipt areas to be those in which interviewers are less able to induce respondents to cooperate.

We provide evidence against this possibility by showing that local participation does not affect a measure of reporting quality. Bollinger and David (2001) as well as Celhay, Meyer and Mittag (2024) show that item non-response predicts how accurately the respondent answers the survey overall. Therefore, we implement a falsification test using an indicator whether the respondent refused to answer the question about earnings as the dependent variable in equation (1). The results in column 9 of table 1 show that the relation between local participation and earnings non-response is very small and insignificant for both TANF+GA and SNAP.

V. Discussion

We document a robust negative relationship between underreporting of welfare participation and local program participation. We rule out a wide range of alternative explanations for this relationship, strongly suggesting that the negative relation we find is indeed due to stigma. We show that observable characteristics do not bias our results. We provide evidence against unobservable factors by using distance to the nearest welfare office as an IV. To rule out a spurious relation based on geographic factors, which could invalidate our IV approach, we show that the effects are weaker or disappear in mail-back responses, where stigma should be less relevant. Finally, we provide evidence against factors that only matter in the presence of an interviewer by showing that the results are not driven by survey accuracy or cooperation overall. Thus, our results strongly suggest the presence of welfare stigma. The intensity of stigma decreases when more people in the social network of an individual participate in the same program. This pattern is more

evident when an interviewer is present, showing that stigma is stronger in the presence of others, even strangers. These findings are relevant for the literatures on welfare programs and social image concerns.

A first contribution to the literature on welfare stigma is that we provide strong evidence that stigma matters for welfare programs. Evidence of its presence is important, because stigma has long been debated as a factor that deters welfare participation both in the academic literature (see e.g. Moffitt 1981; Ranney and Kushman 1987; Keane and Moffitt 1998; Breunig and Dasgupta 2003, Anders and Rafkin, 2022) and when designing welfare policy. In addition to strongly suggesting that stigma deters welfare participation, demonstrating its presence in the context of reporting participation is of interest for survey design, since stigma has long been hypothesized to cause underreporting of socially undesirable behavior (Tourangeau and Yan, 2007).

Beyond establishing the presence of stigma, our results also shed light on its nature by showing that stigma decreases with participation in the network of the recipient. This result is of direct relevance for both policy makers concerned with take-up and survey producers worried about underreporting. Our findings also help to advance theories of welfare stigma. For example, Besley and Coate (1992) propose two theories of welfare stigma, which either arises from statistical discrimination or taxpayer resentment. Our finding that stigma decreases with local program receipt is in the spirit of the former theory. Our results also provide empirical support to the model of welfare stigma in Lindbeck, Nyberg, and Weibull (1999), who theorize that the intensity with which social norms affect individual behavior depends on the number of people that adhere to them. Better knowledge of the nature of stigma can also help to understand how it affects other outcomes, such as voting

(DellaVigna et al. 2017), charitable donations (Hungerman 2013, DellaVigna et al. 2012), crime (Rasmusen 1996), health (Link and Phelan 2006; Mahajan et al. 2008), job displacement (Gibbons and Katz 1991) and unemployment (Biewen and Steffes 2010).

More generally, by showing that program participation in the local network of a recipient affects the intensity of stigma, we also contribute to the literature on the effects of peers and social networks (e.g. Durlauf and Young 2001; Jackson 2014; Chandrasekhar, Larreguy, and Xandri 2015). Peer effects matter in many settings, such as migration and labor market outcomes (Munshi 2003), retirement decisions (Duflo and Saez 2003), employment (Calvó-Armengol and Jackson 2004), and energy savings (Allcott 2011). Prior work has established network or peer effects in program take-up (Bertrand, Luttmer, and Mullainathan 2000; Aizer and Currie 2004; Alatas et al. 2016). Whether these effects arise from information, stigma or other factors is an open question, because higher take-up by peers likely affects both information and stigma. By studying program reporting conditional on take-up, we provide clear evidence that stigma matters.

A specific mechanism behind peer effects is social image concerns. By viewing welfare participation as a negative status good, our results on the nature of stigma are also informative about social image concerns. Previous studies mainly relied on variation in the probability of an action being observed to demonstrate the presence of social image concerns. See Friedrichsen, Koenig and Schmacker (2018) for a study that skillfully varies the observability of welfare take-up in the lab. By studying how stigma varies with participation in the network of an individual, we show that social image concerns indeed also depend on the (perceived) social desirability of the action to the peer group of the individual. This result supports the conceptual framework of Bursztyn and Jensen (2017),

which posits that social image concerns affect the actions or utility of an individual through the product of the social desirability of an action to the peer group of the individual and the probability that the peer group observes the action. Specifically, we provide evidence that stigma creates positional externalities akin to status goods (Bursztyn et al., 2018). We document a similar “negative positional externality” in that the disutility of socially undesirable behavior decreases in the number of peers that engage in the same behavior. Understanding such mechanisms better not only advances theories of social image concerns, but also advances our understanding of how social norms change and the mechanisms that lead to socially undesirable behavior being normalized (Bursztyn, Egorov and Fiorin 2020).

Our study also points toward stigma arising from social- rather than self-image concerns (Benabou and Tirole, 2011), as the latter should depend less on program participation of peers. Weaker or non-existent effects when no interviewer is present further supports this view. However, we cannot rule out self-image, since self- and social-image are difficult to distinguish conclusively (Bursztyn and Jensen, 2017, p. 144). It is noteworthy that peers affect reporting behavior, which is only observed by a stranger and not the relevant peers. This observation allows us to further characterize the mechanisms of social image concerns. In line with DellaVigna et al. (2012, 2017), this fact points towards hedonic, rather than instrumental motives. It also suggests that peer effects are internalized, e.g. by shaping choice rules or social norms, and persist in the absence of the peer group that created them. In the framework of Bursztyn and Jensen (2017), it points toward peer valuations being intertwined: The beliefs of an individual about the social

desirability of an action according to peer group j seem to depend on the behavior of peer group $k \neq j$ that the individual is accustomed to.

Beyond enhancing our understanding of welfare stigma and social image concerns, this study also makes methodological contributions. As discussed in section II, our strategy of examining how underreporting varies with a predictor of stigma provides more credible evidence of stigma, because it avoids key confounders of studying stigma in unexplained program take-up, such as mismeasured eligibility and participation, information or transaction costs.

We demonstrate that empirical evidence on stigma can be obtained from observational studies. We thereby show that social image concerns, which have hitherto mainly relied on RCTs and quasi-experiments, can be examined in an observational setting, i.e. that the key downside of identification can be sufficiently mitigated by carefully isolating variation in the determinants of social image concerns in survey data. Meeting this challenge expands the scope of studies of social image concerns to outcomes and mechanisms where inducing random variation is difficult or impossible. The literature on social image concerns discusses welfare stigma (Bursztyn and Jensen, 2017) but there are only few empirical studies (e.g. Fridrichsen, Koenig and Schmacker, 2018), because studying welfare participation in RCTs is difficult. More generally, it is difficult to cleanly vary the valuation of peers in RCTs (without making exogeneity assumptions akin to observational studies), so our observational approach allows us to better explore this channel. Surveys also typically include larger samples and more information than RCTs. This wealth of information enhances our ability to characterize the nature of social image concerns by analyzing heterogeneity in its effects as we do in our comparisons across survey modes.

To conclude, our results are important for policies to increase program take-up and for survey design seeking to reduce underreporting. They also improve our understanding of stigma and, more generally, social image concerns. Our finding that stigma decreases with local participation shows that peer valuation indeed affects social image concerns. It supports the view that status goods, of which welfare participation can usefully be seen as a negative one, create positional externalities. That stigma is more intense in the presence of an interviewer points toward social image, rather than self-image, concerns as its origin. From a methodological perspective, our study demonstrates the benefits of combining the literature on stigma and social image concerns.

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Tables

TABLE 1: LOCAL PROGRAM PARTICIPATION AND THE PROBABILITY OF NOT REPORTING RECEIPT IN THE ACS. PROBIT MARGINAL EFFECTS

Dependent Variable:		Not Reporting Program Receipt in Survey								Earnings Non- Response
Sample used:		Full Sample				Mail Interviews	Non-Mail Interviews	Mail Interviews	Non-Mail Interviews	Full Sample
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	
<i>A. Results for SNAP</i>										
Participation in Census Tract	-0.201*** (0.010)	-0.092*** (0.013)	-0.087*** (0.015)	-0.153** (0.058)	-0.017 (0.014)	-0.125*** (0.022)	-0.003 (0.055)	-0.223*** (0.085)	-0.007 (0.010)	
Observations	79,707	79,707	79,707	79,706	42,380	37,327	42,379	37,327	79,707	
Mean of Dependent Variable	0.249	0.249	0.249	0.249	0.156	0.298	0.156	0.298	-	
Mean Tract Participation	0.298	0.298	0.298	0.298	0.266	0.315	0.266	0.315	0.298	
Elasticity of Reporting w.r.t. Participation	-0.240	-0.110	-0.109	-0.168	-0.016	-0.142	0.020	-0.188	-	
<i>B. Results for TANF+GA</i>										
Participation in Census Tract	-0.618*** (0.112)	-0.118 (0.112)	-0.315*** (0.132)	-0.089 (0.612)	-0.264 (0.185)	-0.347** (0.162)	0.918 (0.938)	-0.414 (0.742)	0.011 (0.087)	
Observations	14,595	14,595	14,595	14,595	6,283	8,312	6,283	8,312	14,595	
Mean of Dependent Variable	0.587	0.587	0.587	0.587	0.497	0.618	0.497	0.618	-	
Mean Tract Participation	0.053	0.053	0.053	0.053	0.045	0.056	0.045	0.056	0.053	
Elasticity of Reporting w.r.t. Participation	-0.056	-0.010	-0.028	-0.017	-0.023	-0.029	0.063	-0.067	-	
Controls for Household Income	N	Y	Y	Y	Y	Y	Y	Y	Y	
Controls for Demographics	N	N	Y	Y	Y	Y	Y	Y	Y	
IV Using Distance to Nearest Office	N	N	N	Y	N	N	Y	Y	N	

Notes: This table reports average marginal effects of SNAP (upper panel) and TANF+GA (lower panel) participation in the census tract of residence on the probability of not reporting participation by households participating in the respective program according to the linked administrative data in the ACS. The controls for demographics include the number of adults and children, sex, age, education, race, disability, and citizenship status of the household head, rural, whether the head speaks English poorly, reported receipt of other programs, a linear time trend, months since last receipt, months of receipt, monthly amount received, and county fixed effects. The dependent variable in Column 9 is an indicator equal to one if the earnings information for the household is imputed and equal to zero otherwise. The mean of this variable and the corresponding elasticity are missing, because they were not disclosed for our sample. See Appendix Table 3 for the first stage results of the IV estimates. Observations are weighted using survey weights adjusted for the PIK probability using Inverse Probability Weighting. Standard errors (clustered by Census tract) in parentheses. ***p<0.01, **p<0.05, *p<0.1. The Census Bureau has reviewed this data product for unauthorized disclosure of confidential information and has approved the disclosure avoidance practices applied to this release, authorization dated October 5, 2017.

TABLE 2: LOCAL PROGRAM PARTICIPATION AND THE PROBABILITY OF NOT REPORTING RECEIPT IN THE CPS. PROBIT AVERAGE MARGINAL EFFECTS

Dependent Variable:	Not Reporting Program Receipt in Survey			
	(1)	(2)	(3)	(4)
<i>A. Results for SNAP</i>				
Participation in Census Tract	-0.307*** (0.050)	-0.144*** (0.047)	-0.151*** (0.065)	-0.262 (0.193)
Observations	2,763	2,763	2,763	2,646
Mean of Dependent Variable	0.362	0.362	0.362	0.362
Mean Tract Participation	0.313	0.313	0.313	0.313
Elasticity of Reporting w.r.t. Participation	-0.265	-0.124	-0.134	-0.226
<i>B. Results for TANF+GA</i>				
Participation in Census Tract	-1.341*** (0.433)	-0.659 (0.426)	-0.717 (0.514)	2.006 (3.136)
Observations	727	727	692	692
Mean of Dependent Variable	0.594	0.594	0.594	0.594
Mean Tract Participation	0.059	0.059	0.059	0.059
Elasticity of Reporting w.r.t. Participation	-0.134	-0.066	-0.076	0.201
Controls for Household Income	N	Y	Y	Y
Controls for Demographics	N	N	Y	Y
IV Using Distance to Nearest Office	N	N	N	Y

Notes: This table reports average marginal effects of SNAP (upper panel) and TANF+GA (lower panel) participation in the census tract of residence on the probability of not reporting participation by households participating in the respective program according to the linked administrative data in the CPS. Column 1 shows the results from a bivariate regression, column 2 controls for household income, while column 3 adds controls for household composition of adults and children, sex, age, education, race, disability, whether households are rural, report receipt of other programs, a linear trend for years of the survey, months since last receipt, months of receipt, monthly amount received, and county fixed effects. Column 4 uses distance to nearest DSS office as an instrument for SNAP and TANF+GA participation in the Census Tract. See Appendix Table 3 for the results of the first stage regressions. Observations are weighted using survey weights adjusted for PIK probability using Inverse Probability Weighting. Standard errors (clustered by Census tract) in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The Census Bureau has reviewed this data product for unauthorized disclosure of confidential information and has approved the disclosure avoidance practices applied to this release, authorization dated October 5, 2017.

Appendix Tables

APPENDIX TABLE 1: DESCRIPTIVE STATISTICS FOR EACH SAMPLE

Survey and Variables	<i>SNAP</i>			<i>TANF+GA</i>		
	Mean	Std. Dev.	Obs	Mean	Std. Dev.	Obs
ACS						
False Negative Rate	0.249	0.432	79.707	0.587	0.492	14,595
...Mail Interviews	0.157	0.363	42,380	0.497	0.500	6,283
...Non-Mail Interviews	0.298	0.457	37,327	0.618	0.486	8,312
Tract Participation	0.298	0.193	79.707	0.053	0.040	14,595
...Mail Interviews	0.266	0.190	42,380	0.045	0.040	6,283
...Non-Mail Interviews	0.315	0.192	37,327	0.056	0.040	8,312
Poverty Index	1.599	1.532	79.707	1.324	1.434	14,595
Distance to Nearest DSS office (km)	9.766	27.356	79.699	7.784	24.587	14,592
Mode of Interview: Mail	0.349	0.477	79.707	0.257	0.437	14,595
Mode of Interview: CATI	0.078	0.269	79.707	0.056	0.230	14,595
Mode of Interview: CAPI	0.573	0.495	79.707	0.687	0.464	14,595
CPS						
False Negative Rate	0.362	0.481	2.763	0.594	0.491	727
Tract Participation	0.313	0.191	2.763	0.059	0.045	727
Poverty Index	1.527	1.463	2.763	1.223	1.482	727
Distance to nearest DSS office (km)	8.098	22.540	2.646	7.081	24.270	692

Notes: This table shows descriptive statistics for the samples used in this paper. Unless indicated otherwise, statistics are for the full (linked) sample. Observations are weighted using survey weights adjusted for the PIK probability using Inverse Probability Weighting. We measure local participation by the ratio of the number of recipients in each census tract according to the administrative data in each year from 2008 to 2012 to the total number of living units according to the 2010 Census. This measure differs from the participation rate of the census tract, because we divide by the number of living units. There can be multiple cases per living unit, so that our measure can exceed one. Thus, it provides a measure of local participation that is slightly higher than the fraction of households that participate. The Census Bureau has reviewed this data product for unauthorized disclosure of confidential information and has approved the disclosure avoidance practices applied to this release, authorization dated August 9, 2016.

APPENDIX TABLE 2: THE DETERMINANTS OF FAILURE TO REPORT PROGRAM RECEIPT FROM CELHAY,
 MEYER AND MITTAG (2021), PROBIT MARGINAL EFFECTS

Survey	Program	ACS		CPS	
		SNAP	TANF+GA	SNAP	TANF+GA
Single adult, no children		-0.0592***	-0.0148	-0.0035	0.2271***
		(0.0081)	(0.0180)	(0.0388)	(0.0711)
Single adult, with children		-0.0306***	-0.0427***	0.0227	0.0577
		(0.0069)	(0.0136)	(0.0293)	(0.0573)
Multiple adults, no children		-0.0303***	-0.0474***	-0.0485	0.1370**
		(0.0065)	(0.0145)	(0.0325)	(0.0582)
Number of members under 18		-0.0211***	-0.0202***	-0.0222*	0.0405***
		(0.0023)	(0.0041)	(0.0116)	(0.0150)
Number of members 18 or older		0.0031	-0.0149***	0.0307***	0.0726**
		(0.0022)	(0.0053)	(0.0112)	(0.0298)
Rural		0.0040	-0.0026	-0.0146	-0.0248
		(0.0065)	(0.0183)	(0.0326)	(0.0689)
Hispanic		0.0410***	0.0467***	0.0554***	0.0744*
		(0.0059)	(0.0145)	(0.0208)	(0.0450)
Black non-hispanic		0.0678***	0.0877***	0.0958***	0.0683
		(0.0049)	(0.0114)	(0.0210)	(0.0451)
Other non-hispanic		0.0343***	0.0132	0.0953**	0.0014
		(0.0085)	(0.0230)	(0.0376)	(0.1271)
Male		0.0485***	0.0384***	0.0312*	0.0767**
		(0.0038)	(0.0100)	(0.0172)	(0.0376)
Disabled		-0.0947***	-0.0594***	-0.0531	
		(0.0045)	(0.0097)	(0.0691)	
Age 16-29		-0.0295***	0.0361***	0.0340	-0.0530
		(0.0066)	(0.0131)	(0.0279)	(0.0446)
Age 30-39		-0.0005	0.0111	0.0227	-0.0366
		(0.0058)	(0.0123)	(0.0252)	(0.0422)
Age 50-59		-0.0158***	0.0053	-0.0046	-0.0116
		(0.0057)	(0.0124)	(0.0267)	(0.0468)
Age 60-69		-0.0164**	0.0713***	0.0294	0.0717
		(0.0065)	(0.0162)	(0.0304)	(0.0627)
Age 70 or more		0.0009	0.0784***	0.0562*	0.2028
		(0.0068)	(0.0224)	(0.0315)	(0.1450)
Less than high school		-0.0370***	-0.0137	-0.0842***	-0.0502
		(0.0051)	(0.0109)	(0.0222)	(0.0407)
High school graduate		0.0074	-0.0094	-0.0091	-0.0308
		(0.0047)	(0.0108)	(0.0210)	(0.0408)
Complete graduate and beyond		0.0106*	0.0040	0.0225	-0.0525

	(0.0061)	(0.0162)	(0.0278)	(0.0554)
Household language is English only	0.0136**	-0.0402***		
	(0.0054)	(0.0126)		
Speaks English poorly	-0.0781***	0.0128		
	(0.0062)	(0.0158)		
Non-citizen	0.0257***	0.0080		
	(0.0060)	(0.0136)		
Household income/poverty line	0.0449***	0.0150***	0.0798***	0.0273*
	(0.0014)	(0.0038)	(0.0085)	(0.0157)
Household income/poverty line >10	-0.2396***	-0.1358*	-0.4967***	
	(0.0231)	(0.0720)	(0.1598)	
Anyone in household employed	0.0697***	0.1733***	0.0627***	0.0281
	(0.0049)	(0.0096)	(0.0206)	(0.0349)
Reported housing assistance receipt			-0.1957***	-0.0593*
			(0.0175)	(0.0314)
Reported TANF+GA receipt	-0.2189***		-0.3252***	
	(0.0065)		(0.0331)	
Reported SNAP receipt		-0.3115***		-0.3479***
		(0.0105)		(0.0338)
Linear time trend	-0.0085***	0.0147***	-0.0096**	-0.0027
	(0.0013)	(0.0029)	(0.0046)	(0.0091)
Number of observations	81,772	16,962	3,539	908
chi2 statistic of joint significance	9,337	3,451	1,269	385
p-value of joint significance	<0.001	<0.001	<0.001	<0.001

Notes: Reproduced from Celhay, Meyer and Mittag (2021) Table 4. Approved for release by the Census Bureau's Disclosure Review Board, approvals dated August 3, 2015 and August 18, 2016. The samples include imputed observations, but are restricted to recipients according to the linked data. The dependent variable is an indicator for failure to report receipt in the survey. All demographic characteristics refer to the reference person. The time trend is measured in years for the ACS and CPS. The omitted categories are Multiple Adults with Children, Age 40-49, College Graduate and White. All analyses use household weights adjusted for PIK probability. *** p<0.01, ** p<0.05, * p<0.1

APPENDIX TABLE 3: FIRST STAGE IV ESTIMATES OF PARTICIPATION EQUATION

Dependent Variable:	<i>ACS</i>		<i>ACS Mail Interviews</i>		<i>ACS Non-Mail Interviews</i>		<i>CPS</i>	
	SNAP Participation in Census Tract	TANF+GA Participation in Census Tract	SNAP Participation in Census Tract	TANF+GA Participation in Census Tract	SNAP Participation in Census Tract	TANF+GA Participation in Census Tract	SNAP Participation in Census Tract	TANF+GA Participation in Census Tract
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Distance to nearest DSS office (km)	-0.005*** (0.0001)	-0.001*** (0.0001)	-0.004*** (0.0003)	-0.001*** (0.0001)	-0.005*** (0.0004)	-0.001*** (0.0001)	-0.008*** (0.0005)	-0.001*** (0.0003)
Observations	79,706	14,595	42,379	6,283	37,327	8,312	2,646	692
F-statistic	217.812	151.283	181.733	123.126	215.903	119.911	193.903	14.333
F-test p-value	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000

Notes: This table reports the results for the first stage of the IV Probit of participation in SNAP and TANF+GA and the probability of misreporting SNAP and TANF+GA in the ACS and the CPS. The regressions controls for household income, number of adults and children, sex, age, education, race, disability, and citizenship status of the household head, whether the head speaks English poorly, reported receipt of other programs, a linear time trend, months since last receipt, months of receipt, monthly amount received, and county fixed effects. Observations are weighted using survey weights adjusted for the PIK probability using Inverse Probability Weighting. Standard errors (clustered by Census tract) are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The Census Bureau has reviewed this data product for unauthorized disclosure of confidential information and has approved the disclosure avoidance practices applied to this release, authorization dated October 5, 2017.

Appendix – The consequences of Conditioning on Program Participation

In this appendix, we formalize our argument that conditioning on program receipt biases our estimates upward and hence makes it harder for us to detect an effect of stigma. To do so, consider a system of two equations that simultaneously determine whether an individual takes-up the program ($T = 1$) and whether the individual fails to report receipt ($M = 1$):

$$T^* = P\delta^T + \varepsilon^T$$

$$M^* = P\delta^M + \varepsilon^M$$

We omit other regressors to simplify exposition and focus on local program participation, P . By virtue of decreasing stigma, P increases take-up (i.e. $\delta^T \geq 0$) and decreases misreporting (i.e. $\delta^M \leq 0$). As usual in binary choice models, the individual participates in the program iff $T^* > 0$ and does not report receiving the program iff $M^* > 0$. Our strategy of conditioning on participation amounts to truncating the unobservables that determine participation: Only those with $\varepsilon^T \geq -P\delta^T$ are included in our sample. Thus, rather than being drawn from the unconditional distribution of ε^M , F_{ε^M} , the unobservables in our sample is drawn from $F_{\varepsilon^M|\varepsilon^T > -P\delta^T}$. If the unobservables in the two equations are independent, this truncation will not affect our results. However, the two unobservable terms may well contain common factors, S , such as some part of stigma or a factor related to both types of stigma.¹ To examine the consequences of such common factors, we can write the two error terms as

¹ Note that the question of sample selection we discuss here is distinct from the problem of bias from omitting S , which could indeed bias our estimates away from 0. To focus on sample selection, we consider the case of $S \perp P$ here which implies no bias. If S is the part of stigma that our covariates do not capture, this assumption holds by construction. We discuss the problem of omitted variables in detail in the paper and provide empirical evidence that they do not bias our results.

$$\varepsilon^T = S\gamma^T + \varepsilon^{T*}$$

$$\varepsilon^M = S\gamma^M + \varepsilon^{M*}$$

To focus on this particular problem, assume that dependence of the two unobservables arises only from S , so that $\eta^* \perp \varepsilon^*$. Then the covariance of the two error terms is $\gamma^T \gamma^M \sigma_S^2$, where σ_S^2 is the variance of S , which is positive. By definition, S reduces take-up (i.e. $\gamma^T \leq 0$) and increases misreporting (i.e. $\gamma^M \geq 0$). Therefore, the covariance of the two error terms is (weakly) negative. Since $\text{Cov}(\varepsilon^M, \varepsilon^T) \leq 0$, truncating the distribution of ε^T from below tends to remove larger values of ε^M from our sample, so that $E[\varepsilon^M | \varepsilon^T > -P\delta^T] \leq E[\varepsilon^M]$. Thus, the intercept in our model is likely biased downward, since we are studying a sample with lower overall stigma.

Yet our parameter of interest is the slope of the relationship between local program participation and misreporting, γ^M . Bias in our estimate of γ^M arises if $E[\varepsilon^M | \varepsilon^T > -P\delta^T]$ changes with P . P clearly enters the conditioning set, so $\hat{\delta}^M$ may be biased. However, δ^T is positive, so increasing P decreases the (individual specific) point at which F_{ε^T} is truncated from below. Thus, as P increases, low values of ε^T become more frequent in our sample. Since $\gamma^T \leq 0$, these low values of ε^T stem from high values of S . Unobserved stigma S thus increases with P in our sample. Intuitively, at higher P , more individuals participate. This reduces the selection in terms of low unobserved stigma, so unobserved stigma rises as P rises.

But unobserved stigma S increases misreporting ($\gamma^M \geq 0$). Thus, increasing S by increasing P makes high values of ε^M become more frequent in our sample. Therefore, $E[\varepsilon^M | \varepsilon^T > -P\delta^T]$ increases with P . Thus, $\text{Cov}(\varepsilon^M, P) > 0$, so that the bias in the coefficient on P will be positive. In consequence, analyzing a sample of recipients creates

a mechanism that makes misreporting increase with local participation. It would thus bias
the negative effect we hypothesize (and find) in the positive direction.