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Pablo A. Celhay, Bruce D. Meyer, and Nikolas Mittag

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Becker Friedman Institute
FOR ECONOMICS AT UCHICAGO

5757 S. University Ave.
Chicago, IL 60637
Main: 773.702.5599
bfi.uchicago.edu

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ABSTRACT

Stigma of welfare participation is important for policy and survey design, because it deters 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 rule out explanations other than stigma. Stigma decreases when more peers engage in the stigmatized behavior and when such actions are less observable.

Pablo A. Celhay
School of Government
and Economics Institute
Pontificia Universidad Católica de Chile
Avda. Vicuña Mackenna 4860
Macul, Santiago
Chile
pablocelhay@gmail.com

Bruce D. Meyer
Harris School of Public Policy
University of Chicago
1307 E 60th Street
Chicago, IL 60637
and NBER
bdmeyer@uchicago.edu

Nikolas Mittag
CERGE-EI
Politických vězňů 7
Praha
Czech Republic
nikolas.mittag@cerge-ei.cz

I. Introduction

Individual behavior often depends on the judgement of others. The recent literature on social image concerns (surveyed in 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 academic research is welfare stigma.[1] Those eligible for government aid may not apply for it because of stigma (Moffitt 1983; Currie et al., 2001). This lack of program take-up has led many cities and states to invest significant resources to encourage participation in welfare programs by those in need. 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. We examine whether failure to report program receipt in surveys, which we observe by linking an accurate measure of actual receipt to the survey data, is negatively associated with program participation in the census tract of survey respondents. For this relation to provide evidence on the presence and nature of stigma, we need underreporting to be related to

[1] Rather than from social image concerns, stigma could also arise from self-image concerns or embarrassment. Our results below point to social image concerns, but we do not attempt to distinguish social and self-image concerns here, as we discuss in section 5.

stigma and higher local participation to decrease stigma. There are clear precedents for these two hypotheses in the literature. There is a striking asymmetry towards underreporting, which already suggests 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 for stigma to play a role in the failure to report negative information; see Celhay, Meyer and Mittag (2022) for a summary.[2] 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). We discuss these hypotheses further in section III. We also need our empirical strategy to isolate the effect of stigma from other factors that are associated with both program participation in the local network of a respondent and survey responses. We discuss this assumption in section III and provide evidence of its validity in section IV.

Our approach has several advantages. Studying misreporting avoids explanations that can easily be mistaken for stigma. Previous studies typically examine incomplete take-up, which may also be due to information and transaction costs or, alternatively, mismeasured receipt or eligibility. Examining how misreporting varies with another variable that amplifies or (in our case) mitigates stigma (local program participation) further helps us to isolate it from these confounders and avoids the need for the strong assumptions likely

[2] We build on the analysis of Celhay, Meyer, and Mittag (2022) who examine many explanations for misreporting. While that work includes a brief examination of stigma, here we explore the presence and nature of stigma in social programs in depth.

required for structural estimation. Our approach is more closely related to the identification strategy in studies of social image concerns. Such studies typically examine how actions vary with the probability of those actions being observed by peers. We can examine how actions vary with their social desirability to these peers, which is another key determinant of social image concerns. We take the approach of the literature on social image concerns, which relies on experiments in which peer valuation is difficult to manipulate exogenously, to the realm of observational studies. While this strategy comes with the disadvantage of less credible identification, we argue that observational studies can help to expand the scope of such research in section VI.

Our results provide strong evidence of stigma, as misreporting among true recipients is negatively associated with local program receipt. We provide empirical evidence from several additional analyses 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. We show that 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.

This paper proceeds as follows. Section II describes our empirical strategy and the data. Section III discusses how we isolate stigma and the advantages of our approach. Section IV reports the results. Section V discusses our findings and their importance. Section VI concludes.

II. Empirical Approach and Data

We study welfare stigma by examining how local SNAP participation and Temporary Assistance for Needy Families (TANF) or General Assistance (GA) participation affects the probability of misreporting program participation. 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. Our main specification is the following Probit model

$$\Pr\left(\mathrm{Y}_{ijc} = 1 | P_{jc}, X_{ijc}\right) = \Phi\left(\alpha + P_{jc}\delta + X_{ijc}{}'\beta\right), \tag{1}$$

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. P_{jc} is participation in program *j* in census tract *c*. Our main interest is the coefficient δ, which measures the extent to which failure to report program participation varies with local participation. We argue that this coefficient isolates an effect of stigma in the next section. In brief, we condition on a vector of control variables X_{ijc}, which includes household demographics and income as well as household information on program participation such as the number of months of participation and amounts received. In addition, we use differences in local participation due to differences in the distance to the nearest welfare office in an IV approach. To estimate these models, 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. Meyer and Mittag (2017,2019a,2021) and Celhay, Meyer, and Mittag (2021, 2022) also use these data and discuss why the data and linkage are sufficiently accurate to provide a measure of underreporting. 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 (4595 households in the ACS and 727 in the CPS) during the reference periods of the surveys. Appendix Table 1 presents summary statistics. Failure to report participation is pervasive as 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.

Our administrative records contain all cases and their addresses, which allows us to construct our measures of local participation and the distance to the nearest welfare office. We use the census tract [3] 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

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

administrative data in each year from 2008 to 2012 to the total number of living units according to the 2010 Census.

When using instrumental variables, we compute the linear distance from the center of 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. We obtained the addresses of all DSS offices from the OTDA and track office locations back to 2007 (using https://archive.org/web/). 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.

III. Isolating Stigma

Our parameter of interest, δ, measures how underreporting varies with local program participation. Interpreting our estimate of δ as an effect of stigma relies on two key assumptions: First, if stigma exists, it is correlated with both underreporting and local program participation. Second, we control for or purge all factors other than stigma that correlate with both measures. If these two assumptions hold, then rejecting the hypothesis that δ is zero establishes the presence of stigma.

The first assumption should be uncontroversial, since prior literature provides evidence that stigma can lead to failure to report program receipt and varies with local participation. Using failure to report participation to measure of stigma rests on the hypothesis that participation in welfare programs is 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 (2022) for discussion and references. Based on similar arguments, Bharadwaj, Pai and Suziedelyte (2017) measure mental health stigma by the extent of survey underreporting and DellaVigna et al. (2017) use survey non-response to study stigma of not voting.

Using local participation to measure of variation in stigma requires the extent of stigma to decrease 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; 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 recipient households have some knowledge of the frequency with which neighbors participate in each program we study, possibly through interactions at the local store, program office or by word of mouth.

Our second assumption requires more thorough justification, since other factors such as income or census tract characteristics likely also vary with local participation and survey reporting conditional on receipt. To interpret δ as an effect of stigma, we need to purge any confounders that reliably predict both local receipt and conditional reporting. Several features of our approach make it plausible that δ indeed isolates the effect of stigma.

First, we are able to directly 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. Taken together, these two strategies provide strong evidence against any confounders that are not both unobserved and systematically predicted by the distance to the nearest welfare office.

Third, to provide evidence against potential confounders that vary with both our instrument and reporting, 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 in the subsample of households interviewed either by telephone or by face-to-face interviews than among those responding by mail.

Fourth, we provide evidence against another potential explanation, which is that overall survey accuracy may vary with local participation. To explore this possibility, we conduct a falsification test by estimating the same model, but we change the dependent variable to a measure of survey cooperation (whether or not an individual refuses to answer the survey 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. We discuss how these analyses lend credibility to our assumptions further below.

Our approach has several advantages over previous studies of welfare stigma. Following Moffitt (1983), this literature usually attributes to stigma the unexplained gap between expected and actual program take-up in a structural model. A first set of advantages stems from having an accurate measure of participation in the linked data, which allows us to examine whether stigma affects program reporting. 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 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 their 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 misreporting, as supported by the findings on the determinants of misreporting in Celhay, Meyer and Mittag (2022). 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.

A second set of advantages arises from examining how misreporting varies with a second measure that mitigates stigma, local participation. This approach is only feasible because we examine stigma in reporting of participation rather than actual take-up. By using reporting among true participants, we avoid the “reflection problem” (Manski, 1993) that may give rise to an association between individual behavior (take-up) and group behavior (local participation) for reasons other than stigma. The key advantage of studying the relationship between local participation and underreporting to examine stigma is that confounding the robust negative relation we document would require other factors that strongly influence both local receipt and reporting. As we argue above and below, we provide empirical evidence against key confounders and alternative explanations. Thereby, we isolate stigma better from other factors, such as information or transaction costs, than prior studies. Finally, our approach does not require a structural model, which avoids the problem that a difference between expected and actual take-up may also arise from misspecification of expected take-up. Yet an advantage of a structural model is that it may identify the scale of the effect of stigma, which our approach cannot.

Our approach is more similar to the recent literature on social image concerns. These studies (implicitly or explicitly) add a social image term to the utility an individual derives from taking an action such as welfare receipt. Following Bursztyn and Jensen (2017, equation 1), this term depends on the probability that others observe the action, the (perceived) social desirability of the action to them, as well as how much the individual cares about the opinion of those who may observe the action. Studies of social image concerns typically use randomized control trials (RCTs) to attempt to induce exogenous variation in the probability that the action is observed. Finding an effect on the probability that the agent takes the action provides evidence that the action is associated with social image concerns. We extend this approach in two directions: by taking it to the realm of observational studies and by attempting to isolate variation in the perceived desirability of the action.

As with any observational study, a key downside of our approach is that identification rests on stronger assumptions. However, extending this approach to observational studies also has several advantages. For many actions, including welfare receipt, manipulating the probability that an agent takes the action raises ethical or practical problems that may make RCTs infeasible. See Friedrichsen, Koenig and Schmacker (2018), who study stigma in welfare take-up by randomizing observability of take-up of a fictional welfare program in the lab, for a discussion of the trade-off between identification and feasibility as well as external validity. An observational approach can therefore extend the scope of studies of social image concerns. Observational studies can also complement RCTs by shedding light on the nature of social image concerns and the mechanisms through which they affect choices. For example, 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. RCTs also typically do not collect the sample size and the detailed information of our surveys, which allow us to examine the nature of social image concerns by analyzing whether the effects we find are stronger in subsamples where stigma has been hypothesized to matter more.

IV. Results

A. Basic results

We estimate equation (1) for both programs in each survey. Table 1 reports the 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 for TANF+GA are less conclusive than those 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, so that local participation is 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, it is more suitable to compare the CPS results 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. This relationship is likely biased due to the omission of confounding variables like income. Others find income to be a strong predictor of misreporting program participation (e.g., Bollinger and David 1997). Households with lower income are also 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. This pattern holds remarkably closely 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.

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.[4] 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 8.3 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 2022). More importantly, the decrease in reporting 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 observable characteristics of the respondent. Yet, rather than stigma, the effects we find above may arise from unobserved factors that are related to both misreporting and local participation, such as a "welfare culture" or interviewers probing more for program receipt in areas with high participation. In this section, we present IV estimates to provide evidence that the effects are not due to omitted variable bias. Our instrument is the distance from

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[4] Scaled differently, a one SD increase in local SNAP participation reduces underreporting by 1.8 percentage points.

each census tract to the closest DSS office. Our instrument is strong with F-statistics between 120 and 218 in the ACS (and 14 and 16 in the CPS, see Appendix Table 2). For the instrument to be valid, it should have no effect on the probability that a true participant does not report participation, other than through its effect on local participation (conditional on our covariates). We believe that this condition is plausible. Being further away from a DSS office likely reduces program participation through higher transaction costs or less information about the program. Both are unlikely to affect reporting of benefits by recipients. Previous work documents that misreporting is mainly due to factors such as recall and income (Celhay, Meyer and Mittag, 2022). There is little evidence of geographic variation in misreporting once conditioning on demographics (Meyer and Mittag 2019b, Mittag, 2019, Fox, Rothbaum, Shantz, 2022).

Columns 4 of Table 1 and 2 show the results of the IV Probits.[5] In the ACS, a 10-percentage point increase in census tract 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 with even higher p-values in the CPS with its smaller sample.

[5] Linear IV models yield similar results, so they do not seem to be sensitive to functional form assumptions.

Consequently, the IV estimates do not provide evidence of endogeneity. Thus, they further support that we isolate stigma.

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. As previously noted, past work has found most geographic variation in misreporting to be explained by observed characteristics, but factors such as information about the program, 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. Note, however, that these factors should lead to more, rather than less reporting when local participation is high (the opposite of what we observe), but other tract-level characteristics that vary with distance to the nearest DSS office and affect reporting could bias our results. We next use variation within geographic areas to provide evidence against such alternative explanations. We do so by showing that the effect is indeed stronger in the presence of an interviewer where stigma should be more relevant as discussed in section III. Specifically, we use the ACS to repeat our analyses separately for the subsample of households that were surveyed by mail and those surveyed by an interviewer either by telephone or face-to-face.[6]

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[6] 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 in-person interview. Thus,

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 the 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 potential 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. Nevertheless, one may be concerned that respondents are similar in terms of their cooperation across areas in the absence of interviewers, but for some reason high

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reporting may differ by survey mode due to selection. Indeed, misreporting rates for SNAP are lower when no interviewer is present, but the reverse is true for TANF+GA. However, differences in misreporting levels do not invalidate our comparison of interview modes. This would require those who do not respond to the mail questionnaire to be less accurate in areas where program participation is lower, which seems unlikely. To further reduce differences due to non-random assignment, we control for all covariates included in the previous regressions.

welfare receipt areas may 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 (2022) 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 the probability of not answering the earnings question 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. To rule out explanations other than stigma, we show that this relationship cannot be explained by other factors. 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 that participation in welfare programs is associated with stigma. The intensity of stigma decreases when more people in the social network of an individual participate in the same program. We also show that stigma is stronger in the presence of others, even strangers. These findings are relevant for the literature on welfare programs and theories of social image concerns.

A first contribution to the literature on welfare stigma is that we provide strong evidence that stigma matters for welfare programs. Providing 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) and when designing welfare policy. In fact, stigma was one of the principal reasons to change the name from The Food Stamp Program to SNAP and to use electronic benefit cards instead of paper stamps (Ponza et al. 1999). 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 lead to 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 higher 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 the take-up of government programs (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 that has recently received considerable attention is social image concerns. By viewing welfare participation as a negative status good, our results on the nature of the 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.

We thus provide further evidence that status goods create “positional externalities” (Bursztyn et al. 2018), i.e. that their value depends on the degree to which they distinguish the individual from others. Bursztyn et al. (2018) propose such positional externalities as an explanation for why the willingness to pay for a status good decreases when additional customers are enabled to buy the same good. 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 seems crucial to our understanding of how social norms change and the mechanisms that lead to socially undesirable behavior being normalized in a society or among subgroups thereof (Bursztyn, Egorov and Fiorin 2020).

Our study also points toward stigma arising from social- rather than self-image concerns (Benabou and Tirole, 2011, Falk 2021), as the latter should be less dependent on program participation of peers. That the effects are weaker or non-existent when no interviewer is present further supports this view. However, 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 provides evidence 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. That is, 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.

In addition to enhancing our understanding of welfare stigma and social image concerns, this study also makes a methodological contribution by combining the separate strands of literature on these two topics. As discussed in section III, our empirical strategy provides more reliable evidence of welfare stigma, because we do not rely on structural models requiring strong assumptions. We also avoid key confounders of studying stigma in program take-up, such as mismeasured eligibility and participation, information or transaction costs.

Our results also underline that the literature on social image concerns provides a useful formal framework to study stigma. Previous studies in this literature discuss welfare stigma but do not study it empirically (Bursztyn and Jensen, 2017). We demonstrate that empirical evidence on stigma can be obtained from observational studies. We thereby show that social image concerns, which have hitherto relied on RCTs, can be examined in an observational or quasi-experimental 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, such as the effects of peer valuation. 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.

VI. Conclusion

We analyze welfare stigma by studying the relationship between underreporting of program participation and true local program participation. We rule out a wide range of alternative explanations for less underreporting when local participation is high, strongly suggesting that the negative relation we find is indeed due to stigma. The pattern is more evident when an interviewer is present and holds when high local participation is due to proximity to a welfare office. It is not due to a general tendency to respond less accurately in areas with high welfare participation. Stigma decreases when more peers participate in the same program and when reporting is not observed by an interviewer in mail-back forms.

Our results on the presence and nature of stigma are important for policies aimed at increasing program take-up as well as 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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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)
A. Results for SNAP									
Participation in Census Tract	-0.201***	-0.092***	-0.087***	-0.153**	-0.017	-0.125***	-0.003	-0.223***	-0.007
	(0.013)	(0.013)	(0.015)	(0.058)	(0.014)	(0.022)	(0.055)	(0.085)	(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	0.298	0.156	0.298	0.156	0.298	n.a.
Mean Tract Participation	0.298	0.298	0.298	-0.168	0.266	0.315	0.298	0.266	0.298
Elasticity of Reporting w.r.t. Participation	-0.240	-0.110	-0.109	79,699	-0.016	-0.142	0.020	-0.188	n.a.
B. Results for TANF+GA									
Participation in Census Tract	-0.618***	-0.118	-0.315***	-0.089	-0.264	-0.347**	0.918	-0.414	0.048
	(0.112)	(0.112)	(0.132)	(0.612)	(0.185)	(0.162)	(0.938)	(0.742)	(0.128)
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	0.618	0.497	0.618	0.497	n.a.
Mean Tract Participation	0.053	0.053	0.053	0.053	0.045	0.056	0.045	0.056	0.587
Elasticity of Reporting w.r.t. Participation	-0.056	-0.010	-0.028	-0.017	-0.023	-0.029	0.063	-0.067	n.a.
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. See Appendix Table 2 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 are in parentheses. ***p<0.01, **p<0.05, *p<0.1. n.a.: not available. 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)
A. Results for SNAP				
Participation in Census Tract	-0.307***	-0.144***	-0.148***	-0.025
	(0.050)	(0.047)	(0.055)	(0.165)
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
B. Results for TANF+GA				
Participation in Census Tract	-1.341***	-0.659	-0.826*	0.851
	(0.433)	(0.426)	(0.429)	(2.959)
Observations	727	727	727	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 2 for the results of the first stage regressions. Observations are weighted using survey weights adjusted for PIK probability using Inverse Probability Weighting. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.01. 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

	SNAP			*TANF+GA*		
Survey and Variables	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: FIRST STAGE IV ESTIMATES OF PARTICIPATION EQUATION

	ACS		*ACS Mail Interviews*		*ACS Non-Mail Interviews*		*CPS*	
Dependent Variable:	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***	-0.001***	-0.004***	-0.001***	-0.005***	-0.001***	-0.005***	-0.001***
	(0.0001)	(0.0001)	(0.0003)	(0.0001)	(0.0004)	(0.0001)	(0.0012)	(0.0003)
Observations	79,699	14,592	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	15.685	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 are in parentheses. *** p<0.01, ** p<0.05, * p<0. 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.