The Deadweight Loss of Social Recognition*

Luigi Butera,† Robert Metcalfe,‡ William Morrison,§ and Dmitry Taubinsky¶

March 16, 2019

Abstract

A growing body of empirical work shows that social recognition of individuals' behavior can meaningfully influence individuals' choices. This paper studies whether social recognition is a *socially efficient* lever for influencing individuals' choices, relative to standard financial incentives. Because social recognition generates utility from esteem to some but disutility from shame to others, it can be either positive-sum, zero-sum, or negative-sum. We show that this depends on whether the social recognition utility function is convex, linear, or concave, respectively. We develop a new revealed preferences methodology that allows us to investigate this question, as well as to structurally estimate leading models of social signaling and their equilibrium implications. We deploy the methods in a field experiment on promoting attendance to a YMCA health and fitness center. We find that: (i) social recognition increases attendance by 17-23%; (ii) social recognition payoffs strictly increase in attendance and are negative for some; and (iii) the social recognition utility function is significantly concave and thus generates deadweight loss. If our social recognition intervention were applied to all members, the models imply that it would generate deadweight loss of $1.13-$2.15 per dollar of behaviorally-equivalent financial incentives.

*We are indebted to Ryan Johnson and Erik Daubert from the YMCA of the USA for their invaluable guidance and expertise in setting up our research partnership, as well as to Maria-Alicia Serrano for her continuing support. We wish to thank the YMCA of the Triangle Area, particularly Tony Campione, Mark Julian, Brian Spanner, and Janet Sprague, for their outstanding support in implementing our field experiments. We are grateful to Leonardo Bursztyn, Gary Charness, Alain Cohn, Jonathan Davis, Stefano DellaVigna, Ray Fisman, Jana Gallus, David Gill, Alex Imas, John List, Keith Ericson, Fatemeh Momeni, Na'ama Shenhav, Ricardo Perez-Truglia, Daniel Tannenbaum, as well as numerous conference and seminar participants for helpful comments. The views expressed herein do not necessarily reflect the views of the YMCA of the USA or any other YMCA member association. The experiment was approved by University of Chicago IRB, #IRB15-1647. AEA RCT ID: AEARCTR-0004004

†Copenhagen Business School. Email: lbu.eco@cbs.dk.

‡Boston University. Email: rdmet@bu.edu

§UC Berkeley. Email: wmorrison@berkeley.edu

¶UC Berkeley and NBER. Email: dmitry.taubinsky@berkeley.edu

1 Introduction

The human desire for social recognition is a powerful motivator. Many organizations around the world leverage this to affect a variety of economically important behaviors (Loewenstein et al., 2014; Bursztyn and Jensen, 2017). For instance, 89% of businesses use some form of social recognition programs (WorldatWork, 2017), including examples like "employee of the month" (Kosfeld and Neckermann, 2011). Bloom and Van Reenen (2007) find that 60% of manufacturing companies publicly reveal and compare employees' performance data. Governments also use social recognition programs to motivate citizens to pay their taxes (Bø et al., 2015; Perez-Truglia and Troiano, 2018), to motivate bureaucrats to do a better job (Gauri et al., 2018), and to encourage teachers, doctors, and managers in schools and hospitals to improve their performance. Charities often utilize social recognition in the form of giving circles (Karlan and McConnell, 2014).

Recent field experiments confirm that public recognition of individuals' behavior does, indeed, have substantial effects on behavior in a number of economically important domains. Examples include increasing charitable and political donations (Perez-Truglia and Cruces, 2017) by recognizing the donors; increasing tax compliance by publicizing it to neighbors (Perez-Truglia and Troiano, 2018); affecting education and career choices by manipulating the observability to one's peers (Bursztyn and Jensen, 2015; Bursztyn et al., 2017b, 2019); increasing employee productivity by publicizing ranks (Barankay, 2011; Ashraf et al., 2014; Bradler et al., 2016); increasing voter turnout by publicizing voting records to neighbors (Gerber et al., 2008); increasing childhood immunization by publicizing progress through the bracelets given to children (Karing, 2019); increasing the sign up rates for energy conservation programs (Yoeli et al., 2013); and increasing the take-up of credit cards by making them a status signal (Bursztyn et al., 2017a).

Yet social recognition is an emblematic example of why it is crucial to carefully quantify the costs and benefits generated by non-financial levers. It is a positional good: not everyone can be at the top, and so greater visibility brings esteems to some but shame to others. The total sum of the gains from esteem and the losses from shame determines the economic efficiency of utilizing social recognition to affect behavior.

In this paper, we develop a novel approach to analyzing the welfare effects of changing behavior using social recognition. Although it is often assumed that one's social recognition utility is linear in the audience's inferences about one's type (e.g., Bénabou and Tirole 2006, 2011; Ali and Bénabou 2016), we show that this assumption is far from innocuous for questions about the efficiency of social recognition interventions. Deviations from linearity can generate *deadweight loss from social recognition*—the extent to which surplus would be higher if behavior change was instead achieved by revenue-neutral financial incentives. Any intervention that leverages social recognition—whether fully or partially revealing—will generate deadweight loss if the social recognition utility function is concave. This result is a consequence of Jensen's inequality. Intuitively, the gains from esteem for the above-average types will be less intense than the losses from shame for the below-average types. Conversely, the intervention will generate additional social surplus if the social recognition utility function is convex.

We then develop a revealed preferences methodology for estimating the curvature of the social recognition utility function. We do this by eliciting people's (possibly negative) willingness to pay for social recognition conditional on different possible realized future behaviors. This method is robust to forecasting biases that would otherwise invalidate a revealed preferences welfare analysis of non-price levers such as ours.

We implement our approach in a field experiment conducted in partnership with the YMCA of the USA[1] and the YMCA of the Triangle Area (YOTA) in Raleigh, North Carolina.[2] We invited all members of YOTA to participate in a newly designed one-month program called "Grow & Thrive". This program encouraged members to attend their local YMCA more often by having an anonymous donor give $2 to the local YMCA for each day that an individual attended the YMCA. Participants could also be randomly assigned to an additional treatment group, the "social recognition program," (SRP) which would reveal each participant's attendance and donations raised to all other participants in SRP.

To directly estimate a money-metric measure of consumer surplus from social recognition, we elicited willingness to pay (WTP) for social recognition as follows: Prior to the start of the month-long period during which incentives for attendance were provided, participants in our experiment were asked to provide their WTP (possibly negative) for being in the social recognition group, *for each possible realization of their attendances during Grow & Thrive*. To make this incentive compatible, WTP was elicited using the Becker-DeGroot-Marschak (BDM) mechanism.[3] But to generate random assignment, as well as to minimize any negative inferences that could be drawn about participants who are not in the social recognition group, we guaranteed that the BDM responses would be used to determine assignment only with 10% chance. With 90% chance participants would be randomly assigned to be in the social recognition group or not, independent of their BDM decisions. This information was common knowledge among participants. Prior to making their WTP decisions, individuals were also informed about the average YOTA attendance in the prior month.

Our findings are threefold. First, we document that social recognition increases YMCA attendance by approximately 17-23% during the treatment month. This percent increase is roughly the same for individuals with low and high prior attendance.

Second, we provide a direct, non-parametric quantification of social recognition utility, and confirm the key monotonicity assumption of all models of social recognition. We find that individuals' WTP for social recognition, conditional on the number of times they might attend the YMCA during Grow & Thrive, is strictly increasing in this potential future attendance: it ranges from a -$1.70 for zero attendances to $2.61 for twenty-three or more attendances. Perhaps most

[1]The YMCA of the USA is a national, non-profit, charitable organization that supports local communities with a focus on youth development, healthy living, and social responsibility. Please see http://www.ymca.net/.

[2]One of the 850 member association YMCAs, YMCA of the Triangle Area primarily serves the Raleigh-Durham, North Carolina and surrounding communities.

[3]As we argue in Section 3, WTP in this mechanism directly reflects the (forecasted) hedonic effects of social recognition, and is not affected by preferences for commitment if there are time-inconsistent individuals in the population.

importantly, these data directly reveal that social recognition produces both winners and losers.

This finding of both winners and losers sets up our core question: is social recognition positive-sum, zero-sum, or negative-sum? To answer this question, we begin by documenting significant concavity in our non-parametric elicitation of social recognition utility. This implies that social recognition is negative-sum both in "action-based" models in which individuals care about how their action compares to the average (e.g., Becker, 1991; Besley and Coate, 1992; Blomquist, 1993; Lindbeck et al., 1999), and in "type-based" models in which individuals care about what their action reveals about their "type" (e.g., Bénabou and Tirole, 2006; Andreoni and Bernheim, 2009; Ali and Bénabou, 2016).

We then build on this qualitative finding by estimating structural models of social recognition utility. These structural models allow us to quantify the behavior change and deadweight loss that would be generated if our social recognition intervention were applied broadly to all members of the YMCA of the Triangle Area. Performing such an exercise requires characterizing the equilibria of our models to take into account the fact that such an intervention would shift the equilibrium, and thus the social recognition payoffs tied to any one action. For example, individuals who evaluate their behavior relative to the average would have a more demanding average to compare to, since a broadly applied social recognition intervention would increase everyone's attendance. We estimate that a social recognition intervention would change behavior by additional 0.60-0.72 attendances per month, a change that could be equivalently produced by financial incentives of \$0.27-\$0.36 per attendance. However, this change in behavior generates \$0.32-0.35 of deadweight loss, or \$1.23-\$2.15 per dollar of behaviorally-equivalent financial incentives.

The finding of significant deadweight loss not only generates considerations for policy, but also for the efficiency of the social recognition incentives used by employers and other interested organizations. In our setting, individuals are significantly over-optimistic about their future attendance, which would lead them to overvalue the benefits of being in an environment in which their attendance is socially recognized. In the last part of the paper we argue that markets may over-use social recognition incentives in lieu of financial incentives if individuals mis-forecast their future performance.

As we emphasize in the concluding section, our contributions are methodological as much they are substantive. We end by discussing a number of reasons for why caution should be taken in extrapolating too strongly from our specific results. But we hope that in future work, researchers can utilize or build on our methods to analyze not only the effects of social recognition on behavior, but also on social surplus.

Our research is related to several literatures. The most closely related is the large and growing field experimental literature studying the effects of social recognition on individual behavior. However, this literature does not ask whether social recognition is a socially efficient means of bringing about that behavior change. We build on this literature by setting it in a generally applicable welfare framework that extends the central economic concept of deadweight loss.

Our work also relates to a recent literature that evaluates the welfare effects of scalable, "nudge-style" non-price interventions such as reminders (Damgaard and Gravert, 2018), energy-use social comparisons (Allcott and Kessler, 2019), calorie labeling (Thunstrom, 2019), and defaults (Carroll et al., 2009; Bernheim et al., 2015). Beyond analyzing a different and highly popular non-price intervention, we add several technical innovations to this important literature. First, while these papers study the impact of the intervention on consumer surplus only, we extend the concept of deadweight loss to study how socially efficient the intervention is in bringing about the desired behavior change. The focus on deadweight loss makes it possible to consider the efficiency rankings of an arsenal of different possible policy levers, and to have a direct comparison to standard financial incentives. As a simple analogy, while other papers can be seen as evaluating, e.g., the welfare effects of fuel economy standards, our paper provides a framework for asking whether carbon emissions are most efficiently reduced through fuel economy standards or Pigovian taxation or some other policy.

Second, our field experiment utilizes a new design technique, grounded in "strategy method" approaches typically only used in laboratory experiments, that eliminates the need to rely on the assumption that individuals can correctly forecast their future behavior.[4] We establish the need to relax this assumption in our setting, and we discuss its relevance for other studies.

Finally, our model-based design allows us to produce the first structural estimates of leading models of social recognition such as those of Bénabou and Tirole (2006).[5] We therefore also contribute to a recent and growing literature in structural behavioral economics (see DellaVigna, 2018 for a review). The work by DellaVigna et al. (2012) and DellaVigna et al. (2017) is closest in spirit to our paper in this literature, although they do not study the scalable lever of revealing peoples' behavior to others, nor do they estimate the leading social recognition models. These two papers quantify the cost of social pressure exerted by a solicitor to donate, and of the social pressure to tell a get-out-the-vote surveyor that one has voted, respectively.[6] They do this by using structural methods to infer the cost of social pressure from the degree to which individuals avoid interaction with others. In contrast, we use conceptually different, and more direct experimental techniques that leverage the richness of our action space and allow us to directly observe the shape of utility from the social motives. We also develop structural estimation methods for making *out-of-sample predictions* that take into account that the effects of a non-price lever may change in

[4]See also Bernheim and Taubinsky (2018) for a more detailed discussion of the weaknesses of this assumption, as well as the "non-comparability problem" that less theory-grounded approaches such as those of Allcott and Kessler (2019) are subject to.

[5]Karing (2019), Bursztyn et al. (2019), Ariely et al. (2009), and Exley (2018) test comparative statics of the Bénabou and Tirole (2006) model, and Karing (2019) quantifies the value of sending a positive (but not fully-revealing) signal. These papers do not estimate the underlying social recognition utility function.

[6]We delineate between social pressure and social recognition. Social pressure commonly refers to situations in which individuals take actions as a response to direct peers' influence or requests; an example is DellaVigna et al. (2012), where the social pressure is a force layered on top of the information already revealed by choosing to use a do-not-visit tag. Social recognition instead refers to situations in which individuals take actions to influence others' beliefs about them. In some settings both are in play; e.g., when telling a surveyor whether or not one has voted, as in DellaVigna et al. (2017). The implicit assumption of the DellaVigna et al. (2017) model that not answering the door to a pre-announced visit (an action most likely to be taken by those who did not vote) generates no disutility beyond hassle costs is more consistent with a social pressure interpretation and less consistent with leading social signaling models such as those of Bénabou and Tirole (2006) and Andreoni and Bernheim (2009).

equilibrium. We do this by formally working out the (somewhat different) equilibrium predictions of the microfounded models that we estimate.

The remainder of the paper is organized as follows. Section 2 introduces our theoretical framework and provides our definitions of deadweight loss. Section 3 lays out our experimental design. Section 4 reports the reduced-form results from our experiment. Section 5 presents our estimates of the structural models, and the deadweight loss that they imply. Section 7 concludes by discussing limitations, robustness, and questions for future research.

2 Conceptual framework for analysis

In this section we begin by describing two models of preferences over social recognition. In the first model, individuals care about how their observed performance compares to the average performance. In the second model, individuals care about what the audience infers to be their "type." After presenting the models, we formally define the deadweight loss of social recognition that arise from both models. We conclude by discussing the challenges of estimating these models using experimental data.

2.1 The models

We consider individuals who choose the level of intensity $a \in \mathcal{A} \subset \mathbb{R}^+$ to engage in some activity. Choosing a generates *material utility* $u(a;\theta) + y$, where y is the individual's income and θ is the type of the individual, distributed according to some atomless distribution F over $\mathbb{R}$. Assuming that utility is linear in income is a simplifying assumption that is not crucial for our theoretical exposition, but that is realistic given the relatively small financial stakes of our experimental setting. We assume that $u(a;\theta)$ is single-peaked in a and that $\frac{d}{da}u(a;\theta)$ is increasing in θ and is bounded. In words, each individual has some optimal intensity level $a^*(\theta)$, and higher types derive more benefit from choosing higher levels of a. In addition to material utility, individuals also derive social recognition utility S, which we define below.

2.1.1 Action-based social recognition

The first model that we consider posits that when an individual's action is made public, the individual cares about how his action compares to the average action of the population (Becker, 1991; Besley and Coate, 1992; Blomquist, 1993; Lindbeck et al., 1999, 2003). Formally, utility is

$$u(a;\theta) + y + \nu S(a - \bar{a}),$$

where ν is the visibility parameter, $\bar{a}$ is the average action in the population, and S is increasing and satisfies $S(0) = 0$. The model captures the simple intuition that individuals derive "pride" from doing better than the average, and "shame" from doing less well than the average.[7] We follow Ali and Bénabou (2016) in defining the visibility parameter ν to capture the degree of visibility, such as the number of observers. Reference points other than the average behavior $\bar{a}$ are possible (e.g., the median); but as we shall show, the reference point of average behavior matches our results well.

2.1.2 Type-based social recognition

The second model posits that individuals derive utility from what their action reveals about their type to the audience (e.g., Andreoni and Bernheim, 2009; Bénabou and Tirole, 2006; Ali and Bénabou, 2016). Formally, utility is

$$u(a;\theta) + y + \nu S(E[\theta|a];\bar{\theta})$$

where ν is the visibility parameter, $E[\theta|a]$ is the audience's inference about θ given observed action a, $\bar{\theta}$ is the average type in the population, and S is increasing in $E[\theta|a]$ and satisfies $S(\bar{\theta},\bar{\theta}) = 0$. The last assumption that $S(\bar{\theta},\bar{\theta}) = 0$ simply says that no matter how large the audience, if no information is revealed about a person's type then the utility from social recognition is zero. Our formulation is identical to that of Ali and Bénabou (2016), with the single but crucial difference that we do not assume that S is linear.

2.2 The deadweight loss of social recognition

In both models, individuals who are recognized as being below average incur utility losses, while individuals who are recognized as above average enjoy utility gains. Whether social recognition is positive-sum, zero-sum, or negative-sum in either model depends on the curvature of the social recognition utility function S. For example, if S is (strictly) concave, and if E_θ denotes the expectation with respect to types θ who choose actions $a(\theta)$, then Jensen's Inequality implies

$$E_\theta[S(a(\theta)-\bar{a})] < S(E[a(\theta)-\bar{a}]) = S(0) = 0 \tag{1}$$

in the action-based model[8] and

$$E_\theta[S(E[\theta|a(\theta)];\bar{\theta})] < S(E_\theta[E[\theta|a(\theta)]];\bar{\theta}) = S(\bar{\theta},\bar{\theta}) = 0 \tag{2}$$

in the type-based model. That is, concavity of S makes social recognition negative-sum. Conversely, identical arguments show that convexity of S makes social recognition positive-sum, and linearity

[7] Although this class of models is typically formulated for deterministic economic environments such as ours, in which actions are perfectly observed, the model is easily extended to allow for imperfect observability. Given audiences beliefs $H(\cdot|s)$ about the action a given some signal σ about the action, expected social recognition utility can be defined as $\int S(a-\bar{a})dH(a|\sigma)$.

[8] In a more general version of this model in which actions may be imperfectly observed, defined in footnote 7, the average payoffs from social recognition are similarly given by $E_\theta[E[S(a-\bar{a})|\sigma(\theta)]] < S(E_\theta[E[a(\theta)-\bar{a}|\sigma(\theta)]]) = S(0) = 0$.

makes it zero-sum.[9] Despite its strong implications for social welfare, linearity is a commonly made assumption in papers that study the broader policy implications of social recognition, such as Bénabou and Tirole (2011), Ali and Bénabou (2016) and others.

These implications of curvature hold for any kind of public recognition scheme, not just one that reveals individuals' actions perfectly. This includes, for example, two-tier social recognition schemes that publicize only the behavior of the top performers. In the type-based model, suppose that instead of observing an action, the audience observes some signal σ about a person's type (which can be affected by the actions undertaken). Replacing $a(\theta)$ with $\sigma(\theta)$ in equation (2) generates the identical inequality. The same holds for the action-based model, as worked out in footnote 8. Intuitively, this is because *not* being recognized as a top performer is a signal about one's actions and type. Those individuals would prefer to be seen as "average" among everyone, rather than "average" among all but the top.

Intuitively, the question we seek to answer is the extent to which social recognition is positive-sum, zero-sum, or negative-sum; that is, what is the value of $E[S]$? We formalize this by extending the economic concept of deadweight loss to apply to social recognition. Recall that the deadweight loss of a distortionary tax is the amount by which consumer surplus would be higher if the same tax revenue were instead raised through a lump-sum tax instead; that is, it is the impact on consumer surplus relative to the benchmark of a lump-sum transfer. To define the deadweight loss of social recognition, the benchmark we adopt is revenue-neutral financial incentives that achieve the same change in behavior—for each type θ—that social recognition does.

Definition 1. *The* deadweight loss of social recognition *is the amount by which consumer surplus would be higher if the same behavior (by type) change were instead produced by revenue-neutral financial incentives.*

Appendix A provides a formal definition of deadweight loss that follows the deadweight loss of taxation definition in Auerbach (1985), and that is applicable to utility functions that are not quasilinear in income. But for the quasilinear case, the deadweight loss definition is equivalent to simply taking the average of the realized values of S in the population. An immediate implication of equations (1) and (2), therefore, is that when S is concave, social recognition produces positive deadweight loss. Conversely, when S is convex, social recognition produces negative deadweight loss; i.e., social surplus. Finally, when S is linear, social recognition produces zero deadweight loss.

Quantitatively, these effects are magnified by higher visibility ν. These effects may also be affected by nature of the social recognition scheme: publicizing everyone's behavior rather than just those of the top performers will lead to both different effects on behavior and to different deadweight loss estimates (but will not change the sign of the deadweight loss estimates, as we argued above).

These considerations imply that the deadweight loss statistic in Definition 1 does not make clear

[9] There is also a possibility that the curvature of S changes signs as the attendance changes. For example, we could in theory have a convex S for $\theta < \bar{\theta}$ and concave S for $\theta \geq \bar{\theta}$. We show in Section 4.3 that the reduced form S in our setting is well approximated by a quadratic function with global curvature.

in a general sense how inefficient (or efficient) social recognition is relative to financial incentive schemes. In particular, note that both deadweight loss and the change in behavior are increasing in ν, and thus deadweight loss might be small either because the visibility is low or because S exhibits little curvature. It is thus more appropriate to consider a normalized, unitless measure of deadweight loss, that is not tied directly to ν.

To do so, we define $p_\nu(\theta)$ to be the linear piece-rate incentive that would have to be given to each type θ to induce the same change in behavior as the change created by increasing visibility from 0 to ν. We call $\bar{p}_\nu = Ep_\nu(\theta)$ the *equivalent price metric* (EPM) of the social recognition intervention.[10] We use this to construct the following unitless measure:

Definition 2. *The* deadweight loss per dollar of behaviorally-equivalent financial incentives *is the deadweight loss per unit change in behavior, divided by the equivalent price metric.*

Note that simply dividing deadweight loss by its total impact on behavior generates a measure that is in units of dollars per unit of action, and is thus inconveniently tied to the units in which behavior change is measured. Direct comparisons of deadweight loss across different contexts would thus not be possible with such a measure. The unitless measure in Definition 2, however, enables such direct comparisons.

As with the analysis of the deadweight loss of taxation, we separate the question of deadweight loss from questions about the aggregate benefits of the policy. In the same way that questions about deadweight loss of taxation do not touch on how the tax revenue will be used, questions about deadweight loss of social recognition do not touch on the benefits of behavior change itself. Instead, our question is about the efficacy of social recognition, compared to to standard financial incentives, in producing the desired behavior change. Just as fuel economy standards can improve social welfare but are second-best to Pigouvian taxes, social recognition has the capacity to increase welfare while simultaneously creating deadweight loss relative to behaviorally-equivalent financial incentives. The goal of the deadweight loss concept is to facilitate such economic efficiency comparisons.

2.3 Measuring deadweight loss using experiments

Often, the economic questions of interest are about the effects of utilizing social recognition on a whole population, not just the experimental sample. Answering this question requires an additional step of analysis, because the equilibrium response of an individual in an experiment can be very different from equilibrium response of that same individual when social recognition is scaled up to the broader population.

To formalize, call $R : \mathcal{A} \to \mathbb{R}$ the *reduced-form social recognition function* which assigns, for each value a, a social recognition payoff. Let R_{exp} denote the function elicited for the experimental population during the experiment, and let R_{pop} denote the reduced-form social recognition function that would result if social recognition was applied to the whole population of interest. These two

[10] For types θ who do not change their behavior because their optimal choice of action is at a corner, $p_\nu(\theta)$ is undefined. The average $\bar{p}_\nu$ averages only over types for which the p_ν is defined.

objects can be meaningfully different: when the social recognition lever is applied to the whole population, population behavior changes, and thus the benchmark for what is considered relatively good behavior changes as well.

As a simple example, suppose that in our YMCA setting, an individual is observed to have attended the YMCA four times during the month of the experiment, and that average population attendance is 3.5 attendances. In the context of the experiment, an individual attending four times would thus receive positive social recognition payoffs under the action-based social recognition model. However, suppose that after applying the social recognition intervention to the whole population, average attendance would increase to 4.5 attendances. Then an attendance of four would actually generate negative social recognition utility.

Building on this insight, it is apparent that even if the experimental population was perfectly representative of the full population, average social recognition utility could be positive in the experiment, despite S being concave and social recognition being negative-sum in reality. Thus, the inference based on R_{exp} might generate misleading conclusions about deadweight loss.

An important element of our analysis will be to utilize microfounded models of social recognition to extrapolate S from R_{exp}, and consequently to obtain R_{pop}. Our reading of existing literature studying social comparisons and social pressure is that it stops at R_{exp}.[11]

The questions about equilibrium responses are separable from other concerns about external validity, such as the possible non-representativeness of the experimental sample. For example, in our experiment we do not find any interaction between social recognition utility and observable characteristics, but we cannot rule out selection on unobservables that are correlated with social recognition utility functions. These other questions about external validity (for a review, see Duflo et al., 2007; Al-Ubaydli et al., 2017; Banerjee et al., 2017; Davis et al., 2017) are important questions for assessing any kind of field experiment. Our point about equilibrium, however, is intrinsically connected to the formal microfoundations of the intervention being tested. While signaling models are, intrinsically, equilibrium models, there are many "nudge-style" interventions, such as salient information disclosure, where the treatment effects are not determined in equilibrium.

[11]For example, suppose that individuals' utility in the Allcott and Kessler (2019) is a decreasing function of the difference between their energy use and the energy use of the neighbors they are shown. Then the utility that they receive from the information mailer depends on whether the mailer goes out to their neighbors as well. However, since not everyone received the mailer in the experiment, the reduced-form effects that they estimate cannot be used to directly evaluate the policy of sending out mailers to all households. To perform such an evaluation, it would be necessary to take a stand on the structural utility function for social comparisons, to estimate it using the experimental results, and to estimate the counterfactual equilibrium of sending the mailers to everyone in the population.

As another example, consider evaluating individuals' utility from encountering a surveyor who asks about voting behavior. DellaVigna et al. (2017) estimate the utility of doing so after the votes have already been cast. But to evaluate the equilibrium impact of increasing the visibility of one's voting behavior, it is necessary to account for the fact that visibility also changes voting behavior, which changes the payoffs one receives from telling a surveyor if one has voted or not. Evaluating the equilibrium outcomes would thus require one to estimate the structural microfoundations of why individuals like to tell others that they voted.

3 Experimental design

3.1 Overview

The field experiment was conducted in collaboration with the YMCA of USA and the YMCA of the Triangle Area in North Carolina (YOTA), and was publicly called "Grow & Thrive". YMCA members of two large YMCA facilities from YOTA were invited via email to sign up to this program by completing a survey. They were informed that for every day that they attended the YMCA during the program month, an anonymous donor would make a $2 donation to their YMCA branch.[12] The survey also informed participants that they could become part of a second program—the "social recognition program" (SRP). Anyone enrolled in SRP would receive an email at the end of Grow & Thrive revealing the attendance, and thus money generated, of every participant of SRP. Figure 1 provides a screenshot of the what this social recognition email entailed. Key screenshots of instructions and communications are in Appendix C.

3.2 Recruitment

The Grow & Thrive program ran from June 15, 2017 to July 15, 2017. On June 1, 2017, the 15,382 members of the two YOTA branches received an email from their local YMCA announcing the launch of a new pilot program aimed at helping YMCA members to stay active and support their community at the same time.[13] The initial email informed participants about the Grow & Thrive program. The email included a link to an online survey. YMCA members were told that they could sign up for the program by completing the survey and agreeing to participate. Importantly, participants were told that by agreeing to participate in Grow & Thrive they would be giving their consent to be potentially randomized into the social recognition program.

3.3 Online decisions

The online experiment consisted of three parts. Part one of the experiment began by explaining the nature of the incentives during the program. Participants were told that an anonymous benefactor with an interest in promoting healthy living and supporting the broader community provided funds to incentivize YOTA members to attend their local YMCA more frequently. During the month of the Grow & Thrive program, a $2 donation was made on participants' behalf for each day a participant visited the YMCA, up to a total donation of $60 per person (e.g., 30 visits).

In part two, participants were told that they might also be randomly selected to participate in SRP. We explained that if a participant was selected into this additional program, he/she would receive an email at the end of Grow & Thrive, which would: (1) list the names of everyone in SRP; (2) list their attendance during Grow & Thrive; and (3) list the total donations generated by them

[12] Previous research has shown that people are motivated to undertake actions if there are benefits to charity or the public good (e.g., Ariely et al., 2009; Ashraf et al., 2014; Imas, 2014; Gosnell et al., 2016).

[13] Figure C.1 provides a snapshot of the template email received by the YMCA members.

during Grow & Thrive. We explained that only participants in SRP would receive and be listed in the email.

We then elicited people's willingness to pay for receiving (or avoiding) social recognition (e.g., being part or not of the recognition program) using a combination of the *strategy method* and the Becker-DeGroot-Marschak elicitation method (BDM). The strategy-proof method contained 11 questions asking participants to state whether at the end of the program they wanted to participate in the social recognition program for different numbers of realized attendances during Grow & Thrive, and then eliciting for each question how much they were willing to pay (between \$0 and \$8) to guarantee that their choice was implemented (the BDM component). The categories of possible visits were the following: 0 visits, 1 visits, 2 visits, 3 visits, 4 visits, 5 or 6 visits, 7 or 8 visits, 9 to 12 visits, 13 to 17 visits, 18 to 22 visits, and 23 or more visits.[14]

Each of the eleven questions had the following structure: "*If I go X times to the YMCA during Grow & Thrive I would prefer to participate (NOT participate) in the personal recognition program*". Participants were then asked to state, for each of the 11 levels of possible attendance, how much of the experimental budget of \$8 they would be willing to give up to guarantee that their decision about social recognition was implemented. The wording was "*You said you would rather be part (NOT be part) of the personal recognition program if you go X times to the YMCA. How much of the \$8 reward would you give up to guarantee that you will indeed be part (NOT be part) of the personal recognition program?*" Although the procedure was involved, we told participants that, first and foremost, it was in their best interest to answer truthfully. The details were then explained in simple and plain language. Figure C.2 provides a screenshot from the survey of one of the pairs of questions.

To preserve random assignment, as well as to minimize any negative inferences that could be drawn about those not in the social recognition group, we guaranteed that the BDM responses would be used to determine assignment only with 10% chance. We explained to participants that they would have a 10% chance of receiving an additional \$8 reward in the form of an Amazon gift card, the contents of which would be used to "pay" to implement their choices. If they were selected to be in that 10%, at the end of the experiment a computer would check how many times they actually attended the YMCA. The computer would then randomly choose a number between \$0 and \$8. If the computer chose a value smaller or equal to the amount they would give up to receive (or avoid) social recognition for the level of their actual attendance during the experiment, then their favorite decision about the SRP would be implemented. In this case, their extra reward would either be equal to \$8 with 50% chance, or equal to \$8 minus the amount chosen by the computer with 50% chance. If instead the computer chose a value larger than the amount that they would give up to receive/avoid social recognition, then their favorite decision would be implemented with only 50% probability and they would receive a reward of \$8.[15]

From subjects' perspective, this procedure is equivalent to a second price sealed-bid auction

[14] We believed that asking more than eleven outcomes would be overly burdensome and increase attrition.

[15] Participants were also told that each draw for each participant is independent.

against an unknown bidder. Note that providing a 50-50 chance to receive the desired option whenever random draws are above participants' bids was necessary, as otherwise participants would have had an incentive to misrepresent their true preferences in the first part of the "yes/no" elicitation and then simply bid zero.[16] In essence, participants were able to pay to deviate from a 50% chance of receiving their least favorite option.

Because others' behavior plays a crucial role in social recognition payoffs in the models summarized in Section 2, it was important to ensure that participants had accurate beliefs about others' behavior. Prior to making their decisions about being part of the SRP, participants were therefore informed about the average attendance to their local YMCA during the prior month.[17]

Part three elicited participants' beliefs about their future attendance during Grow & Thrive. We asked how many times they believed they would attend during the month of the experiment if: (1) they happened to be randomly selected to participate in the social recognition program; (2) they happened not to be selected to SRP; (3) if they happened not to be part of Grow & Thrive (and consequently of SRP). Finally, we reminded participants that a computer would randomly determine whether they would be part of SRP, and we asked them to explicitly agree to participate in Grow & Thrive.

3.4 Randomization and balance procedures

We randomized our 428 participants into the social recognition group by blocking and balancing over WTP survey responses and attendance in the thirteen months preceding the experiment. 192 participants were randomly assigned to participate in Grow & Thrive but not in the social recognition program, 193 participants were randomly assigned to participate in both Grow & Thrive and the social recognition program. 43 participants were randomly assigned to participate in Grow & Thrive, receiving the extra $8 reward for themselves, and to be part or not of the social recognition program based on their survey answers and their attendance. All participants were notified by the YMCA of the Triangle via email about their treatment assignment the morning of the first day of Grow & Thrive. The 43 participants for whom the participation in the social recognition program is endogenous are excluded from our empirical analysis.

[16] In a standard BDM procedure, participants bid to acquire a given good or service, which they do not receive if their bid is lower than the random draw. In our context, this approach is unfeasible because people may derive positive utility from not receiving the good (e.g., social recognition). Such standard formulation of a BDM would therefore not be incentive compatible because participants could report wanting their least favorite option and set the WTP for this undesirable option to zero, effectively securing their desired outcome with 100% probability. Our novel extension of the BDM procedure ensures that it is, in fact, incentive compatible for subjects to indicate their preferred option honestly and to bid their true WTP to have their preferences implemented.

[17] In principle, the desired information is actually attendance during the treatment month. In practice, we could not provide this information, nor is that attendance very different from attendance in the month before. Note also that we argue in Section 5 that the relevant information is about the attendance of all YMCA participants, not just those who happened to be in the treatment arm.

3.5 Communications

All communications with YMCA members took place via email. We prepared a FAQ document covering common questions YMCA members might have about the program.[18] To guarantee the consistency of the responses, and to minimize the burden on YMCA employees, we instructed employees working at the front desk to encourage members to address their questions via email to a specific contact person at the YMCA; the contact person would then use the answers provided in the FAQ to respond.[19]

As mentioned, all participants were notified via email about their treatment assignment before the beginning of Grow & Thrive. As Figure 1 shows, participants assigned to the social recognition treatment received a reminder summary of the social recognition treatment when they were notified of their assignment.

3.6 Administrative attendance data

The YMCA of the Triangle Area provided us with administrative attendance data for both of the branches with which we conducted the experiment. Members access the YMCA facilities by swiping their personal YMCA card on a turnstile, and front desk employees check that the access cards belong to the users. We used the recorded access timestamp as our outcome variable.[20] This data includes all members of the YOTA branches, not just those in the experiment. We utilize attendance data for non-experimental participants in the out-of-sample predictions in Section 5.

3.7 Discussion of the design

Connection to theory

Our simple theoretical framework posits that individuals derive social recognition utility from attending the YMCA whenever such action is visible to others. Our method allows us to directly trace the utility individuals derive from social recognition for all possible actions that they may convey to others during Grow & Thrive. That is, the experiment provides a direct estimate of R_{exp}. Our approach therefore allows us to test whether, indeed, utility from social recognition is monotonically increasing in the number of visits to the YMCA, and whether social recognition generates both winners and losers (e.g., low types deriving disutility from having their actions revealed). As we show in our analysis of structural models, our approach also allows fairly direct inference about the curvature of S, the structural social recognition utility function.

[18] A transcript of the FAQ can be found in Online Appendix D.

[19] The YMCA contact reported that only one participant contacted him, asking whether he could be added to the SRP. After the (negative) response, there were no further questions from the participant.

[20] While YMCA members have to swipe-in to access the YMCA, they do not have to swipe-out to leave. Therefore we do not have information about how much time participants spent at the YMCA. To account for the risk of participants strategically swiping in-and-out without accessing YMCA programs and initiatives during their stay, YMCA employees were told to track any unusual activities among YMCA members. YMCA employees did not report any unusual pattern of access to the facilities during the experiment. Participants knew that multiple accesses during the same day would only count as one attendance.

What are individuals signaling?

Due to the nature of our setting and the wishes of the YMCA, we were not able to implement a treatment in which participants received social recognition without the Grow and Thrive incentive of raising \$2 per attendance for YOTA. As such, we cannot fully disentangle between whether YMCA members were motivated by the desire to be socially recognized for attending the YMCA, or for being charitable. However, this ambiguity is unlikely to matter for the broader implications of our findings, unless the nature of the motive has a deep interaction with the curvature of the social recognition utility function. We leave that for future empirical research.

Preference for signaling versus preferences for information

Note that the control group received no information about others' behavior. To the extent that individuals have an intrinsic preference for knowing about others' behavior, our design would therefore not isolate individuals' pure demand (or lack therefore) for social recognition alone. However, this would have to be a minor factor, because prior to making any decisions, all individuals in our experiment were told the average attendance of all YOTA members in the previous month. Moreover, in practice, the counterfactual to a social recognition scheme is not anonymized information provision—it is nothing at all. Our choice not to give any information to the control group thus better mirrors the reality of how such policies are implemented. Although in principle we could have included a second control group that did receive additional information, we chose not to do so because of statistical power considerations.

Using social recognition as commitment

To the extent that individuals attend the YMCA to exercise rather than to participate in some other more immediately pleasurable activity, and to the extent that they are (partially) sophisticated about their self-control problems, they may wish to motivate their future selves to attend the YMCA more. We argue that features of our design make this unlikely.

To begin, consider an alternative design in which individuals state a WTP for being part of the program as a whole, irrespective of their future actions.[21] In this case, individuals' WTP might reflect not only their social recognition utility, but also their desire to engage in more of the beneficial behavior their future self might undervalue. However when WTP is elicited for different possible levels of attendance, there is no direct sense in which WTP at a particular attendance level should incorporate individuals' desire for changing their future selves' behavior.

That said, there is a very nuanced method for creating a partial commitment device out of the attendance-contingent WTPs as well.[22] This entails individuals lowering expected payoffs for low attendance levels so as to discourage those low attendance levels. However, an individual can decrease an expected payoff for a low attendance level either by inflating or deflating their WTP

[21] See Allcott and Kessler (2019) for an example of such a design applied to social comparisons.

[22] It took the authors of this paper several weeks to figure it out. It remains an open question whether the participants in our study are far more sophisticated than us in building nuanced commitment devices.

for the social recognition treatment at that attendance level. Thus, the bias is unsigned, and it is reasonable to suppose that if individuals did engage in this behavior, then their deviations from truth-telling would not be systematic and simply average to zero.

However, we think it is simply psychologically unrealistic that individuals would try to manipulate their future behavior in such subtle and sophisticated ways. Moreover, Augenblick and Rabin (2019) find that individuals are almost completely naive about their future self-control problems, and that none try to utilize deviations from truth-telling (in an arguably much easier to utilize incentive structure) to create partial commitment devices.

4 Reduced-form results

We organize our results as follows. First, we calculate the changes in the attendance that result from randomization into the social recognition treatment. Second, we quantify participants' WTP for social recognition, which we then use to trace out the reduced-form social recognition function.

4.1 The experimental sample

A total of 428 YOTA members completed the survey and agreed to participate in Grow & Thrive. From this sample, we always exclude the 43 participants whose BDM choices determined their participation in the social recognition program, and whose treatment assignment therefore endogenous—we call the remainder the "full sample." Unless otherwise noted, from the remaining 385 participants we also exclude 46 participants who gave "incoherent" answers in our elicitation of WTP for social recognition. We define a participant to be incoherent if he/she "switches" from wanting to be socially recognized to not wanting to be socially recognized as the number of attendances increases.[23] We call these remaining participants the "coherent sample." This sample is most likely to consist of individuals who were taking our survey prompt seriously, but all of our results are robust to re-including these participants, as we show in Online Appendix H.

Table 1 shows that all pre-experiment outcomes as well as preferences elicited through our entry survey are balanced across YMCA members who participated in Grow & Thrive with or without social recognition.[24] In the thirteen months preceding the experiment, participants on average attended 6.67 times each month (for our two treatments with and without social recognition, respectively 6.47 and 6.87 ($p = 0.52$).[25] Overall, 73% of participants were female, and the average age was 44.

[23] While we believe these responses are due to lack of attention when filling out the survey, we do acknowledge the possibly that some individuals may have these preferences. For example, there could be modest individual(s) who do not want to be socially recognized far away from average attendance, but do not mind as much being observed closer to the mean population.

[24] Online Appendix Table H.1 shows a balance table for the full sample, including the incoherent individuals.

[25] Moreover, the pre-trends are very similar for both participants and non-participants, as seen in Online Appendix E.

4.2 The effect of social recognition on behavior

Figure 2 displays the cumulative distribution functions of attendance by treatment, showing that the impact of social recognition is positive across all levels of attendance. We quantify these results in Table 2.[26] Column (1) of Table 2 reports a simple OLS regression on whether individuals received the randomized social recognition treatment. The regression shows that social recognition increased attendance by 1.19 visits ($p = .105$), which constitutes a 17% precent increase from the 6.96 attendances in the control group. In column (2) we control for past attendance, which is highly correlated with treatment month attendance ($\rho = 0.78$, $p < 0.001$) and thus shrinks standard errors significantly. Controlling for past attendance increases our estimate to 1.57 attendances[27] ($p = 0.001$), which constitutes a 23% increase above control group attendance. Column (3) replicates column (2) while also controlling for individuals' beliefs about their attendance during the treatment month assuming that they would be part of the social recognition treatment.[28]

Finally, in columns (4) and (5) we explore treatment effect heterogeneity. Column (4) reports a regression that interacts the treatment effect of social recognition with past attendance. The regression shows that the impact of social recognition is increasing by 0.14 attendance per past attendance ($p = 0.067$) and that social recognition does not have much of an impact on members with zero prior attendances. Given the tight correlation between past attendance and treatment month attendance, this suggests that the treatment effect of social recognition is mostly multiplicative. That is, social recognition increases attendance by a constant multiple of what attendance would have been in the absence of social recognition. We formally estimate and test such a model in column (5) by instrumenting the interaction between treatment and current attendance with the interaction between treatment and past attendance. We estimate that the treatment effect of social recognition is 14% ($p = 0.048$) of what attendance would have been in the absence of social recognition. Overall, these results indicate that social recognition causally increased attendance at the YMCA by between 17-23%.

4.3 The reduced-form social recognition function

We now estimate the utility from social recognition by analyzing individuals' WTP to receive (or avoid) social recognition. Our approach permits within-subject identification by eliciting participants' WTP for social recognition conditional on all possible number of attendances. This directly and non-parametrically identifies what we called the reduced-form social recognition function R_{exp} in Section 2.

Figure 3 plots how WTP depends on the attendance level for the whole sample of coherent participants, and also for YMCA members whose attendance prior to the experiment is above or below median (5.1 visits per month). We plot the average WTP against the midpoint of the

[26] Table H.2 shows that the results are robust to using the full sample.

[27] Recall that the untreated group had slightly higher past attendance, and thus a slightly higher propensity to attend the YMCA during treatment month.

[28] The results are identical when controlling for individuals' beliefs about their attendance during the treatment month assuming that they would *not* be part of the social recognition treatment

attendance interval for which it was elicited.[29] The figure shows that WTP for social recognition is increasing in the number of visits to the YMCA, consistent with both of the models summarized in Section 2. Utility ranges from -\$1.70 for zero attendances to \$2.61 for twenty-three or more attendances. As is apparent, these results do not depend at all on whether individuals have above- or below-median attendance.

Figure 4 plots the ex-post utility from social recognition that each individual receives, together with a quadratic fit. If an individual attended a times during treatment month, and had a WTP for social recognition of $R(a)$, then we assign social recognition utility $R(a)$ to this individual. This figure looks much like Figure 3, which is unsurprising given that Figure 3 suggests that there is little heterogeneity in the social recognition function by one's propensity to attend the YMCA. A useful feature revealed by Figure 4, which we will utilize in our structural estimation, is that the reduced-form social recognition function is well approximated by a quadratic function.

We quantify the reduced-form social recognition function in Table 3. Columns (1) and (2) present OLS regressions of linear and quadratic fits, and show that WTP for social recognition is increasing and concave in the number of visits. Columns (3) and (4) replicate this analysis with Tobit regressions, which account for the fact that participants in our experiment could not express WTP below -\$8 or above \$8.

In Table 4 we examine the degree of heterogeneity in the reduced-form social recognition function, using both OLS and Tobit regressions with linear and quadratic terms for past attendance. As visually suggested by Figure 3, columns (1) and (2) show that there is little difference in the social recognition functions of those with above versus below median past attendance. Column (3) analyzes this further by interacting past attendance with the coefficients on attendance and attendance squared. The interactions are tightly estimated zeroes, while the coefficients on attendance and attendance squared remain largely unchanged compared to column (4) of Table 3. We find very similar results for the tobit (columns 4 to 6). This provides strong evidence that the social recognition function does not vary with individuals' propensity to attend the YMCA.[30] We use this fact in in our structural estimation of the impacts of applying our social recognition intervention to the whole YOTA population, which has a somewhat lower average attendance than the experimental population.[31]

Finally, we observe that the reduced-form social recognition function estimated here is consistent with the action-based model. The action-based model predicts that SR utility should equal zero at the population average, which was 3.16 attendances for the YOTA population during Grow &

[29] As noted in Section 3.3, the intervals of possible visits were the following: 0 visits, 1 visits, 2 visits, 3 visits, 4 visits, 5 or 6 visits, 7 or 8 visits, 9 to 12 visits, 13 to 17 visits, 18 to 22 visits, and 23 or more visits.

[30] To be clear, we mean the social recognition *function*—not the social recognition utility a person ends up obtaining ex-post at the end of the treatment month.

[31] In Online Appendix F we examine the relationship between social recognition functions and other demographics. Wald tests for the OLS and Tobit models suggest that there isn't much of an interaction ($p = 0.06$ and $p = 0.16$, respectively), although there is some evidence that women have a less concave social recognition function. Because women are somewhat over-represented in our experiment, this would imply that our out-of-sample estimates of deadweight loss would slightly underestimate it.

Thrive.[32] Figures 3 and 4 both show that social recognition payoffs are approximately zero at 3 attendances. Both the OLS and Tobit models estimated in Table 3 cannot reject that that social recognition is indeed zero at 3.16 attendances ($p = 0.45$ and $p = 0.98$, respectively).

5 Structural estimates of models and deadweight loss

Our results thus far provide us with a non-parametric estimate of the reduced form social recognition function R_{exp}, and provide strong evidence of concavity. This qualitative finding of concavity implies that social recognition will generate deadweight loss.

In this section, we build on this qualitative finding in two ways. First, we structurally estimate the underlying social recognition utility functions. Second, we use those estimates to quantify the degree of deadweight loss. In particular, we estimate the impact of applying our social recognition intervention to the whole population of the YMCA of the Triangle Area (YOTA).

Throughout, we make the natural assumption that individuals care about how they are seen relative to the broader YOTA population. For the action-based model, this implies the reference point is the average attendance of the YOTA population. This assumption is well supported by our reduced-form results that WTP at the average YOTA attendance is approximately zero.

For the type signaling model, note that individuals who are not socially recognized for their particular attendance level are not recognized for being part of the experiment, and thus an observer's expectation of their type can only be that it is the mean of the broader population. Thus the nature of the experimental design implies that an individual's WTP for social recognition must be the difference between the image utility of having their type revealed and the image utility of an observer simply believing that they are part of the broader population. This implies that behavior corresponding to that of the "average type" should generate the same social recognition payoffs as if nothing about that individual's behavior were observed.

Although natural, these assumptions complicate analysis because they imply that behavior in our experiment is partial equilibrium behavior. A successful social recognition intervention shifts the equilibrium, and thus the social recognition payoffs tied to any one particular action. For example, individuals who evaluate their behavior relative to the average would have a more demanding average to compare to, since a broadly applied social recognition intervention would increase everyone's behavior. Consequently, the function R_{exp} cannot be directly used to quantify what deadweight loss from social recognition would be in a full equilibrium. Our strategy is therefore to use the experimental data to estimate the structural social recognition function S, and use that to quantify the equilibrium effects of applying social recognition to the YOTA population.

The key assumption for our out-of-sample estimates to be unbiased is that conditional on past attendance, selection into our experiment is uncorrelated with social recognition utility functions. Concretely, the question we answer is thus this: given the distribution of past attendance of the full

[32] This was (unsurprisingly) very close to the prior average month's average of approximately 3 attendances, which we communicated to participants in our experiment.

YOTA population, but assuming that conditional on past attendance the experimental and the full population have the same utility functions, what would be the deadweight loss of applying social recognition to this full population. For the purpose of quantitatively interpreting our reduced-form results about concavity, we view an answer to this potentially more narrow question as informative as well. Even for this more modest out-of-sample exercise to be valid, however, we must still still rely on the assumption, strongly supported by Table 4, that social recognition utility functions do not vary by one's propensity to attend the YMCA.

5.1 Assumptions and parameters for model estimation

Material utility

We normalize the material utility function such that an individual's choice of attendance absent social recognition or other incentives is simply his type θ. We also assume that individuals with $\theta = 0$ choose $a = 0$ and are inelastic to both social recognition and financial incentives, but that the function is continuous in $\theta > 0$. The existence of a large mass of $\theta = 0$ types is motivated by the empirical distribution of attendance.[33] This normalization allows us to recover the distribution of types in the population non-parametrically, simply by observing the distribution of attendance in the broader YOTA population. These assumptions are satisfied by the functional form $u(a; \theta) = a - \frac{a^2}{2\theta}$.

Social recognition utility

Motivated by our reduced-form results and the quadratic fit in Figure 4, we assume that the reduced-form social-recognition function R_{exp} in the experiment is quadratic. In the action-based model, this assumption implies then S is given by $\beta_1(a - \bar{a}) - \frac{\beta_2}{2}(a - \bar{a})^2$,[34] and thus R_{exp} is given by

$$R_{exp}(a) = \gamma_1 a - \frac{\gamma_2}{2}a^2 - \left(\gamma_1 \bar{a}_0 - \frac{\gamma_2}{2}\bar{a}_0^2\right), \tag{3}$$

where $\bar{a}_0$ is the attendance of the whole YOTA population that month, and where $\gamma_2 = \beta_2$ and $\gamma_1 = \beta_1 + \gamma_2 \bar{a}_0$. Notably, R_{exp} has only two free parameters: γ_1 and γ_2.

To study the implications of the type-based model, recall the assumption that $S(\bar{\theta}, \bar{\theta}) = 0$, where $\bar{\theta}$ is the average of the broader YOTA population. Define $a^*_{exp}(\theta)$ as the optimal action choice of a type θ in the experiment. When combined with our normalization assumption that individuals choose $a = \theta$ in the absence of social recognition, this implies that R_{exp} must equal zero

[33] In the data, 39-51% of members attend 0 times on any given month. And conditional on not attending in the prior month and remaining a member, the likelihood of attending in the current month is only 19%.

[34] Since we assume that S is increasing, we technically have $S = \max(\bar{s}, \beta_1(a - \bar{a}) - \frac{\beta_2}{2}(a - \bar{a})^2)$, where $\bar{s} = \max_a \left(\beta_1(a - \bar{a}) - \frac{\beta_2}{2}(a - \bar{a})^2\right)$. This means that S is strictly increasing for all a satisfying $\beta_1 - \beta_2(a - \bar{a}) \geq 0$ and is flat thereafter. The estimates of β_1 and β_2 that we obtain imply that S is, in fact, strictly increasing for all possible attendance levels $a \leq 31$ both in the experiment and in the counterfactual equilibrium that would result if social recognition were applied to the whole population.

at $a^*(\bar{\theta}) = a^*(\bar{a}_0)$. Thus R_{exp} under social recognition utility must have the form

$$R_{exp}(a) = \gamma_1 a - \frac{\gamma_2}{2}a^2 - \left(\gamma_1 a^*(\bar{a}_0) - \frac{\gamma_2}{2}a^*(\bar{a}_0)^2\right). \tag{4}$$

Appendix B provides the derivations for the structural social recognition function $S(\theta; \bar{\theta})$ that generates the quadratic reduced-form social recognition function as above. There, we formally derive the separating equilibrium, and we show that S is concave and approximately quadratic in θ as well.

The parametric form of total utility

Our normalization assumptions on total utility $U = a - \frac{a^2}{2\theta} + R_{exp}(a)$ imply that R_{exp} is in units of "utils" rather than in units of money, as in WTP data. It will thus be convenient to write $R_{exp}(a) = \eta \tilde{R}_{exp}(a)$, where η is the marginal utility of money and $\tilde{R}_{exp}(a)$ is the social recognition utility in units of dollars. We will define $\tilde{R}(a) := \tilde{\gamma}_1 a - \frac{\tilde{\gamma}_2}{2}a^2 - \tilde{\gamma}_0$, so that $\gamma_1 = \eta\tilde{\gamma}_1$ and $\gamma_2 = \eta\tilde{\gamma}_2$. Intuitively, the parameters $\tilde{\gamma}_1$ and $\tilde{\gamma}_2$ are revealed by our data on WTP for social recognition at different levels of attendance, while the parameter η is revealed by our estimate of how much social recognition actually affects behavior, given our monetization of the social recognition utility function. To see the second point concretely, note that the attendance of a type θ receiving social recognition in the experiment is given by

$$a^*_{exp}(\theta) = \theta\frac{1 + \eta\tilde{\gamma}_1}{1 + \theta\eta\tilde{\gamma}_2} \tag{5}$$

$$\approx \theta(1 + \eta\tilde{\gamma}_1) \tag{6}$$

That is, social recognition increases attendance by a fraction of approximately $\eta\tilde{\gamma}_1$. The approximation in equation (6) holds when $\tilde{\gamma}_2$ is small relative to $\tilde{\gamma}_1$ (and when θ is not too large), which we will estimate to be the case. This approximation coincides with our empirical results that the effects of social recognition are approximately a constant fraction of what attendance would be in the absence of social recognition.

The distribution of types

The distribution of types θ in the YOTA population is revealed non-parametrically by the distribution of their attendance. This follows directly from our normalization assumptions on the material utility function.

5.2 Moment conditions for model parameters

The key parameters we must estimate are $\tilde{\gamma}_1$, $\tilde{\gamma}_2$ and η. We do this via generalized method of moments (GMM). Equations (3) and (4) for the action-based and type-based models, respectively, lead to three moment conditions of the form

$$E\left[\tilde{R}_{exp}(a_i)\cdot a_i^k\right]=0$$

for $k=0,1,2$. Given η and $\bar{a}_0$ these moments identify $\tilde{\gamma}_1$ and $\tilde{\gamma}_2$, analogous to the regressions in Table 3.

Next, equation (5) implies the moment condition

$$E\left[\frac{a_i+a_i\eta\tilde{\gamma}_1}{1+a_i\eta\tilde{\gamma}_2}-a_i|i\in\text{control}\right]=\tau_{SR}$$

for individuals i in the control group, where τ_{SR} is the average treatment effect of social recognition. Given τ_{SR} and $\tilde{\gamma}_1$ and $\tilde{\gamma}_2$, this moment identifies η. To obtain τ_{SR} with, we set-up moment conditions corresponding to the regression in column (2) of Table 2, which controls past behavior a_i^{past}. These three moment conditions are simply

$$\begin{aligned}
E\left[a_i-\tau_{SR}-b\cdot a_i^{past}-\bar{a}_{0,exp}\right]&=0\\
E\left[\left(a_i-\tau_{SR}-b\cdot a_i^{past}-\bar{a}_{0,exp}\right)a_i^{past}\right]&=0\\
E\left[\left(a_i-\tau_{SR}-b\cdot a_i^{past}-\bar{a}_{0,exp}\right)\mathbf{1}_{SR}\right]&=0
\end{aligned}$$

where $\mathbf{1}_{SR}$ is an indicator for being randomized into the social recognition treatment and $\bar{a}_{0,exp}$ is the average attendance of those in the experimental population not treated with social recognition.

Together, this yields seven moment conditions for a vector of six parameters: $\tilde{\gamma}_1,\tilde{\gamma}_2,\eta,\tau_{SR},b$ and $\bar{a}_{0,exp}$. The overidentification is a consequence of the restrictions that both the action-based and type-based models impose on the constant term in equations (3) and (4), which restrict it to be a function of $\tilde{\gamma}_1$ and $\tilde{\gamma}_2$.

Letting $\xi:=(\tilde{\gamma}_1,\tilde{\gamma}_2,\eta,\tau_{SR},b,\bar{a}_{0,exp})$ denote the parameters, the GMM estimator chooses the parameters $\hat{\xi}$ that minimize $\left(m(\xi)-m(\hat{\xi})\right)^{\prime}W\left(m(\xi)-m(\hat{\xi})\right)$, where $m(\xi)$ are the theoretical moments, $m(\hat{\xi})$ are the empirical moments, and W is the optimal weighting matrix given by the inverse of the variance-covariance matrix of the moment conditions. We approximate W using a two-step estimator outlined in Hall (2005). In the first step, we set W equal to the identity matrix,[35] and use this to solve the moment conditions for $\hat{\xi}$, which we denote $\hat{\xi}_1$. Since $\hat{\xi}_1$ is consistent, by Slutsky's theorem the sample residuals $\hat{u}$ will also be consistent. We then use these residuals to estimate the variance-covariance matrix of the moment conditions, S, given by $Cov(\mathbf{z}u)$, where $\mathbf{z}$ are the instruments for the moment conditions. We then minimize $\left(m(\xi)-m(\hat{\xi})\right)^{\prime}\hat{W}\left(m(\xi)-m(\hat{\xi})\right)$ using $\hat{W}=\hat{S}^{-1}$, which gives the optimal $\hat{\xi}$ (Hansen, 1982).

[35]One other common approach is to use $(\mathbf{z}\mathbf{z}')^{-1}$as the weighting matrix in the first-stage, where $\mathbf{z}$ is a vector of the instruments in the moment equations. We confirmed our standard errors and point estimates are the same under both choices.

5.3 Equilibrium predictions

The parameters $\gamma_1 = \eta\tilde{\gamma}_1$ and $\gamma_2 = \eta\tilde{\gamma}_2$ allow us to compute the equilibrium effects of applying social recognition to the full population. The action-based and type-based models, however, impose different requirements for inferring equilibrium behavior and deadweight loss from R_{exp}, which we work through below.

Action-based model

The structural action-based model is $S(a;\bar{a}) = \beta_1(a-\bar{a}) - \frac{\beta_2}{2}(a-\bar{a})^2$, where the β_i are obtained from γ_i through the identities $\beta_2 = \gamma_2$ and $\beta_1 = \gamma_1 - \gamma_2\bar{a}_0$. To derive the equilibrium consequences of applying social recognition to the whole YOTA population we must therefore compute what $\bar{a}$, average attendance, would be in equilibrium. Applying social recognition to the whole population raises attendance and thus $\bar{a}$. This raises the standard for "good" attendance, which in turn increases the marginal social recognition payoffs from increasing a. Indeed, $\frac{\partial}{\partial a}S = \beta_1 + \beta_2(\bar{a} - a)$, which is increasing in $\bar{a}$.

To solve for equilibrium $\bar{a}$, we note that a type θ's optimal choice of action given $\bar{a}$ is $a^*(\theta) = \theta\frac{1+\beta_1+\beta_2\bar{a}}{1+\theta\beta_2}$. Equilibrium average attendance must therefore satisfy

$$\bar{a} = E\left[\theta\frac{1+\beta_1}{1+\theta\beta_2}\right] + \bar{a}E\left[\frac{\theta\beta_2}{1+\theta\beta_2}\right]. \tag{7}$$

Because equation (7) is linear in $\bar{a}$, it has a unique solution, and thus the equilibrium of the action-based model must be unique. Equation (7) implies that equilibrium average behavior is given by

$$\bar{a}_E = \frac{(1+\beta_1)E\left[\frac{\theta}{1+\theta\beta_2}\right]}{1-\beta_2E\left[\frac{\theta}{1+\theta\beta_2}\right]}$$

Type-based model

In contrast to the action-based model, the type-based model implies that $R_{exp} = R_{pop}$. That is, the reduced-form social recognition utility function we estimate for the experimental population would be the same one that applies in an equilibrium in which social recognition is scaled up to apply to everyone in the population. This is for two reasons. First, both the experimental population and the broader population are assumed to care about how they compare to the average type $\bar{\theta}$ in the population. Unlike equilibrium average attendance $\bar{a}$, $\bar{\theta}$ is a primitive of the model that does not change.

Second, because we have an (approximately) continuous strategy space, the equilibrium in our model is a separating equilibrium, in which each type θ's optimal choice of action depends on the structural social recognition function S and on $\bar{\theta}$, but not on any other moments of the distribution of θ (see Appendix B). This implies that even though the types θ that are in the experiment are not representative of those in the population, the equilibrium choice of action of any give type θ will be the same.

The property that a type θ's choice of action is independent of the distribution of types (beyond $\bar{\theta}$) generally holds for any signaling model with a continuous action space and a utility function that satisfies the "single-crossing" property (Mailath, 1987). When the action space is not continuous, however, this property generally does *not* hold for signaling models; see, e.g., Bénabou and Tirole (2006) for insightful implications of this for the case of binary actions. Our continuous action space is thus crucial not only for making direct inferences about the shape of the social recognition function, but also for making valid out-of-sample predictions for type-based signaling models.

Deadweight loss

We compute un-normalized deadweight loss as $E[\tilde{R}_{pop}(a_i^*)]$, where a_i^* is the action chosen by individuals in equilibrium when social recognition is applied to the whole YOTA population. We also compute the equivalent price metric by computing for each individual i, the reward per attendance p_i that would induce them to choose a_i^* in the absence of social recognition. The first order condition for the optimal action in the absence of social recognition is $a_i = \theta_i(1+p_i)$ in the absence of social recognition.[36] Since by definition this must equal individuals, attendance in the presence of of social recognition, we have

$$1 + p_i = \frac{a_i^{SR}}{\theta_i} = \frac{1+\eta\tilde{\gamma}_1}{1+\theta\eta\tilde{\gamma}_2} \tag{8}$$

where a_i^{SR} is attendance in the presence of social recognition (but no piece-rate incentives).[37]

5.4 Results

Table 5 presents our estimates. Panel 5a presents the estimates of the main parameters $\tilde{\gamma}_1$, $\tilde{\gamma}_2$ and η. For the action-based social recognition function, the parameter estimates imply $S = 0.45(a-\bar{a}) - 0.01(a-\bar{a})^2$.[38] For the type-based social recognition function, the results in ppendix B give us the approximation $S(\theta,\bar{\theta}) \approx 0.47(\theta-\bar{\theta}) - 0.02(\theta-\bar{\theta})^2$.[39] All standard errors are clustered at

[36] Specifically, let $p_i(\theta)$ denote the per-attendance price given to consumer i that lets them achieve the same level of attendance a^* as in social recognition. Then (suppressing θ for convenience) each individual's utility is given by $U(\theta) = a - \frac{1}{2\theta}a^2 + pa$. We can then use this to explicitly solve for p as a function of a^* (and hence of θ). By taking the FOC with respect to a:

$$0 = 1 - \frac{1}{\theta}a + p$$
$$p = \frac{a}{\theta} - 1$$

So to induce attendance a^*, an individual would need to be offered a price p such that $p = \frac{a^*}{\theta} - 1 = \frac{a^*}{a} - 1$

[37] We exclude individuals with zero attendance from the average price metric, as this metric is undefined for them.

[38] From section 5.3, we have $S(a;\bar{a}) = \beta_1(a-\bar{a}) - \frac{\beta_2}{2}(a-\bar{a})^2$. Using $\beta_1 = \eta\tilde{\gamma}_1 - \eta\tilde{\gamma}_2\bar{a}_0$ and $\beta_2 = \eta\tilde{\gamma}_2$, we can rewrite this as $S(a;\bar{a}) = (\eta\tilde{\gamma}_1 - \eta\tilde{\gamma}_2\bar{a}_0)(a-\bar{a}) - \frac{\eta\tilde{\gamma}_2}{2}(a-\bar{a})^2$. We then substitute $\bar{a}_0 = 3.16$ and the estimates from Table 5a, $\eta = 1.32$, $\tilde{\gamma}_1 = 0.41$, and $\tilde{\gamma}_2 = 0.02$, to obtain the result.

[39] From equation 9, we have $S(\theta;\bar{\theta}) \approx \left(\gamma_1(1+\gamma_1) - \bar{\theta}\gamma_2(1+\gamma_1)^2\right)(\theta-\bar{\theta}) - \frac{\gamma_2(1+\gamma_1)^2}{2}(\theta-\bar{\theta})^2$. The results in Appendix B imply $\bar{\theta}(1+\gamma_1) = \bar{a}_0$. Substituting this and $\gamma_1 = \eta\tilde{\gamma}_1$, $\gamma_2 = \eta\tilde{\gamma}_2$ into S yields $S(\theta;\bar{\theta}) \approx$

the subject level, and are based on the variance-convariance matrix computed using the efficient two-step GMM estimator described in Section 5.2.

Panel 5b utilizes the parameter estimates from panel 5a to present estimates of deadweight loss, behavior change, the equivalent price metric, and the normalized deadweight loss values. Because the statistics in panel 5a are highly nonlinear functions of the parameters in panel 5a, we compute 95% confidence intervals without relying on asymptotic normality approximations. Instead, we compute our confidence intervals using percentile-based bootstrap blocked at the individual level. In Appendix G we show that our bootstrapped confidence intervals are nearly identical to those computed analytically from asymptotic normality approximations for all but the normalized deadweight loss estimates. Using bootstrap simulations, we show that this is because the distributions of the normalized deadweight loss estimates are not well approximated by normal distributions, which implies that the first-order approximation utilized by the delta method has to perform poorly.

Column (1) in Table 5b presents the un-normalized deadweight loss estimates, while column (2) presents the change in attendance that would result if the attendance of everyone in YOTA were socially recognized. The estimates in column (2) imply a 19-23% increase in attendance, which closely matches our reduced-form estimate of a 23% increase on the experimental population, despite the two populations having different baseline attendance. The approximation in equation (6), which shows that social recognition has an approximately proportional effect, clarifies why our two estimates of percent increases are so similar.

Column (3) shows that the deadweight loss per extra attendance is 0.45 (CI 0.27-0.89) and 0.58 (CI 0.23-1.31) for the action-based and type-based models. Column (4) shows that on average, the effects of social recognition are equal to piece-rate incentives of \$0.36 and \$0.27 in the action-based and type-based models. Column (5) combines these estimates to produce deadweight loss per dollar of behaviorally-equivalent incentives. We find this to be \$1.23 for the action-based model and \$2.15 for the type-based model. On the whole, these results are consistent with significant concavity in the social recognition utility function, which produces significant deadweight loss.

These results also underscore the importance of working through the microfoundations of social recognition utility. While both models produce estimates in the same ballpark, they are still meaningfully different. One reason for this is that the action-based model predicts more behavior change because of equilibrium adjustment that does not occur in the type-based model.

6 Misprediction of own behavior and its implications

A key and innovative feature of our design is that our elicitation of people's WTP for social recognition does not require them to form beliefs about their future attendance. This design feature was motivated by growing evidence that individuals may hold incorrect beliefs about their future behavior (e.g., DellaVigna and Malmendier, 2006; Grubb, 2009, 2015) In Figure 5, we assess the

$(\eta\tilde{\gamma}_1(1+\eta\tilde{\gamma}_1) - \bar{a}_0\eta\tilde{\gamma}_2(1+\eta\tilde{\gamma}_1))(\theta-\bar{\theta}) - \frac{\eta\tilde{\gamma}_2(1+\eta\tilde{\gamma}_1)^2}{2}(\theta-\bar{\theta})^2$. We then substitute $\bar{a}_0 = 3.16$ and the estimates from Table 5a, $\eta = 1.11$, $\tilde{\gamma}_1 = 0.35$, and $\tilde{\gamma}_2 = 0.02$, to obtain the result.

accuracy of individuals' beliefs. We find that on average, individuals overestimate their attendance by 35% ($p < .05$). Such misprediction has several important implications, which we discuss below.

6.1 Implications for experimental design

First, the possibility of misprediction implies that "strategy method" designs such as ours are crucial for avoiding biases in individuals' valuations of non-price levers. Consider the alternative design strategy of simply eliciting WTP for being part of the social recognition program, without conditioning on attendance. Because social recognition utility is monotonic in attendance, overestimation of one's future attendance would then lead one to overestimate the expected social recognition utility one would derive. An analyst taking those WTPs at face value might then conclude that social recognition is in fact positive-sum, rather than negative-sum as we find.

Such strategy-method designs are crucial beyond work evaluating social recognition schemes. For example, applying our design strategy to an experiment such as that of Allcott and Kessler (2019) would allow for estimates of the utility effect of conserving more or less energy than one's neighbors, without having to assume that people can accurately forecast their future energy use.

6.2 Implications for market structure

Incorrect beliefs about future behavior may lead individuals to overestimate the utility they may obtain from a social recognition scheme. This overestimation can significantly effect market structure. As we discuss in the introduction, social recognition programs are frequently used by businesses to incentivize high performance among their employees . However, individuals' evaluations of workplaces with particularly aggressive social recognition programs would be upwardly biased if individuals overestimate their likelihood of being recognized for good performance. In line with the reasoning of behavioral IO models studying the implications of misprediction of behavior (DellaVigna and Malmendier, 2004), and of shrouded attributes (Gabaix and Laibson, 2006; Heidhues et al., 2016), such overestimation can lead firms to use social recognition programs even when they are negative-sum. This would result in inefficient incentive structures.

7 Limitations, robustness, and conclusions

A recent and growing literature establishes that public recognition of behavior can have large effects in a number of economically consequential field settings. We build on this literature by developing a framework for quantifying the social efficiency of using social recognition for behavior change. We show theoretically that a simple indication of whether social recognition programs are more socially efficient than traditional financial incentives is the shape of the social recognition utility function—if it is concave (convex), then social recognition is negative-sum (positive-sum). Although it is often assumed that one's social recognition utility is linear in the audience's inferences about one's type (e.g., Bénabou and Tirole 2006, 2011; Ali and Bénabou 2016), we show that this assumption is far from innocuous for questions about the efficiency of social recognition interventions. We then develop experimental techniques that permit robust and direct elicitation of the social recognition utility function for individuals in the experiment, including a direct estimate of the curvature of this function. And we develop structural methods that allow us to analyze—taking into account the equilibrium effects—the behavioral and social efficiency consequence of scaling up the social recognition intervention beyond the experimental sample.

We implement our approach with the YMCA of the Triangle Area. We estimate that publicizing the attendance of all members of these branches would increase attendance by about 20%, but would generate deadweight loss of \$1.23-\$2.15 per dollar of behaviorally-equivalent financial incentives. This suggests that social recognition may not compare favorably to financial incentives as a means of creating behavior change. Beyond the immediate policy implications, this finding also suggests that, because of individuals' overly optimistic beliefs, a competitive market could overuse social recognition schemes.

That said, a number of caveats are in order.

First, our deadweight loss quantification should not be interpreted as a holistic assessment of the welfare effects. Even if there is significant deadweight loss, the social benefits from behavior change may be large, and thus a social recognition intervention may yield large and positive welfare effects compared to doing nothing. Rather, our research question is about the welfare effects of social recognition, *holding constant the benefits from behavior change.* Answering such a question for an arsenal of different possible levers for behavior change provides a direct comparison of which tools are most efficient in bringing about the desired behavior change.

Second, while our deadweight loss concept builds on classical tools and concepts in public finance, practical applications of our framework may be tempered by political economy constraints. In some settings, it may be more politically acceptable to rely on non-price interventions such as social recognition rather than on financial incentives. And because, as we note above, the welfare effects of behavior change may be large, social recognition may be an attractive second-best solution. But even in such settings, our framework helps provide an answer to how socially costly those political economy constraints are, and how important it may be to try to relax them.

Relatedly, the set of possible financial incentive schemes may be limited. We have shown that social recognition provides the highest marginal incentives for the lowest types by virtue of being a concave function. To the extent that targeting such low types is desirable, social recognition may have an attractive targeting property over financial incentives if political economy or practical considerations limit to linear incentive schemes. In this case our framework would be used to weight the benefits of targeting against the efficiency costs of relying on a negative-sum lever.

Third, we emphasize that our results constitute only an initial data point on the efficiency of social recognition levers. We conducted our study on a selected sample that is not fully representative of the U.S. population, and thus our broad conclusions about the deadweight loss of social recognition must be interpreted with caution. To the extent that curvature of the social recognition utility function may vary meaningfully over the population, our quantitative estimates may not hold in other contexts. Whether it is plausible that *curvature* may vary substantially across different populations is a question for future research. Variation in the *sensitivity* to social recognition poses a smaller problem, particularly for our normalized deadweight estimates. Since sensitivity scales both the change in behavior and the WTP to be socially recognized, variation in sensitivity is not likely to have a big effect on our conclusions about normalized deadweight loss.

Fourth, the core reasons for why individuals derive utility from social recognition matter. We believe that our experiment mostly channels an intrinsic desire to appear "good," as do many other settings. In some contexts, however, the information revealed about individuals directly affects their material payoffs. Plainly, our results about the intrinsic motives are not informative about the shape of utility from extrinsic motives in such settings.

Fifth, our quantitative estimates cannot be directly applied to social recognition schemes that produce different information structures; e.g., ones that recognize only if people are at the top or not. However, as we show in Section 2, any information structure will generate deadweight loss if the structural social recognition function is concave, and social efficiency gains if it is convex. Thus, all else equal, different information structures would not change our qualitative result, and would probably not have a big affect the normalized deadweight loss estimates.

Finally, despite a novel feature of our design that eliminates the need to (incorrectly) assume rational expectations of future behavior, our elicitation method is not immune from all possible forecasting biases. In particular, individuals may misforecast the intensity of feelings of pride or shame, conditional on a particular realization of behavior. Testing this possibility would require a very data-intensive design that elicits peoples WTP for social recognition after their attendance is realized. Such an approach would require an order of magnitude more data than ours, and is thus left for future research.

Despite the open questions and the weaknesses of our approach that we hope future work will evaluate and address, our approach nevertheless provides a tractable toolkit for evaluating non-price levers. Although non-price interventions such as "nudges" have become popular tools among governments around the world (Thaler and Sunstein, 2008; OECD, 2017), there is little rigorous economic analysis of the actual welfare effects of such interventions (Bernheim and Taubinsky, 2018). Extending the core frameworks of public economics to consider non-price interventions is crucial for robust and holistic welfare analysis.

References

Al-Ubaydli, Omar, John A List, Danielle LoRe, and Dana Suskind, "Scaling for Economists: Lessons from the non-adherence problem in the medical literature," *Journal of Economic Perspectives*, 2017, *31* (4), 125–44. 2.3

Ali, S. Nageeb and Roland Bénabou, "Image versus information: Changing societal norms and optimal privacy," Working Paper 22203, National Bureau of Economic Research 2016. 1, 2.1.1, 2.1.2, 2.2, 7

Allcott, Hunt and Judd B Kessler, "The welfare effects of nudges: A case study of energy use social comparisons," *American Economic Journal: Applied Economics*, 2019. Forthcoming. 1, 4, 11, 21, 6.1

Andreoni, James and B. Douglas Bernheim, "Social image and the 50–50 norm: A theoretical and experimental analysis of audience effects," *Econometrica*, 2009, *77* (5), 1607–1636. 1, 6, 2.1.2

Angrist, Joshua D and Jorn-Steffen Pischke, *Mostly harmless econometrics: An empiricist's companion*, Princeton University Press, 2008. G

Ariely, Dan, Anat Bracha, and Stephan Meier, "Doing good or doing well? Image motivation and monetary incentives in behaving prosocially," *American Economic Review*, 2009, *99* (1), 544–555. 5, 12

Ashraf, Nava, Oriana Bandiera, and B Kelsey Jack, "No margin, no mission? A field experiment on incentives for public service delivery," *Journal of Public Economics*, 2014, *120*, 1–17. 1, 12

Auerbach, Alan J., *The theory of excess burden and optimal taxation*, Elsevier, 1985. 2.2, A

Augenblick, Ned and Matthew Rabin, "An experiment on time preference and misprediction in unpleasant tasks," *Review of Economic Studies*, 2019. Forthcoming. 3.7

Banerjee, Abhijit, Rukmini Banerji, James Berry, Esther Duflo, Harini Kannan, Shobhini Mukerji, Marc Shotland, and Michael Walton, "From proof of concept to scalable policies: Challenges and solutions, with an application," *Journal of Economic Perspectives*, 2017, *31* (4), 73–102. 2.3

Barankay, Iwan, "Rankings and social tournaments: Evidence from a crowd-sourcing experiment," *Wharton School of Business, University of Pennsylvania Working Paper*, 2011, pp. 1–38. 1

Becker, Gary S., "A Note on Restaurant Pricing and Other Examples of Social Influences on Price," *Journal of Political Economy*, 1991, *99* (5), 1109–1116. 1, 2.1.1

Bénabou, Roland and Jean Tirole, "Incentives and Prosocial Behavior," *American Economic Review*, 2006, *96* (5), 1652–1678. 1, 5, 6, 2.1.2, 5.3, 7

_ and _ , "Laws and Norms," Working Paper 17579, National Bureau of Economic Research 2011. 1, 2.2, 7

Bernheim, B. Douglas and Dmitry Taubinsky, *Behavioral public economics*, Vol. 1, Elsevier, 2018. 4, 7

_ , Andrey Fradkin, and Igor Popov, "The welfare economics of default options in 401 (k) plans," *American Economic Review*, 2015, *105* (9), 2798–2837. 1

Besley, Timothy and Stephen Coate, "Workfare versus welfare incentive arguments for work requirements in poverty-alleviation programs," *American Economic Review*, 1992, *82* (1), 249–61. 1, 2.1.1

Blomquist, N. Soren, "Interdependent behavior and the effect of taxes," *Journal of Public Economics*, 1993, *51* (2), 211–218. 1, 2.1.1

Bloom, Nicholas and John Van Reenen, "Measuring and explaining management practices across firms and countries," *Quarterly Journal of Economics*, 2007, *122* (4), 1351—1408. 1

Bø, Erlend E, Joel Slemrod, and Thor O Thoresen, "Taxes on the internet: Deterrence effects of public disclosure," *American Economic Journal: Economic Policy*, 2015, *7* (1), 36–62. 1

Bradler, Christiane, Robert Dur, Susanne Neckermann, and Arjan Non, "Employee recognition and performance: A field experiment," *Management Science*, 2016, *62* (11), 3085–3099. 1

Bursztyn, Leonardo and Robert Jensen, "How Does Peer Pressure Affect Educational Investments?," *Quarterly Journal of Economics*, 2015, *130* (3), 1329–1367. 1

_ and _ , "Social Image and Economic Behavior in the Field: Identifying, Understanding, and Shaping Social Pressure," *Annual Review of Economics*, 2017, *9* (1), 131–153. 1

— , **Bruno Ferman, Stefano Fiorin, Martin Kanz, and Gautam Rao**, "Status goods: experimental evidence from platinum credit cards," *Quarterly Journal of Economics*, 2017, *133*, 1561–1595. 1

— , **Georgy Egorov, and Robert Jensen**, "Cool to be Smart or Smart to be Cool? Understanding Peer Pressure in Education," *Review of Economic Studies*, 2019. Forthcoming. 1, 5

— , **Thomas Fujiwara, and Amanda Pallais**, "'Acting Wife': Marriage Market Incentives and Labor Market Investments," *American Economic Review*, 2017, *107* (11), 3288–3319. 1

Carroll, Gabriel D, James J Choi, David Laibson, Brigitte C Madrian, and Andrew Metrick, "Optimal defaults and active decisions," *Quarterly Journal of Economics*, 2009, *124* (4), 1639–1674. 1

Damgaard, Mette Trier and Christina Gravert, "The hidden costs of nudging: Experimental evidence from reminders in fundraising," *Journal of Public Economics*, 2018, *157*, 15–26. 1

Davis, Jonathan, Jonathan Guryan, Kelly Hallberg, and Jens Ludwig, "The economics of scale-up," Technical Report, National Bureau of Economic Research 2017. 2.3

DellaVigna, Stefano, "Structual Behavioral Economics," in B. Douglas Bernheim, Stefano DellaVigna, and David Laibson, eds., *Handbook of Behavioral Economics*, number 1, Elsevier, 2018. 1

— **and Ulrike Malmendier**, "Contract design and self-control: Theory and evidence," *Quarterly Journal of Economics*, 2004, *119* (2), 353–402. 6.2

— **and** — , "Paying not to go to the gym," *American Economic Review*, 2006, *96* (3), 694–719. 6

— , **John A List, and Ulrike Malmendier**, "Testing for altruism and social pressure in charitable giving," *Quarterly Journal of Economics*, 2012, *127* (1), 1–56. 1, 6

— , — , — , **and Gautam Rao**, "Voting to tell others," *The Review of Economic Studies*, 2017, *84* (1), 143–181. 1, 6, 11

Duflo, Esther, Rachel Glennerster, and Michael Kremer, "Using randomization in development economics research: A toolkit," *Handbook of development economics*, 2007, *4*, 3895–3962. 2.3

Exley, Christine, "Incentives for Prosocial Behavior: The Role of Reputations,," *Management Science*, 2018, *64* (5), 2460–2471. 5

Gabaix, Xavier and David Laibson, "Shrouded attributes, consumer myopia, and information suppression in competitive markets," *Quarterly Journal of Economics*, 2006, *121* (2), 505–540. 6.2

Gauri, Varun, Julian C Jamison, Nina Mazar, Owen Ozier, Shomikho Raha, and Karima Saleh, "Motivating bureaucrats through social recognition: evidence from simultaneous field experiments," 2018. 1

Gerber, Alan S, Donald P Green, and Christopher W Larimer, "Social pressure and voter turnout: Evidence from a large-scale field experiment," *American Political Science Review*, 2008, *102* (1), 33–48. 1

Gosnell, Greer K, John A List, and Robert Metcalfe, "A new approach to an age-old problem: solving externalities by incenting workers directly," Technical Report, National Bureau of Economic Research 2016. 12

Grubb, Michael D., "Selling to overconfident consumers," *American Economic Review*, 2009, *99* (5), 1770–1807. 6

— , "Overconfident consumers in the marketplace," *Journal of Economic Perspectives*, 2015, *29* (4), 9–36. 6

Hall, Alistair R., "Generalized Method of Moments," 2005. 5.2

Hansen, Lars Peter, "Large sample properties of Generalized Method of Moments estimators," *Econometrica*, 1982, *50* (4), 1029–1054. 5.2

Heidhues, Paul, Botond Kőszegi, and Takeshi Murooka, "Exploitative Innovation," *American Economic Journal: Microeconomics*, 2016, *8* (1), 1–23. 6.2

Imas, Alex, "Working for the warm glow: On the benefits and limits of prosocial incentives," *Journal of Public Economics*, 2014, *114*, 14–18. 12

Karing, Anne, "Social signaling and childhood immunization: A field experiment in Sierra Leone," Working Paper 2019. 1, 5

Karlan, Dean and Margaret A McConnell, "Hey look at me: The effect of giving circles on giving," *Journal of Economic Behavior & Organization*, 2014, *106*, 402–412. 1

Kosfeld, Michael and Susanne Neckermann, "Getting more work for nothing? Symbolic awards and worker performance," *American Economic Journal: Microeconomics*, 2011, *3* (3), 86–99. 1

Lindbeck, Assar, Sten Nyberg, and Jorgen Weibull, "Social Norms And Economic Incentives In The Welfare State," *Quarterly Journal of Economics*, 1999, *114*, 1–35. 1, 2.1.1

_, _, and Jörgen Weibull, "Social Norms and Welfare State Dynamics," *Journal of the European Economic Association*, 2003, pp. 533–542. 2.1.1

Loewenstein, George, Cass R Sunstein, and Russell Golman, "Disclosure: Psychology changes everything," 2014, *6*, 391–419. 1

Mailath, George J, "Incentive compatibility in signaling games with a continuum of types," *Econometrica: Journal of the Econometric Society*, 1987, pp. 1349–1365. 5.3, B

OECD, *Behavioural Insights and Public Policy: Lessons from Around the World*, OECD Publishing, 2017. 7

Perez-Truglia, Ricardo and Guillermo Cruces, "Partisan interactions: Evidence from a field experiment in the united states," *Journal of Political Economy*, 2017, *125* (4), 1208–1243. 1

_ and Ugo Troiano, "Shaming Tax Delinquents: Theory and Evidence from a Field Experiment in the United States," *Journal of Public Economics*, 2018, *167*, 120–137. 1

Thaler, Richard H and Cass R Sunstein, "Nudge: Improving decisions about health, wealth, and happiness," 2008. 7

Thunstrom, Linda, "Welfare effects of nudges: The emotional tax of calorie menu labeling," *Judgment and Decision Making*, 2019, *14* (1), 11–25. 1

WorldatWork, "Trends in Employee Recognition," 2017. 1

Yoeli, Erez, Moshe Hoffman, David G Rand, and Martin A Nowak, "Powering up with indirect reciprocity in a large-scale field experiment," *Proceedings of the National Academy of Sciences*, 2013, *110* (Supplement 2), 10424–10429. 1

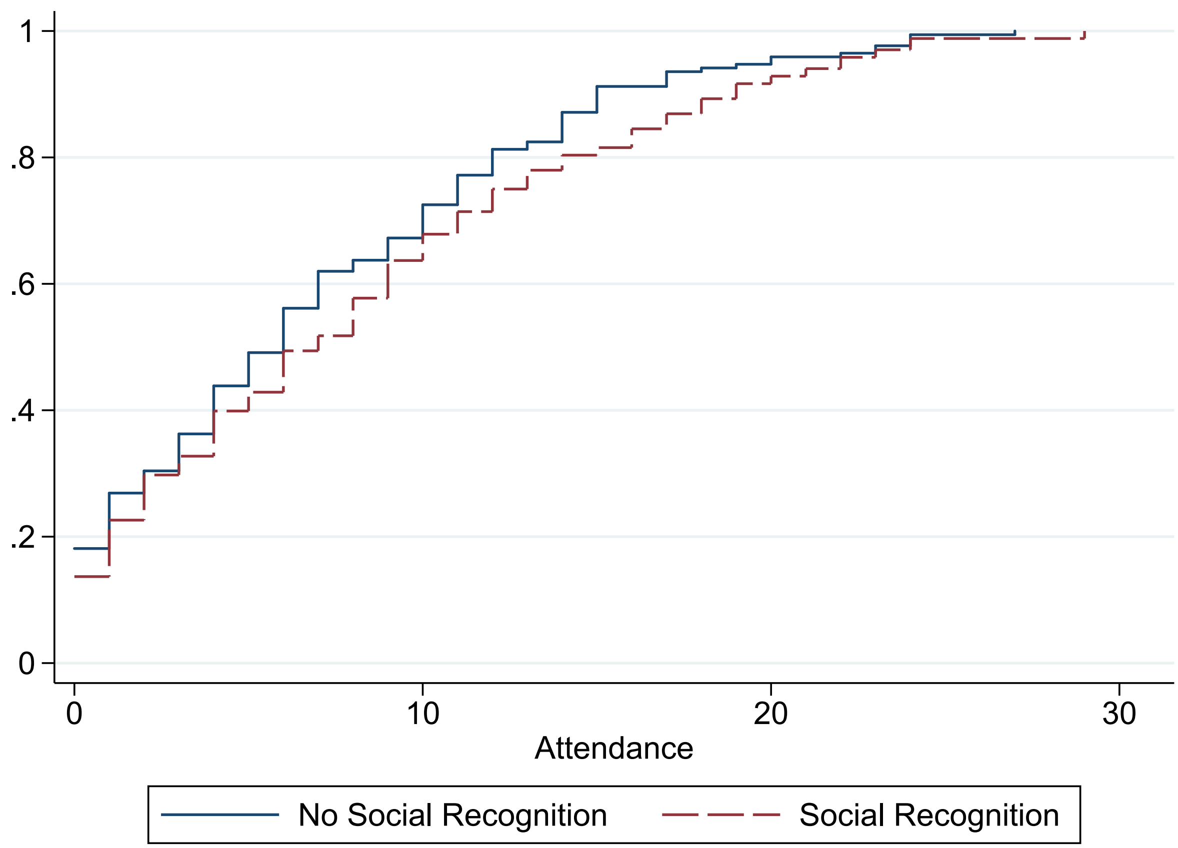
Figures and Tables

Figure 1: Illustration of social recognition information

Thank you for joining Grow & Thrive from your friends at YMCA!		
	# of visits	Dollars Raised
1. John Doe	25	$50
2. Mary Adams	24	$48
..		
49. Jack Black	10	$20
..		

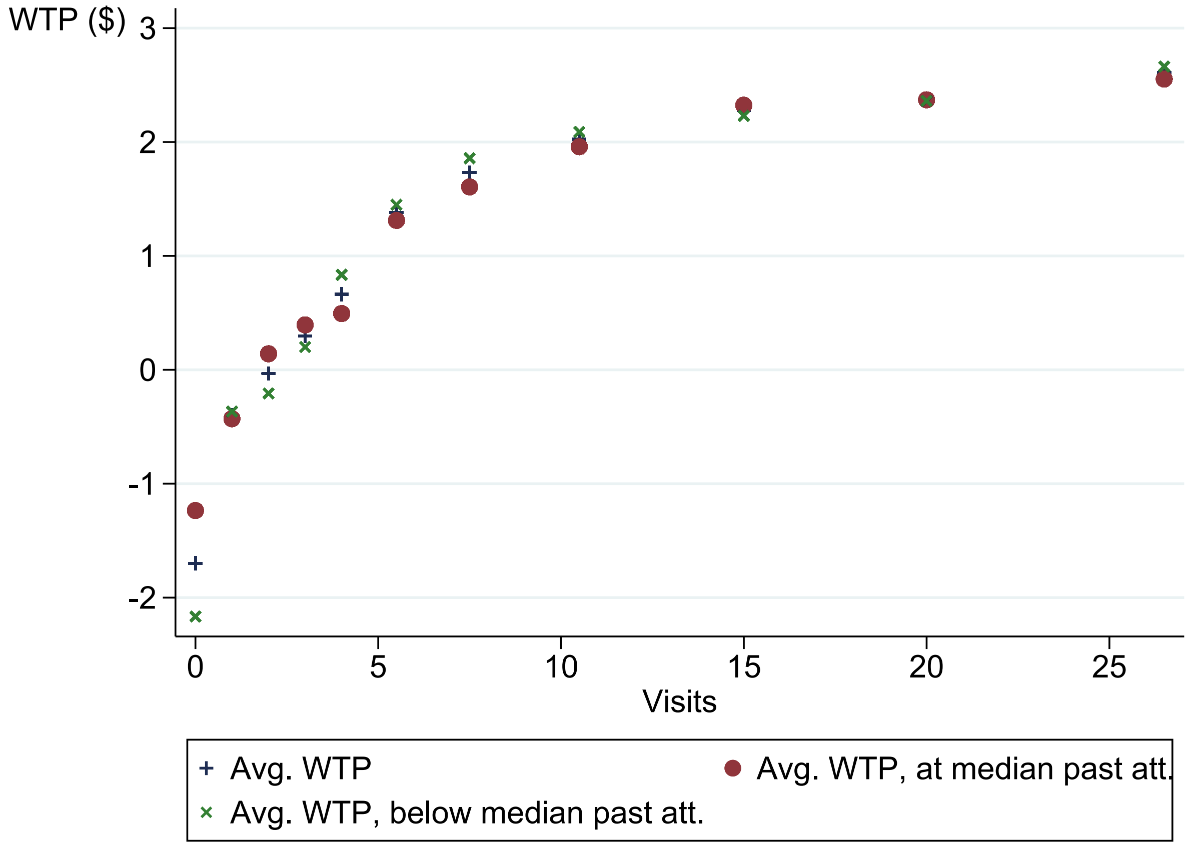
Note: Figure shows the cumulative distribution function of attendance during the experiment across YMCA members in the social recognition treatment and the treatment without social recognition.

Figure 2: Cumulative distribution function of attendance during the experiment by treatment



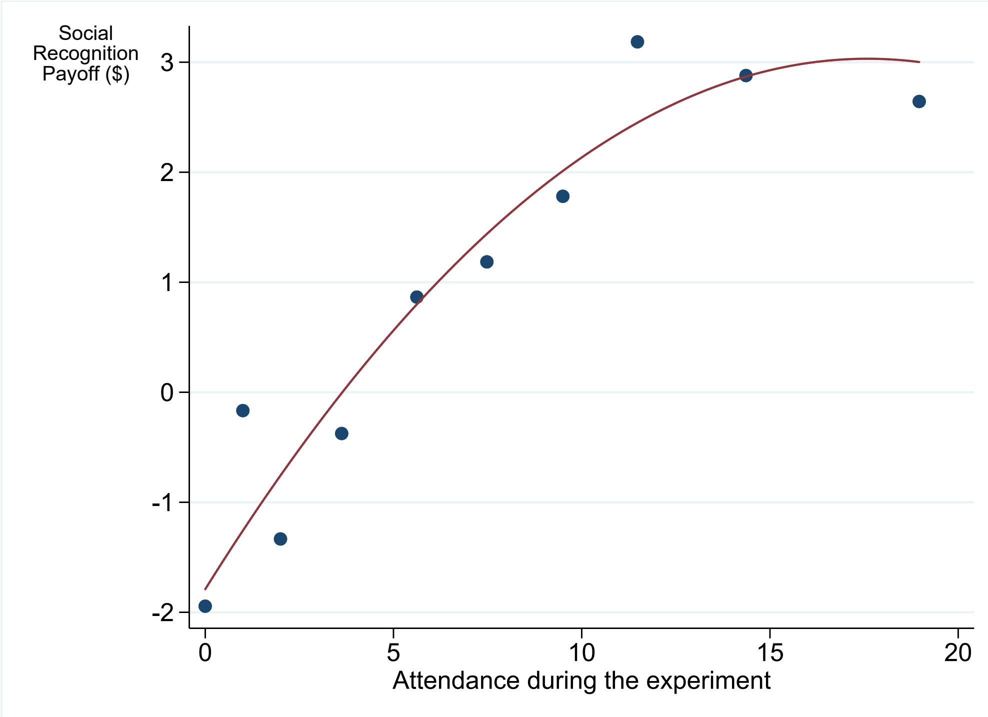
Notes: Figure 2 shows the cumulative distribution function of attendance during the experiment across YMCA members in the social recognition treatment and the treatment without social recognition.

Figure 3: The reduced-form social recognition function (R)



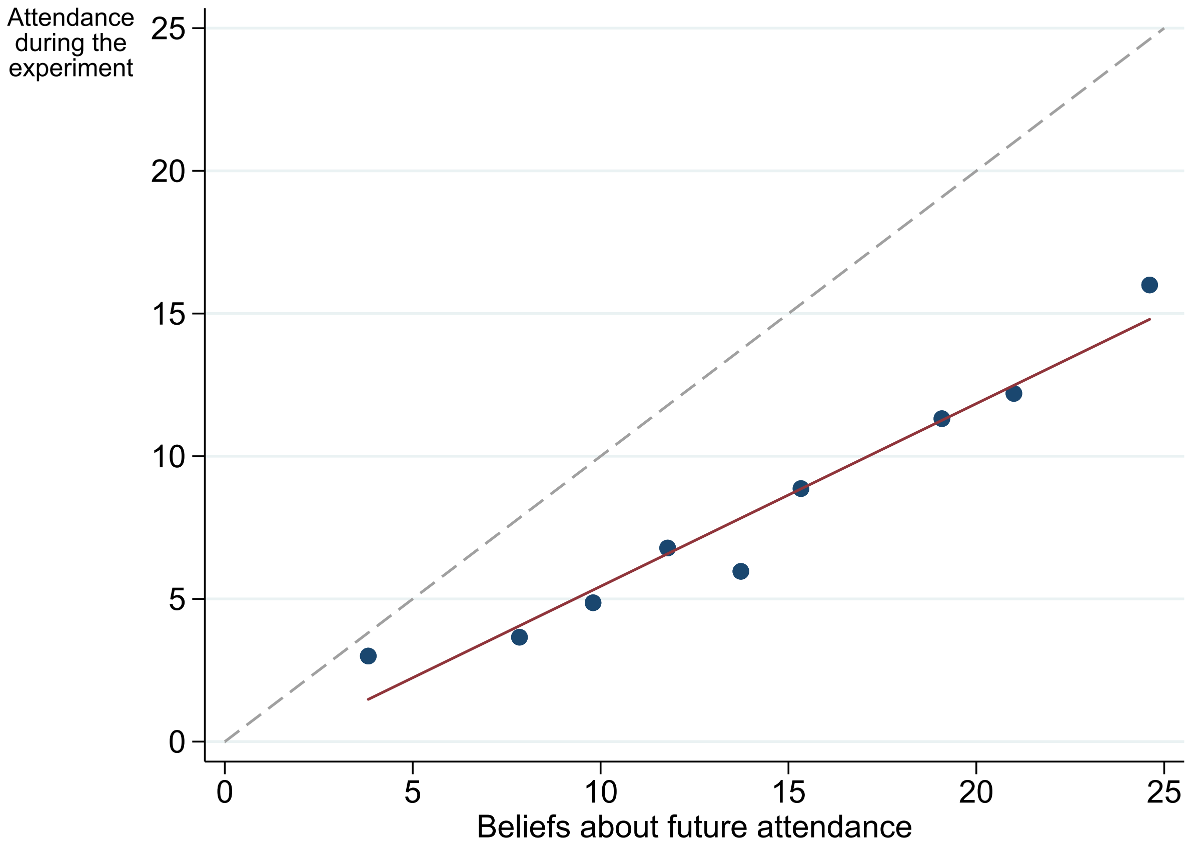
Notes: Figure 3 plots the average WTP for each of the eleven intervals of possible future attendances to the YMCA during the experiment. For intervals including more than one number of visits (e.g., "between 7 and 8 visits") the WTP is plotted at the average point of visits. The figure separately reports the average WTP for the whole sample of coherent participants, and for coherent participants whose average attendance prior the experiment was below/above the median past attendance.

Figure 4: Image payoffs by realized attendance during the experiment



Notes: Figure 4 presents a binned scatterplot of the WTP of each subject to receive social recognition at their realized attendance level. The red line is a quadratic fit.

Figure 5: Beliefs about future attendance by attendance during the experiment



Notes: Figure 5 presents a binned scatterplot summarizing the relationship between the actual attendance of subjects in the experiment and their beliefs about their attendance. For subjects in the social recognition treatment, their elicited beliefs about attendance the next month in the presence of social recognition are plotted. For subjects not in the social recognition treatment, their elicited beliefs about attendance the next month absent social recognition are plotted.

Table 1: Balance table

	No SR Treatment	SR Treatment	p-value
Average WTP (over all possible N. of visits)	0.98	1.06	0.88
	(5.26)	(5.16)	
Average monthly attendance prior experiment (all 13 months)	6.87	6.47	0.52
	(5.75)	(5.64)	
Beliefs about attendance assuming social recognition	14.05	13.61	0.51
	(5.91)	(6.24)	
Beliefs about attendance assuming no social recognition	12.70	12.01	0.29
	(5.95)	(6.09)	
Gender (0=Male; 1=Female)	0.72	0.73	0.89
	(0.45)	(0.45)	
Age	43.89	43.54	0.78
	(11.17)	(11.70)	
No. of Subjects	171	168	

Note: Table 1 reports summary statistics for our main variables of interest across all coherent participants in the two treatments. Variable "Average WTP (over all possible N. of visits)" is the average individual WTP across all possible intervals of future attendance. Variables "Beliefs about attendance assuming (no) social recognition" reports the average expectations YMCA members had about their future attendance assuming that they would (not) be part of the social recognition treatment. The last column reports two sided p-values to test for balance across our experimental treatment. Standard deviations are reported in parentheses.

Table 2: The effect of social recognition (SR) on attendance

Model	OLS	OLS	OLS	OLS	IV
Dependent Var.	att.	att.	att.	att.	att.
	(1)	(2)	(3)	(4)	(5)
SR	1.19	1.57***	1.59***	0.64	0.40
	(0.73)	(0.45)	(0.44)	(0.68)	(0.73)
SR * Avg. past att.				0.14*	
				(0.08)	
SR * Predicted no-SR-att					0.14**
					(0.07)
Avg. past att.		0.94***	0.86***	0.79***	0.80***
		(0.04)	(0.05)	(0.06)	(0.05)
Beliefs			0.13***	0.13***	0.12***
			(0.04)	(0.04)	(0.04)
Constant	6.96***	0.54	-0.75	-0.27	-0.15
	(0.52)	(0.42)	(0.60)	(0.65)	(0.63)
No. of Subjects	339	339	339	339	339

Notes: Table 2 presents regression estimates of the effect of social recognition on attendance during the month of the experiment. "Beliefs" reports the expectations YMCA members had about their future attendance assuming that they would be part of the social recognition treatment. The analysis excludes the 46 individuals with "incoherent" preferences for social recognition. Table H.2 repeats this analysis including these 46 individuals. Standard errors are reported in parentheses.*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 3: Willingness to pay for social recognition conditional on number of visits

Model	OLS	OLS	Tobit	Tobit
Dependent Var.	WTP	WTP	WTP	WTP
	(1)	(2)	(3)	(4)
N. of visits	0.13***	0.39***	0.25***	0.68***
	(0.01)	(0.04)	(0.03)	(0.08)
N. of visits squared		-0.01***		-0.02***
		(0.00)		(0.00)
Constant	-0.14	-0.91***	-0.69	-2.00***
	(0.32)	(0.34)	(0.63)	(0.68)
Observations	3729	3729	3729	3729
No. of Subjects	339	339	339	339

Note: Table 3 reports coefficient estimates from linear and quadratic models of willingness to pay for social recognition at different numbers of visits. Columns (1) and (2) use OLS models while columns (3) and (4) use Tobit models. The analysis excludes the 46 individuals with "incoherent" preferences for social recognition. Table H.3 repeats this analysis including these 46 individuals. Standard errors, clustered at the subject level, are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 4: Willingness to pay for social recognition conditional on number of visits—heterogeneity analysis

Model	OLS	OLS	OLS	Tobit	Tobit	Tobit
Dependent Var.	WTP	WTP	WTP	WTP	WTP	WTP
	(1)	(2)	(3)	(4)	(5)	(6)
N. visits	0.35***	0.43***	0.41***	0.62***	0.74***	0.70***
	(0.05)	(0.06)	(0.06)	(0.11)	(0.12)	(0.12)
N. visits squared	-0.01***	-0.01***	-0.01***	-0.02***	-0.02***	-0.02***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Avg. past att.			0.03			0.04
			(0.06)			(0.12)
N. visits * Avg. past att.			-0.00			-0.00
			(0.01)			(0.01)
N. visits sq. * Avg. past att.			0.00			0.00
			(0.00)			(0.00)
Constant	-0.74	-1.08**	-1.11**	-1.80*	-2.19**	-2.26**
	(0.48)	(0.47)	(0.52)	(0.98)	(0.95)	(1.04)
Observations	1870	1859	3729	1870	1859	3729
No. of Subjects	170	169	339	170	169	339
Sample	$\geq$ Median	$<$ Median	All	$\geq$ Median	$<$ Median	All

Note: Table 4 reports coefficient estimates from linear and quadratic models of willingness to pay for social recognition at different numbers of visits. Columns (1) and (2) replicate Column (2) from 3, restricting the sample to subjects who have above (below) median attendance during the fifty-four weeks prior to the experiment. Likewise, Columns (4) and (5) repeat Column (4) from 3, restricting the sample to subjects who have above (below) median attendance during the fifty-four weeks prior to the experiment. Columns (3) and (6) repeat Columns (2) and (4) from 3, but including interaction terms between past attendance and number of visits. The analysis excludes the 46 individuals with "incoherent" preferences for social recognition. Table H.4 repeats this analysis including these 46 individuals. Standard errors, clustered at the subject level, are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 5: Structural Model Results

(a) Model Parameters

	$\tilde{\gamma}_1$	$\tilde{\gamma}_2$	η
Action-Based	0.41	0.02	1.32
	(0.04)	(0.00)	(0.49)
Type-Based	0.35	0.02	1.11
	(0.04)	(0.00)	(0.40)

(b) DWL Results

	(1): DWL	(2): Change in Att.	(3): $\frac{(1)}{(2)}$	(4): EPM	(5): $\frac{(3)}{(4)}$
Action-Based	0.32	0.72	0.45	0.36	1.23
	[0.23, 0.38]	[0.30, 1.17]	[0.27, 0.89]	[0.14, 0.59]	[0.47, 6.10]
Type-Based	0.35	0.60	0.58	0.27	2.15
	[0.20, 0.50]	[0.23, 1.02]	[0.23, 1.31]	[0.11, 0.43]	[0.65, 12.31]

Note: Panel 5a presents the GMM estimates from the moment conditions defined in 5.2. Standard errors are clustered at the individual level, and are computed from the variance-covariance matrix derived from the efficient two-step estimator described in Section 5.2. Panel 5b presents the estimates for the counterfactual of enacting social recognition for the whole YMCA of the Triangle Area population. Column (1) presents estimates of the DWL. Column (2) presents estimates of the average change in attendance, using equation (5). Column (4) presents estimates of the equivalent price metric, using equation(8).Bootstrapped percentile-based confidence intervals, sampled by subject with 1000 iterations, are reported in brackets for panel 5b. The analysis excludes the 46 individuals with "incoherent" preferences for social recognition. Table H.5 repeats this analysis including these 46 individuals.

A General definition of deadweight loss

Consumers have utility given by $U = u(a^*, y) + S$, where y is their net earnings, and S is the utility they derive from social recognition, as we defined in the body of the paper. Let Γ denote the social recognition scheme, which consists of a visibility level ν as well as rules for what behaviors are revealed or what signals are sent (e.g., all actions are revealed, or whether one is only a top performer or not). We say $\Gamma = \emptyset$ if visibility is zero or if no information is revealed.

Let p denote a piece-rate incentive for behavior, such that choosing a produces financial rewards of pa to the consumer. For each type θ, define the revenue-neutral incentive scheme $C_\Gamma(\theta) = (L_\Gamma(\theta), p_\Gamma(\theta))$ such that $p_\Gamma > 0$ and $L_\Gamma(\theta) = -p_\Gamma(\theta)a_\Gamma(\theta)$, and such that C induces the effect on type θ's behavior as does Γ.[40] Then we say that the deadweight loss of social recognition is how much higher consumer surplus would be under incentives $\{C_\Gamma(\theta)\}$ than under Γ. Our definition of normalized deadweight loss is with respect to the average piece-rate incentives $\bar{p}_\Gamma = \int_{\theta \in \Theta} p_\Gamma(\theta)\mu(\theta)$, where μ is the probability measure on types.

To formalize consumer surplus, we follow Auerbach (1985). Let $a^*(p, Z, \Gamma; \theta)$ denote the action given piece-rate incentive p, wealth Z, transfer $-L$, and social recognition scheme Γ. Let $V(p, Z, \Gamma; \theta) = u(a^*, Z, \Gamma; \theta) + R(a^*)$ denote consumer's indirect utility, where $R(\cdot)$ is the reduced-form social recognition function mapping actions to utils. Generalizing the typical definition of the consumer expenditure function, we define $e(p, \Gamma; \theta)$ as the minimum wealth necessary to obtain utility V given a piece-rate incentive p and social recognition scheme Γ. The deadweight loss of social recognition is

$$DWL(\Gamma) = \int e(0, \Gamma; \theta)\mu(\theta) - \int e(p_\Gamma, \emptyset; \theta)\mu(\theta)$$

[40] Formally, such an incentive scheme always exists as long as substitution effects don't overwhelm income effects in the material utility function, an innocuous assumption that we make.

B Signaling model microfoundations

Define $\phi(\theta) = \frac{\theta(1+\gamma_1)}{1+\theta\gamma_2}$. The structural social recognition function that leads to a quadratic reduced-form social recognition function is given by $S(\theta, \bar{\theta}) = \gamma_1 \phi(\theta) - \frac{\gamma_2}{2}\phi(\theta)^2 - \bar{s}$, where $\bar{s} = \gamma_1 \phi(\bar{\theta}) - \frac{\gamma_2}{2}\phi(\bar{\theta})^2$. Note that for small values of θ, $\phi(\theta) \approx \theta(1+\gamma_1)$, and thus S is approximately quadratic:

$$S \approx \gamma_1(1+\gamma_1)\theta - \frac{\gamma_2(1+\gamma_1)^2}{2}\theta^2 - \bar{s}.$$

Using the same approximation for $\bar{\theta}$, i.e., $\phi(\bar{\theta}) \approx \bar{\theta}(1+\gamma_1)$,we can also see S is approximately quadratic in $\theta - \bar{\theta}$:

$$S \approx \left(\gamma_1(1+\gamma_1) - \bar{\theta}\gamma_2(1+\gamma_1)^2\right)(\theta - \bar{\theta}) - \frac{\gamma_2(1+\gamma_1)^2}{2}(\theta - \bar{\theta})^2 \tag{9}$$

We now show that an equilibrium action function is given by $a^*(\theta) = \phi(\theta)$. To see this, note that if it were the case, then then the reduced-form social recognition function would be given by $R = \gamma_1 a - \frac{\gamma_2}{2}a^2 - \bar{s}$. Given this reduced-form social recognition function, equation (5) gives that each type θ's optimal response is then simply

$$\begin{aligned} a^* &= \theta \frac{1+\eta\gamma_1}{1+\theta\gamma_2} \\ &= \phi(\theta) \end{aligned}$$

This shows that $a^*(\theta) = \phi(\theta)$ is, indeed, an equilibrium action function. Finally, because the material utility function $a - \frac{1}{2}\frac{a^2}{\theta}$ satisfies the single-crossing property (the derivative with respect to a, $1 - \frac{a}{\theta}$, is increasing in θ), the results of Mailath (1987) imply that this separating equilibrium must be a unique separating equilibrium.

The remaining appendices are for online publication only

C Screenshots from the Experiment

Figure C.1: Invitation email to participate in Grow & Thrive

Dear YMCA Member,

The YMCA of the USA, the University of Chicago, and an anonymous benefactor are partnering again to bring you Grow & Thrive, an initiative that helps YMCA members live healthier and happier lives by staying active and supporting their community at the same time. We are inviting you as a YMCA member to be a part of this exciting pilot program!

To get started, we're asking you to complete a short survey. Once you complete the survey, you'll be entered in a drawing to win a $250 Y program voucher. At the end of the survey, you'll be given the chance to participate in the Grow & Thrive pilot. **Grow & Thrive lasts for one month and if you agree to participate you will receive an incentive each time you exercise at the Y between June 15 and July 15.**

To learn from the Grow & Thrive pilot program, we will share anonymous, aggregated data with University of Chicago researchers. This data will not contain any of your personal information.

Please click the survey link below to receive more program information. The survey closes on June 10, 2017.

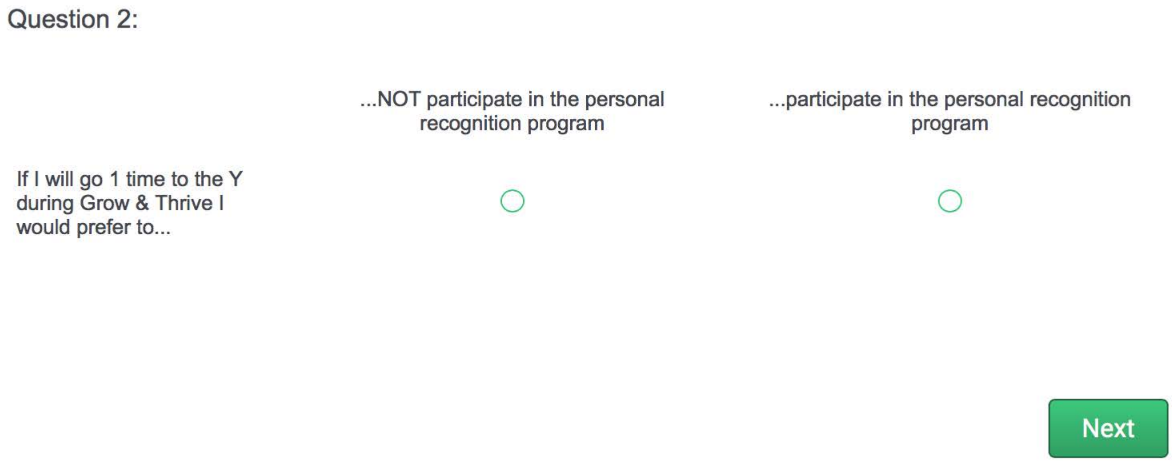
Grow & Thrive Survey Link

Thank you for being a part of the Y. We hope you'll be a part of this exciting initiative and help us continue to look for ways to build a stronger, healthier community.

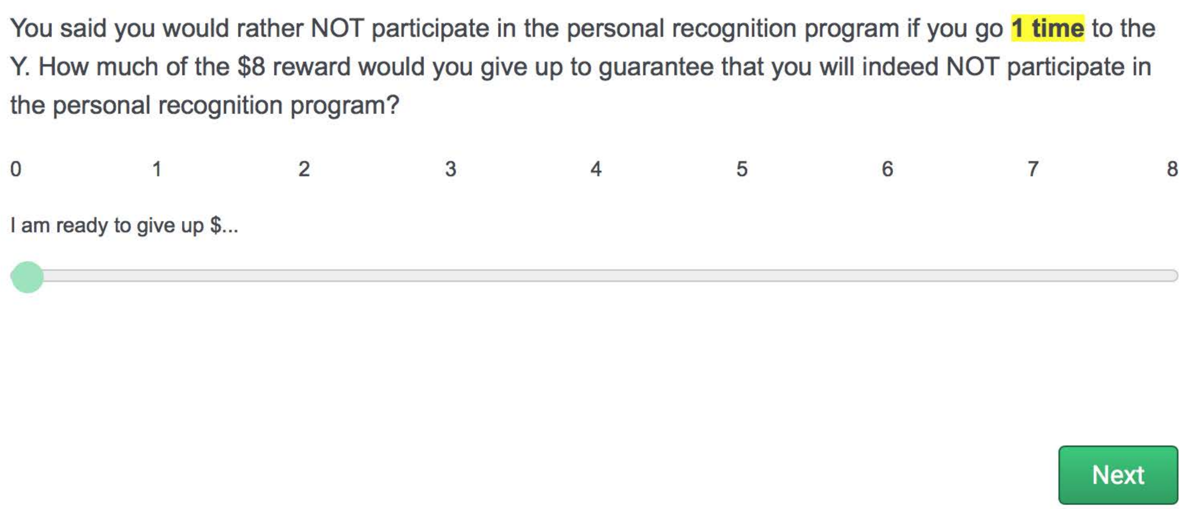
Figure C.1 shows a screenshot of the invitation email sent to YMCA members.

Figure C.2: Non-parametric elicitation of WTP: an example

(a) Strategy method



(b) BDM mechanism



Note: Figure C.2 shows a screenshot from our survey of the WTP elicitation for each number of possible visits to the YMCA during the experiment. The example above reports the elicitation of WTP for attending one time. The top panel asks whether a participant wants to be socially recognized for attending one time. Participants make their choice by clicking their favorite option. The bottom panel asks how much a participant is willing to pay (from $0 to $8) to guarantee that his favorite option is implemented (notice that the bottom panel assumes that the participant answered that he prefers not be recognized for attending one time). Participants choose the amount by moving the slider bar.

D Transcript of Frequently Asked Questions

FAQ – Grow & Thrive

Prepared for YMCA employees

Some general comments

If asked questions at the front desk, it is best if the front desk person says that he/she is aware of the Grow & Thrive program, but that she encourages the Y member to email questions to a specific person who can answer more thoroughly. For instance, the Y member inquiring could be provided with an email address.

Y staff should NOT reveal to Y members that they are part of an economic experiment. If Y members ask about why different people get different incentives, answer as per FAQ.

Below you will find a list of questions that YMCA members may ask, and the corresponding suggested answers.

Q: I have only completed the first part of the survey: will I still participate in drawing of $250 Y voucher?
A: Yes

Q: I am curious: why are people randomized/assigned to the personal recogntion program and to receive extra rewards?
A: Grow & Thrive is a pilot program: as such, we are evaluating different ways in which we might implement it on a larger scale in the future. [*if the person insists*:] We cannot provide further details at this moment.

Q: I have agreed to participate but now I don't want to be in the social recognition program: can you take me out of the social recognition program?
A: By agreeing to participate in Grow & Thrive, you have also accepted the possibility to be randomly assigned by a computer lottery to participate in the social recognition program. If you do not want to be part of the social recognition program, you may withdraw from Grow & Thrive, in which case however you will also NOT receive any of the incentives of the program.

Q: Why is University of Chicago involved?
A: University of Chicago scholars will help us evaluating our Grow & Thrive program and help us to improve it in the future.

Q: Is this an experiment?
A: Grow & Thrive is a pilot program: if successful, we hope in the future to implement it on a larger scale to help our YMCA members live a healthier and fuller life.

Q: When will my donations be made?
A: In July after the end of the Grow & Thrive program, we will email you with details on how the donations will be implemented.

Q: During Grow & Thrive, do I have to check in with the front desk when I come to the Y?
A: No, if you are part of Grow & Thrive, at the end of the month the Y staff will check how many times you came to the Y by counting the number of days you swiped your Y card to access the facilities.

Q: How will I know if I won the $250 Y voucher?
A: We will notify the winner of the drawing after the end of Grow & Thrive

Q: If I win, will I receive the $250 in cash?
A: You will receive it in the form of a discount on your future YMCA membership.

Q: Who is providing the money for Grow & Thrive?
A: An anonymous benefactor who has a strong passion for promoting healthy living and supporting the broader community.

Q: Who else is participating in the social recognition program?
A: I don't have this information at the moment, I am sorry.

Q: In which group am I? [*bits of this answer can also be used for people asking why they didn't get the extra reward, or why they are (are not) in the social recognition program despite their answers in the survey etc.*]
A: We will email you by June 15th to let you know which group you have been randomly assigned to. If you agreed to participate, with 10% chance a computer lottery will randomly select one of the answers you gave, and what you chose in that question will determine your program and reward. Otherwise, your program and reward will be randomly assigned to you: in this case you may or may not be part of the social recognition program.

Q: What is the social recognition program again?
A: Some Grow & Thrive members will also be part of a social recognition program. This group will receive at the end of the month a thank-you email, which will list their names and highlight the good that they have done for themselves by going to the Y during the month. The email will show how many times each participant of the social recognition program went to the Y during the 1-month Grow & Thrive. If you are not part of the social recognition program you will not receive nor be listed in this thank-you email. This is the only difference between being part or not of the social recognition program.

E Pre-trends in attendance by treatment group

Figure E.1 compares the average monthly attendance of the YMCA members for the treatment group with social recognition to the treatment group absent social recognition.

Figure E.1: Attendance by treatment

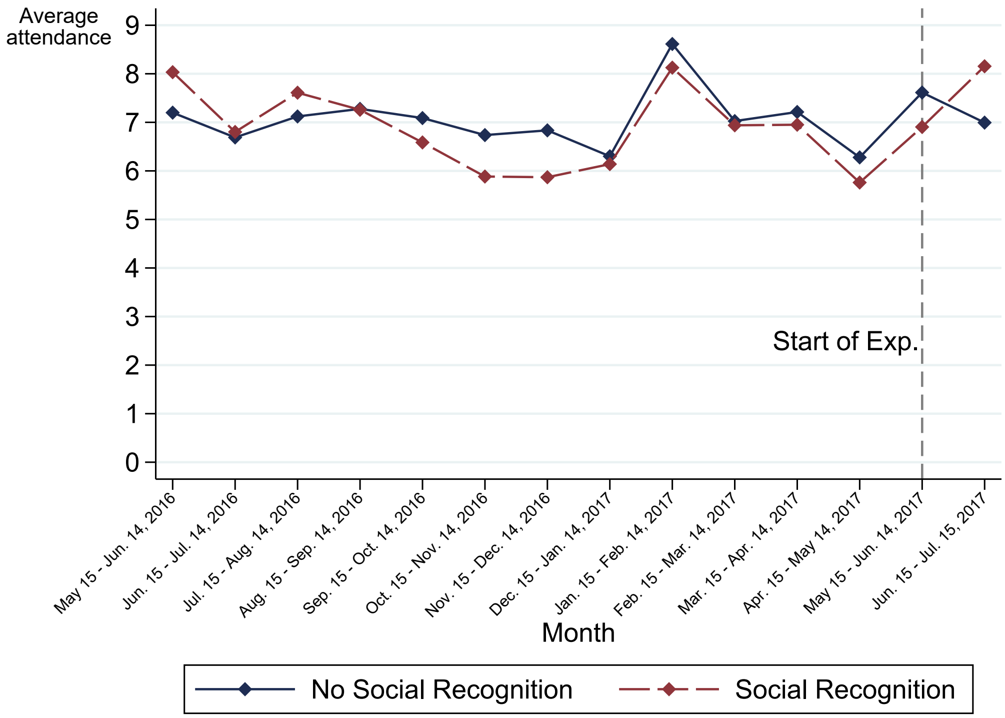


Figure E.1 shows the average monthly attendance of YMCA members in our two treatment groups during the thirteen months prior the experiment, and the month of the experiment..

F Reduced form SR function by member characteristics

Table F.1 repeats columns (2) and (4) of Table 3 including interactions with subjects' age, gender (0 = male, 1 = female), average monthly past attendance, and donation to the last YMCA campaign. In columns (2) and (4), we report the Wald Statistic for the test that the coefficients on all the characteristics and their interactions with number of visits and number of visits squared are zero. These statistics correspond to p-values of 0.06 and 0.16 respectively. Although the evidence that the SR function varies by characteristics is weak, the results are suggestive that women have a less concave SR function than men. Because women are over-represented in our experimental sample, this implies that our deadweight loss estimates for the full YOTA population are slight lower bounds

Table F.1: Willingness to pay for social recognition conditional on number of visits—demographic analysis

Model	OLS	OLS	Tobit	Tobit
Dependent Var.	WTP	WTP	WTP	WTP
	(1)	(2)	(3)	(4)
N. visits	0.39***	0.67***	0.68***	1.20***
	(0.04)	(0.17)	(0.08)	(0.31)
N. visits squared	-0.01***	-0.02***	-0.02***	-0.03***
	(0.00)	(0.01)	(0.00)	(0.01)
Avg. past att.		0.04		0.07
		(0.06)		(0.13)
N. visits * Avg. past att.		-0.00		-0.01
		(0.01)		(0.01)
N. visits sq. * Avg. past att.		0.00		0.00
		(0.00)		(0.00)
Age		-0.02		-0.04
		(0.03)		(0.05)
N. visits * Age		-0.00		-0.00
		(0.00)		(0.01)
N. visits sq. * Age		-0.00		-0.00
		(0.00)		(0.01)
Gender (1 = Female)		1.21		2.47*
		(0.75)		(1.47)
N. visits * Female		-0.31***		-0.56***
		(0.09)		(0.17)
N. visits sq. * Female		0.01***		0.02***
		(0.00)		(0.01)
Donation to last YMCA campaign		0.00		0.00
		(0.00)		(0.01)
N. visits * Donation		-0.00*		-0.00
		(0.00)		(0.00)
N. visits sq. * Donation		0.00**		0.00
		(0.00)		(0.00)
Constant	-0.91***	-1.10	-2.00***	-2.31
	(0.34)	(1.43)	(0.68)	(2.65)
Wald statistic (12 restrictions)		1.40		1.63
P-value		0.06		0.16
Observations	3729	3729	3729	3729
No. of Subjects	339	339	339	339

Note: Table F.1 reports coefficient estimates from linear and quadratic models of willingness to pay for social recognition at different numbers of visits. Columns (1) and (3) replicate columns (3) and (6) from Table 3. Columns (2) and (4) repeat Columns (1) and (3), but including interaction terms between number of visits and average past attendance, gender (0 = male, 1 = female), age, and donation to the previous YMCA campaign..For columns (2) and (4) we report the Wald statistic for these demographic controls and their interactions with number of visits (12 restrictions) and the corresponding p-values. The analysis excludes the 46 individuals with "incoherent" preferences for social recognition. Standard errors, clustered at the subject level, are reported in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

G Comparison of GMM and bootstrap results

For robustness, we repeat the GMM procedure outlined in 5.2, but here use the procedure to estimate change in attendance, equivalent price metric, and deadweight loss directly rather than via bootstrap. Mathematically, we minimize $\left(m(\xi) - m(\hat{\xi})\right)' W \left(m(\xi) - m(\hat{\xi})\right)$, where $\xi := (\tilde{\gamma}_1, \tilde{\gamma}_2, \eta, \tau_{SR}, b, \bar{a}_{0,exp}, \Delta a, EPM, DWL)$. To do so, we use 5 and 8 as additional moment conditions for both the type-based and action-based models. We also use 7 as an additional moment condition for the action-based model. We then use these estimates to calculate $\frac{DWL}{\Delta a}$, the deadweight loss per attendance, and $\frac{\frac{DWL}{\Delta a}}{EPM}$, the deadweight loss per dollar of behaviorally-equivalent financial incentives. The standard errors from this approach are calculated using the multivariate delta method. We illustrate with the deadweight loss per attendance ratio. First note that the DWL and Δa estimated by GMM are consistent. We denote the estimated values $D\hat{W}L$ and $\Delta\hat{a}$, and use N to denote the number of observations. To calculate the standard errors for the deadweight loss per attendance, we first note that, using the first two terms of the Taylor approximation:

$$\frac{D\hat{W}L}{\Delta\hat{a}} \approx \frac{DWL}{\Delta a} + \nabla\left(\frac{DWL}{\Delta a}\right) \cdot \left(\frac{D\hat{W}L}{\Delta\hat{a}} - \frac{DWL}{\Delta a}\right)$$

Taking the variance of both sides:

$$Var\left[\frac{D\hat{W}L}{\Delta\hat{a}}\right] \approx Var\left[\frac{DWL}{\Delta a} + \nabla\left(\frac{DWL}{\Delta a}\right) \cdot \left(\frac{D\hat{W}L}{\Delta\hat{a}} - \frac{DWL}{\Delta a}\right)\right]$$

After simplifying, this yields:

$$Var\left[\frac{D\hat{W}L}{\Delta\hat{a}}\right] \approx Var\left[\frac{Var(DWL)}{N\mu_{DWL}^2} - 2\frac{Cov(DWL, \Delta a)}{\mu_{DWL}^2} + \frac{Var(\Delta a)\mu_{\Delta a}^2}{N\mu_{DWL}^4}\right]$$

Note that the DWL and Δa estimated by GMM are consistent, so we can use the estimated moments of these parameters in the right-hand side of the above equation to estimate $Var\left[\frac{D\hat{W}L}{\Delta\hat{a}}\right]$ (e.g., use $\hat{\mu}_{\Delta a}$for $\mu_{\Delta a}$). Note that for the delta method and the resulting confidence intervals to be a good approximation, the distribution of the underlying parameter must be approximately normal (Angrist and Pischke, 2008). We provide normal probability plot of this in Figure G.1 as a visual test. This figure compares the simulated quantiles of each of these parameters from 1000 bootstrap iterations to corresponding quantiles were the distribution normal. This figure suggests that the iterations of each parameter, and the DWL per attendance are well approximated by a normal. It also suggests that $\frac{\frac{DWL}{\Delta a}}{EPM}$ is not well approximated by a normal, and that there is a significant right tail. Thus the standard errors and resulting confidence intervals from the delta method are likely to be a poor approximation. Table G compares the point estimates and confidence intervals from this approach and the approach outlined in 5. Comparing the point estimates and confidence interval bounds, we note there are large discrepancies for the deadweight loss per dollar of behaviorally-

Table G.1: Structural model results: Comparison of bootstrap and GMM approaches

(a) Bootstrap Results

	(1): DWL	(2): Change in Att.	(3): $\frac{(1)}{(2)}$	(4): Price Eq.	(5): $\frac{(3)}{(4)}$
Action-Based	0.32	0.72	0.45	0.36	1.23
	[0.23, 0.38]	[0.30, 1.17]	[0.27, 0.89]	[0.14, 0.59]	[0.47, 6.10]
Type-Based	0.35	0.60	0.58	0.27	2.15
	[0.20, 0.50]	[0.23, 1.02]	[0.23, 1.31]	[0.11, 0.43]	[0.65, 12.31]

(b) GMM Results

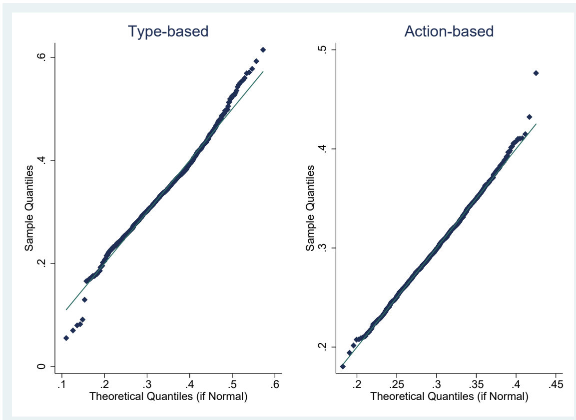
	(1): DWL	(2): Change in Att.	(3): $\frac{(1)}{(2)}$	(4): Price Eq.	(5): $\frac{(3)}{(4)}$
Action-Based	0.30	0.72	0.42	0.36	1.19
	[0.23, 0.38]	[0.30, 1.19]	[0.19, 0.66]	[0.12, 0.59]	[-0.24, 2.62]
Type-Based	0.31	0.60	0.51	0.27	1.89
	[0.16, 0.45]	[0.22, 0.99]	[0.17, 0.85]	[0.10, 0.44]	[-0.31, 4.09]

Note: Panel G.1a repeats Panel 5b, our preferred structural estimates. These use bootstrapped percentile-based confidence intervals, sampled by subject with 1000 iterations, are reported in brackets. Panel G.1b presents estimates and confidence intervals using GMM and the delta method outlined in Section G.

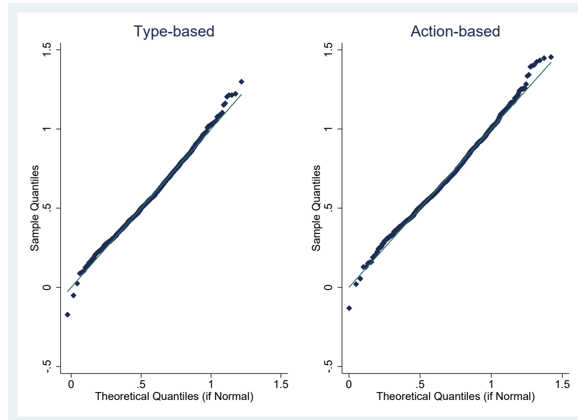
equivalent financial incentives between the two approaches. This provides further evidence that the distribution of the statistic is not normal, and that percentile-based confidence intervals are more appropriate.

Figure G.1: Normal Probability Plots of Table 5 structural estimates

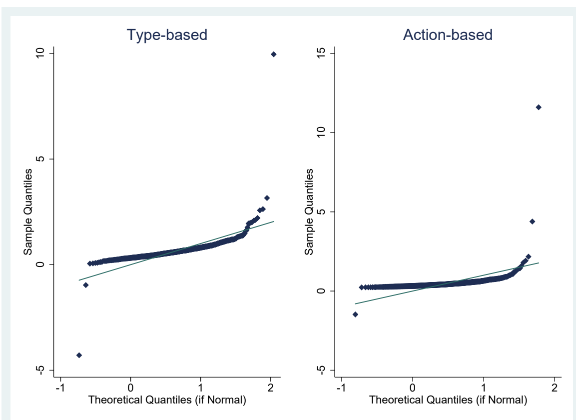
(a) Change in DWL



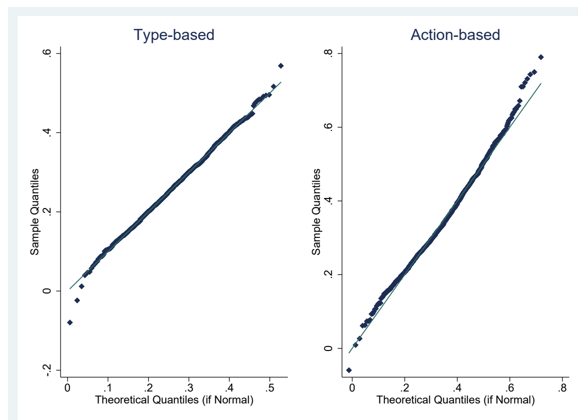
(b) Change in average attendance ($\Delta\bar{a}$)



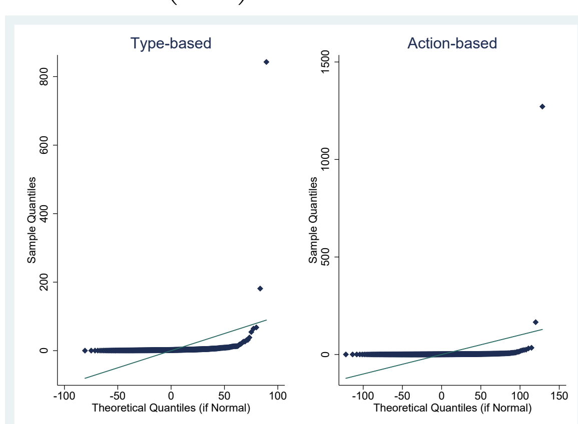
(c) Change in DWL per attendance $\left(\frac{DWL}{\Delta\bar{a}}\right)$



(d) Change in EPM



(e) Change in DWL per $ of behaviorally equiv. financial incentives $\left(\frac{\frac{DWL}{\bar{a}}}{EPM}\right)$



Notes: For each structural estimate in Table 5b, Figure G.1 compares the simulated quantiles from 1000 bootstrap iterations to corresponding quantiles were the distribution normal. Estimates that are better approximated by the normal distribution lie closer to the 45 degree line. These estimates are charted separately for the type-based and action-based models.

H Results using the full sample

In our main analysis we exclude 46 participants who gave "incoherent" answers in our elicitation of WTP for social recognition. We define a participant to be incoherent if he/she "switches" from wanting to be socially recognized to not wanting to be socially recognized as the number of attendances increases. This leaves us with a sample of 339 participants. In this appendix we repeat Tables 1– 5 using the full sample of 385, i.e., including all 46 participants. The results are seen in Tables 1– H.5. We find that all our results are robust to including these incoherent participants.

Comparing Table H.1 to Table 1, we see that including the incoherent participants does not lead to the sample being unbalanced along any dimension. In the thirteen months preceding the experiment, participants on average attended with and without social recognition, respectively 6.34 and 6.65 ($p = 0.59$) times per month. This is slightly lower than 6.47 and 6.87 ($p = 0.52$) attendances with and without social recognition respectively from our preferred sample, but well in the same range.

In Table H.2 we reestimate that social recognition increases attendance by 16-20% over a one-month period in our experiment.[41] These are similar to the estimated 17-23% change in attendance calculated using our preferred sample of coherent participants. Compared to Table 2, we do see decreased significance in the interaction between social recognition and average past attendance($p =$ 0.17) in Column (4) and between social recognition and the predicted attendance absent social recognition in Column (5) ($p = 0.14$).

Tables H.2 and H.4 show that our main results about the reduced-form social recognition function are robust to including the incoherent participants. Columns (1) and (2) of Table H.2 present OLS regressions of linear and quadratic fits of the reduced form social recognition function. Columns (3) and (4) replicate this analysis with Tobit regressions, which adjust for the censoring of our WTP estimate at -\$8 and \$8. These results show that the properties of the reduced form social recognition function, WTP for social recognition is increasing and concave in the number of visits, are robust to the sample selection.

Table H.4 also verifies that we cannot reject the social recognition function is the same for those with above median attendance compared to those with below median attendance for this enlarged sample. We can see this by comparing Columns (1) and (2), which use OLS, and Columns (4) and (5), which use the Tobit.

Table H.5 shows the deadweight loss estimates from our structural models when using the full sample. We predict that attendance would increase by 16-20% if the Grow & Thrive program were applied to the whole YMCA of the Triangle Area population using this full sample, which is similar to the 19-23% from our preferred sample. Additionally, our structural estimates of per dollar of behaviorally-equivalent financial incentives would increase from \$1.23-\$2.15 in our preferred sample to \$1.45-\$2.47 when including the incoherent participants.

[41] The numerators of these percentages are the coefficients on SR in columns (1) and (2) of Table H.2. The denominator is the average attendance of the control group, 6.88.

Table H.1: Balance table (Full sample)

	No SR Treatment	SR Treatment	p-value
Average WTP (over all possible N. of visits)	1.23	1.16	0.88
	(5.10)	(4.96)	
Average attendance prior experiment (all 13 months)	6.65	6.34	0.59
	(5.67)	(5.54)	
Beliefs about attendance assuming social recognition	13.93	13.44	0.43
	(5.88)	(6.17)	
Beliefs about attendance assuming no social recognition	12.44	11.87	0.36
	(6.09)	(6.11)	
Gender (0=Male; 1=Female)	0.72	0.74	0.63
	(0.45)	(0.44)	
Age	44.23	43.79	0.70
	(11.12)	(11.67)	
No. of Subjects	192	193	

Note: Table H.1 repeats Table 1 including the 46 subjects who gave "incoherent" answers in our elicitation of WTP for social recognition. Variable "Average WTP (over all possible N. of visits)" is the average individual WTP across all possible intervals of future attendance. Variables "Beliefs about attendance assuming (no) social recognition" reports the average expectations YMCA members had about their future attendance assuming that they would (not) be part of the social recognition treatment. The last column reports two sided p-values to test for balance across our experimental treatment. Standard deviations are reported in parentheses.

Table H.2: The effect of social recognition (SR) on attendance (Full Sample)

Model	OLS	OLS	OLS	OLS	IV
Dependent Var.	att.	att.	att.	att.	att.
	(1)	(2)	(3)	(4)	(5)
SR	1.11	1.40***	1.44***	0.78	0.60
	(0.68)	(0.41)	(0.41)	(0.63)	(0.69)
SR * Avg. past att.				0.10	
				(0.07)	
SR * Predicted no-SR-att					0.10
					(0.07)
Avg. past att.		0.94***	0.86***	0.81***	0.82***
		(0.04)	(0.04)	(0.06)	(0.05)
Beliefs			0.13***	0.13***	0.12***
			(0.04)	(0.04)	(0.04)
Constant	6.88***	0.66*	-0.64	-0.30	-0.21
	(0.48)	(0.38)	(0.56)	(0.60)	(0.60)
No. of Subjects	385	385	385	385	385

Notes: Table H.2 repeats Table 2 including the 46 subjects who gave "incoherent" answers in our elicitation of WTP for social recognition.. This table presents regression estimates of the effect of social recognition on attendance during the month of the experiment. "Beliefs" reports the expectations YMCA members had about their future attendance assuming that they would be part of the social recognition treatment. Standard errors are reported in parentheses.*** p<0.01, ** p<0.05, * p<0.1.

Table H.3: Willingness to pay for social recognition conditional on number of visits (Full Sample)

Model	OLS	OLS	Tobit	Tobit
Dependent Var.	WTP	WTP	WTP	WTP
	(1)	(2)	(3)	(4)
N. of visits	0.10***	0.35***	0.18***	0.60***
	(0.01)	(0.04)	(0.03)	(0.07)
N. of visits squared		-0.01***		-0.02***
		(0.00)		(0.00)
Constant	0.33	-0.41	0.22	-1.04***
	(0.30)	(0.31)	(0.57)	(0.61)
Observations	4235	4235	4235	4235
No. of Subjects	385	385	385	385

Note: Table H.3 repeats Table 3 including the 46 subjects who gave "incoherent" answers in our elicitation of WTP for social recognition.. This table reports coefficient estimates from linear and quadratic models of willingness to pay for social recognition at different numbers of visits. Columns (1) and (2) use OLS models while columns (3) and (4) use Tobit models. Standard errors, clustered at the subject level, are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table H.4: Willingness to pay for social recognition conditional on number of visits—heterogeneity analysis (Full Sample)

Model	OLS	OLS	OLS	Tobit	Tobit	Tobit
Dependent Var.	WTP	WTP	WTP	WTP	WTP	WTP
	(1)	(2)	(3)	(4)	(5)	(6)
N. visits	0.32***	0.38***	0.37***	0.56***	0.64***	0.62***
	(0.05)	(0.05)	(0.05)	(0.10)	(0.10)	(0.11)
N. visits squared	-0.01***	-0.01***	-0.01***	-0.02***	-0.02***	-0.02***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Avg. past att.			0.01			0.01
			(0.06)			(0.11)
N. visits * Avg. past att.			-0.00			-0.00
			(0.01)			(0.01)
N. visits sq. * Avg. past att.			0.00			0.00
			(0.00)			(0.00)
Constant	-0.42	-0.40	-0.49**	-1.20	-0.89	-1.08
	(0.45)	(0.44)	(0.48)	(0.90)	(0.82)	(0.92)
Observations	2123	2112	4235	2123	2112	4235
No. of Subjects	193	192	385	193	192	385
Sample	$\geq$ Median	$<$ Median	All	$\geq$ Median	$<$ Median	All

Note: Table H.4 repeats Table 4 including the 46 subjects who gave "incoherent" answers in our elicitation of WTP for social recognition. This table reports coefficient estimates from linear and quadratic models of willingness to pay for social recognition at different numbers of visits. Columns (1) and (2) replicate Column (2) from H.3, restricting the sample to subjects who have above (below) median attendance during the fifty-four weeks prior to the experiment. Likewise, Columns (4) and (5) repeat Column (4) from H.3, restricting the sample to subjects who have above (below) median attendance during the fifty-four weeks prior to the experiment. Columns (3) and (6) repeat Columns (2) and (4) from H.3, but including interaction terms between past attendance and number of visits. Standard errors, clustered at the subject level, are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table H.5: Structural Model Results (Full Sample)

(a) Model Parameters

	$\tilde{\gamma}_1$	$\tilde{\gamma}_2$	η
Action-Based	0.40	0.02	1.25
	(0.04)	(0.00)	(0.47)
Type-Based	0.31	0.01	1.03
	(0.04)	(0.00)	(0.40)

(b) DWL Results

	(1): DWL	(2): Change in Att.	(3): $\frac{(1)}{(2)}$	(4): EPM	(5): $\frac{(3)}{(4)}$
Action-Based	0.29	0.63	0.47	0.32	1.45
	[0.23, 0.37]	[0.25, 1.08]	[0.29, 1.07]	[0.12, 0.56]	[0.454, 8.44]
Type-Based	0.28	0.50	0.56	0.23	2.47
	[0.12, 0.38]	[0.16, 0.94]	[0.14, 1.76]	[0.07, 0.37]	[0.43, 23.26]

Note: Table H.5 repeats Table 5 including the 46 subjects who gave "incoherent" answers in our elicitation of WTP for social recognition. Panel H.5a presents the GMM estimates from the moment conditions defined in 5.2. Standard errors are clustered at the individual level, and are computed from the variance-covariance matrix derived from the efficient two-step estimator described in Section 5.2. Panel H.5b presents the estimates for the counterfactual of enacting social recognition for the whole YMCA of the Triangle Area population. Column (1) presents estimates of the DWL. Column (2) presents estimates of the average change in attendance, using equation (5). Column (4) presents estimates of the equivalent price metric, using equation (8). Bootstrapped percentile-based confidence intervals, sampled by subject with 1000 iterations, are reported in brackets for panel H.5b.