When incentives backfire: Spillover effects in food choice

Manuela Angelucci[*], Silvia Prina[†], Heather Royer[‡], Anya Samek[§] [¶]

July 17, 2015

Abstract

The spillover effects of incentives are a frequently discussed but understudied topic. They are often thought to amplify the direct effect of incentives themselves. In this paper, however, we investigate the possibility of negative spillover effects, in the spirit of Benabou and Tirole (2003). We analyze these spillover effects through a field experiment involving children food choices – grapes v. cookie. We randomize which child is incentivized, the fraction of children incentivized, and who can observe whether peers' choices are incentivized. We find that, while incentives increase the likelihood of initially choosing grapes, there are both positive and negative non-linear spillover effects. The overall marginal effect of incentives is initially positive but becomes negative when more than 70% of the group is incentivized and incentives are salient. We rule out that this effect is driven by envy and social conformity.

Keywords: food choice, incentives, spillovers, field experiment

JEL Classification: C93, I1, J13

[*]University of Michigan (mangeluc@umich.edu)

[†]Case Western Reserve University (silvia.prina@case.edu)

[‡]University of California Santa Barbara (royer@econ.ucsb.edu) and NBER

[§]University of Wisconsin-Madison (AnyaSamek@gmail.com)

[¶]We are grateful to Tristin Ganter, Justin Holz, and the rest of the Behavioral and Experimental Economics (BEE) Research Group at the University of Chicago for conducting the experiment, to Meera Mahadevan and Irvin Rojas for their outstanding research assistance, and to David Chan, Peter Kuhn, Pedro Rey Biel, and seminar participants at the University of Michigan, University of Chicago Harris School, Universidad Pompeu Fabra, Bocconi University, and the World Bank for their comments. This research was approved by an IRB from the University of Chicago and the University of California-Santa Barbara.

1 Introduction

Incentives, in the form of cash, cash equivalent, or subsidies are commonly used in a range of arenas, including education, health, labor, and pro-social behavior.[1] The implicit premise of the use of incentives is to promote long-lasting behavioral changes. For that to occur, we would expect an increase in the short-term take-up of the incentivized action.

While the aforementioned papers find that incentives generally increase take-up of the incentivized behavior, incentives may also decrease take-up of the incentivized behavior. In a widely-cited piece, Benabou and Tirole (2003) argue that incentives can influence behaviors as extrinsic motivators, but incentives may also signal the difficulty of the task, or other information about the task that the principal has but that the agent does not. This signaling effect could be negative and, thus, reduce the effectiveness of incentives. This effect is consistent with the finding in the psychology literature that incentives make a task less attractive and with evidence from behavioral economics.[2] For example, Gneezy and Rustichini (2000) find in two separate experiments that performance on a task fell when small monetary incentives were offered, compared to offering no monetary incentives.

The incentive literature primarily focuses on the effects of incentives on their recipients – the *direct* effects. We argue that the *spillover* effects may be substantial enough, may counteract the direct effect of incentives, and may be non-linear. Therefore, any evaluation of incentives needs to consider both the direct and spillover effects. This point has both practical significance for the evaluation of field experiments and important implications for the extrapolation of field experiment results to real-world policies.

To illustrate our point, consider the case of choosing a more or less healthy snack – grapes v. cookie. Suppose that we gave monetary incentives to pick grapes to a random group of children and, when asked to make a snack choice simultaneously, we found that incentivized children were more likely to pick grapes. This experiment, which identifies the direct effect of incentives, may appear to suggest that incentives are effective at increasing grape choice. However, now suppose that children could change their mind after seeing their peers' initial choices. In this case, there may be both positive and negative spillover effects of incentives, depending on whether the

[1] A not nearly-exhaustive list of these studies include Volpp et al. (2008, 2009); Charness and Gneezy (2009); Acland and Levy (2015); Babcock and Hartman (2010); Babcock et al. (forthcoming); Cawley and Price (2013); John et al. (2011); Royer, Stehr and Sydnor (forthcoming); Belot, Jonathan and Patrick (2013); List and Samek (2014, 2015) for healthy behaviors, Angrist, Lang and Oreopoulos (2009); Bettinger (2012); Fryer Jr (2011); Levitt et al. (2011); Levitt, List and Sadoff (2011) for academic achievement, Ariely, Bracha and Meier (2009); Lacetera and Macis (2010); Lacetera, Macis and Slonim (2013) for pro-social behavior, and Gneezy and List (2006); Fehr and List (2004); Bandiera, Barankay and Rasul (2013); Shearer (2004) for worker effort.

[2] See Deci et al. (1999) for a meta analysis of the evidence from psychology and Gneezy, Meier and Rey-Biel (2011) and Kamenica (2012) for evidence from economics.

incentive status of others is public or private: while seeing that my peers choose grapes may make grapes more appealing (leading to a positive spillover effect), seeing that my peers are incentivized to choose grapes may send a negative signal about the appeal of grapes (leading to a negative spillover effect). If the latter effect is large enough, it may offset the positive effects of incentives. In that case, the net effect of incentives would be lower than its direct effect, and perhaps even null or negative.

To study these issues, we conducted and experiment to examine food choices among over 1,600 children in grades K-8 in Chicago. At lunchtime, students had the choice of either grapes or cookies. As discussed below, some students randomly received incentives to choose grapes over cookies. This population is ideal for two main reasons. First, relative to most environments, the peer network is well-defined, as students sit together at tables. The lunchroom table is our relevant social network in this experiment. We also consider other network definitions via collected social network data. Second, almost a third of US children aged 2-19 are now deemed overweight or obese, and part of the problem is the habitual decision to consume high calorie, low nutrient foods (Ogden et al., 2010). Incentivizing the choice of healthy food may be one policy tool to reduce child overweight and obesity.

We designed our experiment to identify and estimate the direct and spillover effects of incentives and, in particular, to separate the positive and negative spillover effects. In most situations, it is difficult to isolate the direct and spillover effects of incentives. To sort out these two effects, we first ask students to make their decision between grapes and cookies without knowing their peers' choice. We use this initial choice to identify the direct spillover effect. Then, after seeing their peers' actions, students are allowed to change their minds. We use this second choice to identify the spillover effects of incentives.

Our experiment involves several layers of randomization, each set up to understand the direct and spillover effects of incentives. First, we randomize whether or not each individual receives an incentive to choose grapes. This randomization lets us identify the direct effect of incentives. Second, we randomize the fraction of peers receiving incentives at each lunchroom table. This variation lets us identify the spillover effects. Third, we randomize whether children can observe if their peers choose incentivized grapes (we refer to this as the public and private treatments). This variation allows us to test the signaling effect of incentives. That is, via a contrast of the public and private incentive groups, we can separately identify the positive and negative spillover effects of incentives.

Recording initial and final choice – which allows us to separate the direct and spillover effects of incentives – and changing the amount of public information – which helps us understand the mechanisms behind the spillover effects – are new to the spillover literature. As summarized by Baird et al. (2014), the previous literature identifies spillover effects by not treating some group members, by using plausible exogenous variation in the fractions of peers treated, and by looking at differential treatment effects within a predetermined peer group.[3] Our method of identifying spillover effects resting on randomization relies on more credible identification assumptions.

We report three main findings. First, incentives increase the likelihood of initially choosing grapes by over 50%, from 49% to 75%. Second, in the public treatment (in which both peers' food choice and whether they picked incentivized grapes are observed), the probability of ending up with grapes is a non-monotonic function of the fraction of lunch table mates incentivized. The treatment effect of incentives is maximized when roughly 70% of lunch table mates are incentivized. Third, in contrast, the private treatment (in which only peers' food choice is observed) grows monotonically with respect to the fraction incentivized. In this case, a 1% increase in the proportion incentivized increases the likelihood of ending up with grapes, conditional on one's initial choice, by 0.05 percentage points. Since, in the private treatment, other subjects are not made aware of whether or not their table mates are eligible for incentives for choosing grapes, the contrast of the public versus private treatment is consistent with incentives acting as a signal.

We do not find evidence that the spillover effects are driven by the desire to conform to one's best friends, popular kids, or kids of the same gender. We can also rule out that the negative spillover effects are driven by being envious of other children's incentive status, as these effects are largest in tables in which everybody is incentivized.

Our findings have three broad implications. First, to understand the total impact of incentives, one should consider both their direct and spillover effects. Therefore, one should design randomized control trials that account for spillover effects (Angelucci and Di Maro, 2015; Baird et al., 2014). Second, when providing incentives, one should take into account that their effects may be non-linear in the fraction incentivized. Thus, extrapolation of results from randomized-controlled trials where some fraction of the population is eligible for incentives to policy interventions where everyone may be eligible could be challenging, given the potential for non-linear negative spillovers. Third, if observing others' incentives reduces take-up, private incentives may be preferable. However, keeping incentives private may not be feasible in settings in which

[3] Baird et al. (2014) refers to several papers in each vein of this spillover literature. For papers belonging to the first set, see, e.g.,Angelucci and De Giorgi (2009); Barrera-Osorio et al. (2011); Bobonis and Finan (2009); Duflo and Saez (2003); Lalive and Cattaneo (2009); Guiteras, Levinsohn and Mobarak (2015). For papers belonging to the second set, see, e.g., Babcock and Hartman (2010); Beaman (2012); Conley and Udry (2010); Duflo and Saez (2002); Munshi (2003). For papers belonging to the third set, see, e.g., Banerjee et al. (2013); Chen, Humphries and Modi (2010); Macours and Vakis (2008); Neumark-Sztainer et al. (2012).

people communicate, as often occurs in the school or workplace.

We conjecture that an understanding of how the size of the incentivized population influences spillover effects is likely to be worthwhile in several settings. The first type of setting is one in which the value of the incentivized action is not well known and in which subjects believe peers and policy makers may have private information (e.g., new technology adoption). Such settings allow subjects to learn from peers' and policymakers' actions. The second type of setting is one in which the incentivized behavior has short-run costs but long-run benefits (e.g., nutrition, exercise, education, and other behaviors that increase health and human capital).[4] In this case, the incentive may increase the salience of the short-term cost over the long-term benefit of the incentivized action. The third type of setting is in pro-social behaviors, such as recycling, charitable giving, or blood donation, whose 'warm-glow' value exceeds the monetary value of incentives. Finally, the fourth possible setting is in interesting or pleasant activities, where incentives may reduce interest in the task.[5]

To our knowledge, this is the first paper that models and separately identifies positive and negative spillover effects of incentives. Other studies of spillover effects have results that are broadly consistent with our findings of both positive spillover effects (e.g., in lifestyle habits by Babcock and Hartman (2010) and in enrollment to Tax Deferred Accounts by Duflo and Saez (2003)), and negative spillover effects (e.g., in deworming drugs by Kremer and Miguel (2007)).

2 Theory

In this section, we present a simple model that highlights potential mechanisms that would give rise to both positive and negative direct and spillover effects. In this model, the direct effects of incentives can be positive as incentives change the cost of performing some behavior, but, on the other hand, the effects can be negative if the introduction of incentives provides a negative signal. Similarly, spillover effects may also be positive and negative, as they are the result of two behaviors - response to peer choices (i.e., what fraction of peers chose grapes) and response to peer incentives (i.e., what fraction of peers chose incentivized grapes). Overall the signal of the spillover effects is indeterminate.

Consider the choice of grapes versus cookies for child i in a setting with asymmetric information. The child decides to pick grapes over cookies if the expected private benefits of this choice exceed its costs. The child's beliefs of the benefits, B_i, depend on her idiosyncratic taste, τ_i, as well as on the behavior of her peers and of the experimenter, whom the child believes to

[4] Gneezy, Meier and Rey-Biel (2011) review the evidence of incentives backfiring in this type of setting.
[5] Kamenica (2012) review the evidence of incentives backfiring for the last two settings.

have private information about the relative value of grapes over cookies. The child observes the behavior of her peers and of the experimenter to infer their private information.[6] The choice depends also on the monetary (or cash-equivalent) cost of choosing grapes over cookies, C. This cost is a function of incentives, I.

The child observes a signal of the private information of her peers and of the experimenter from their actions. In our experiment, she observes i) the fraction of her peers who choose grapes over cookies, $\bar{G}_{-i}$, ii) the fraction of her peers who choose incentivized grapes, $\bar{I}_{-i}$, and iii) whether she is incentivized to pick grapes. $\bar{I}_{-i}$ is a lower bound of the fraction of peers who were incentivized to choose grapes, TP. In our experiment, we can distinguish behavior in response to i) versus ii) by comparing the private and public treatments.

In sum, the expected utility of choosing grapes over cookies can be expressed as:

$$E[U(G_i = 1) - U(G_i = 0)] = B_i(\tau_i, \bar{G}_{-i}(TP), \bar{I}_{-i}(TP), I) - C(I) \tag{1}$$

2.1 Direct Effect of Incentives

Consider first the direct effect of incentives, I on the incentivized person:

$$\frac{\partial E[U(G_i = 1) - U(G_i = 0)]}{\partial I} = \frac{\partial B_i}{\partial I} - \frac{\partial C}{\partial I} \tag{2}$$

The first right-hand side term is the effect of introducing (or increasing) the incentive on the child's belief on the relative value of grapes over cookies. The sign of this effect is indeterminate. For example, in Benabou and Tirole (2003), being incentivized (or having a higher-valued incentive) signals "bad news" – e.g., that the action is unpleasant, perhaps because grapes do not taste as good as cookies.[7] This may make her revise her prior beliefs about the benefits downward. On the other hand, the incentive may signal that the experimenter thinks grapes are really good for the child (maybe despite not tasting as good as the cookie), inducing her to revise her prior belief of the benefits of grapes upward.

Conversely, the second right-hand side term, which represents the effect of the incentives on cost, is always positive, as compensating the child to pick grapes over cookies reduces its cost.

In sum, the sign of the direct effect is unknown, partly due to the ambiguity of the sign of $\frac{\partial B_i}{\partial I}$.

[6] The benefits of choosing grapes over cookies may also depend on social conformity, as we discuss in Section 8. Our empirical results, detailed in that section, point against social conformity as an explanation for the behaviors we find, and, thus, we exclude it from the description of the model below.

[7] In Benabou and Tirole (2003), a principal has private information about attractiveness of an action and may offer larger incentives for less attractive tasks. The agent, therefore, expects larger incentives to signal more unpleasant tasks and may be less motivated to do it.

2.2 Spillover Effects of Incentives

Now consider the effect of the fraction incentivized. This is what we term the spillover effect. To do that, consider an increase in the proportion of children who are incentivized to pick grapes, TP, which affects the fraction of her peers who initially choose grapes over cookies, $\bar{G}_{-i}$, and who initially choose incentivized grapes, $\bar{I}_{-i}$.

$$\frac{\partial E[U(G_i = 1) - U(G_i = 0)]}{\partial TP} = \frac{\partial B_i}{\partial \bar{G}_{-i}} \frac{\partial \bar{G}_{-i}}{\partial TP} + \frac{\partial B_i}{\partial \bar{I}_{-i}} \frac{\partial \bar{I}_{-i}}{\partial TP} \tag{3}$$

The first right-hand side term is the spillover effect of incentives through watching others pick grapes and has an indeterminate sign. The sign of $\frac{\partial B_i}{\partial \bar{G}_{-i}}$ is positive, if an increase in the proportion picking grapes sends a positive signal about the value of grapes. Therefore, the sign of this term depends on how increasing the proportion incentivized affects the proportion picking grapes initially, $\frac{\partial \bar{G}_{-i}}{\partial TP}$. This is the same as the direct effect of incentives because initial choices are private (no inference from peers).[8]

The second right-hand side term is the spillover effect of incentives through watching others pick incentivized grapes. It has an indeterminate sign because the signs of its two parts are both indeterminate. The sign of $\frac{\partial B_i}{\partial \bar{I}_{-i}}$, depends on how children interpret the experimenter's intent to incentivize children to pick grapes, while the sign of the second part is also the same as the direct effect of incentives. $\frac{\partial \bar{I}_{-i}}{\partial TP}$ is positive, as an increase in the proportion picking grapes sends a positive signal about the value of grapes. Therefore, the sign of this term depends on how increasing the proportion incentivized affects the proportion picking grapes, $\frac{\partial \bar{G}_{-i}}{\partial TP}$.

There are, therefore, the following 3 cases, also summarized in table 1:

Case 1: Incentives send a weakly positive signal on the value of grapes ($\frac{\partial B_i}{\partial I} \geq 0$). When this happens, the direct effect of incentives is positive, as $\frac{\partial B_i}{\partial I} - \frac{\partial C(I)}{\partial I} > 0$. If the direct effect is positive, then increasing the proportion incentivized increases the proportion choosing grapes, incentivized or not, ($\frac{\partial \bar{G}_{-i}}{\partial TP} > 0$ and $\frac{\partial \bar{I}_{-i}}{\partial TP} > 0$). Moreover, if the incentive sends a weakly positive signal on value of grapes, then the belief of the value of grapes grows with the proportion of children choosing incentivized grapes, $\frac{\partial B_i}{\partial \bar{I}_{-i}} \geq 0$, and, therefore, the spillover effect of incentives is also positive. That is, in this case the spillover effects reinforce the direct effects.

Case 2: Incentives send a negative signal on the value of grapes ($\frac{\partial B_i}{\partial I} < 0$), but the direct effect is positive, because the cost reduction more than offsets the negative signal for incentivized

[8] The sign of $\frac{\partial B_i}{\partial \bar{G}_{-i}}$ can also be negative. We do not explicitly model this option because it would increase the number of possible cases, and thus lengthen the exposition, without adding to our main point that the direct and spillover effects may have opposite signs. Moreover, the sign of $\frac{\partial B_i}{\partial \bar{G}_{-i}}$ is positive in our data, so modelling this option is not essential in this application.

children, $\frac{\partial B_i}{\partial I} < \frac{\partial C(I)}{\partial I}$. If the incentive sends a negative signal on value of grapes, then the belief of the value of grapes decreases with the proportion of children choosing incentivized grapes, $\frac{\partial B_i}{\partial \bar{I}_{-i}} < 0$. Moreover, if the direct effect is positive, then increasing the proportion incentivized increases the proportion choosing grapes, incentivized or not, ($\frac{\partial \bar{G}_{-i}}{\partial TP} > 0$ and $\frac{\partial \bar{I}_{-i}}{\partial TP} > 0$). Because of these opposite effects, the sign of the spillover effect is indeterminate: the first term is positive, the second negative, and the two effects of conformity are both positive. That is, in this case the spillover effects may either reinforce or offset the direct effects.

Case 3: Incentives send a negative signal on the value of grapes ($\frac{\partial B_i}{\partial I} < 0$) and the direct effect is negative, because the cost reduction is offset by the negative signal for incentivized children, $\frac{\partial B_i}{\partial I} > \frac{\partial C(I)}{\partial I}$. If the incentive sends a negative signal on value of grapes, then the belief of the value of grapes decreases with the proportion of children choosing incentivized grapes, $\frac{\partial B_i}{\partial \bar{I}_{-i}} < 0$. Moreover, if the direct effect is negative, then increasing the proportion incentivized reduces the proportion choosing grapes, incentivized or not, ($\frac{\partial \bar{G}_{-i}}{\partial TP} < 0$ and $\frac{\partial \bar{I}_{-i}}{\partial TP} < 0$). Because of these effects, the sign of the spillover effect is indeterminate: the first term is negative, the second positive, and the two effects of conformity are both negative. That is, in this case the spillover effects may either reinforce or offset the direct effects.

In sum, we have the following 4 broad conclusions. First, we have shown that incentives may have both a positive and a negative direct effect.[9]

Second, when incentives are "bad news" ($\frac{\partial B_i}{\partial I} < 0$), the spillover effects of incentives have both a positive and negative component.

Third, when incentives are "bad news," the direct and spillover effects of incentives may offset each other. In the empirical setting sketched above, the direct effect may be a poor approximation of the *overall* effect of incentives, or even an informative bound of the true overall effect.

Fourth, consider the case in which the direct effect of incentives is positive. In this case, we can identify whether incentives are "bad news" by estimating the signs of the two separate spillover effects of incentives.

Based on this model, we can use our experiment to identify the direct and spillover effects of incentives. The final grape choice, G_2, is the sum of the initial grape choice, G_1, and any later change in choice, ΔG: $G_2 = G_1 + \Delta G$. The overall effect of incentives on the final grape choice is the sum of the direct effect – that is, the effect of incentives on G_1 for incentivized children – and the spillover effect – the spillover effect of the choices of others on ΔG for incentivized and nonincentivized children. We proceed to describe how we identify and estimate the direct and

[9] In empirical settings such as ours, we cannot separately identify the positive and negative direct effects of incentive, as we observe only their sum.

spillover effects of incentives.

3 Background and Experimental Design

To measure the direct and spillover effects of incentives, we designed an artefactual field experiment (Harrison and List, 2004) in which we randomly vary both whether a child receives incentives to pick grapes and the proportion of her peers that is incentivized. This field experiment took place in school cafeterias during lunch. Nine elementary schools in Chicago Heights, Illinois participated. Lunch is administered in much the same way in each of these schools. Depending on their size, schools hold either 2, 3 or 4 lunch periods each day, assigning kids to periods based on their school grade. Children arrive for lunch during their designated lunch period together with their class. They go through a lunch line where they receive a school lunch and then sit at a table in the cafeteria. Except for kindergartners, students can typically sit with any other children from their grade and tend to form groups of 3-10 students at each table. In this school district children do not have a choice about their lunch. Moreover, Chicago Heights, Illinois is in a low-income neighborhood and most children qualify for free or reduced-price lunch, meaning that all kids eat the same school-provided meal each day.[10]

We conducted the experiment after children had collected their lunch trays and sat down to eat at their table. Once children were seated, members of the research team came to the table and read a script announcing the procedures of the experiment.[11] To minimize cross-table contamination, we treated adjacent tables simultaneously. Each child was asked to pick both a grape card (green on the back) and a cookie card (blue on the back) from a card deck (see Figure 1). To facilitate data collection, each child's ID number from the experiment was written on each of his or her cards. Then, each child made a choice: he or she could either choose to have grapes as an additional food (by placing the grape/green card down on the table), or he or she could choose to have cookies as an additional food (by placing the cookie/blue card down on the table). Children were told that they could only choose one snack, and that the actual food item they had selected would be delivered to their table immediately at the end of the experiment. The initial choice was always made simultaneously and children were asked not to talk during the experiment. This initial food choice, G_1, is our first variable of interest. We use it to measure the direct effect of incentives.

After the initial choice, children were given a chance to play a different card after having

[10]In 7 of the 9 schools, at least 98 percent of children qualify for free or reduced-price lunch. In the other two schools, 93 percent and 73 percent qualify.

[11]The script of the experiment is reported in Appendix A.

observed their peers' choices. The final food choice, G_2 is our second variable of interest. We use it to measure the spillover effect of incentives.

Our experiment on this snack choice involved 3 levels of randomization: 1) randomization of whether or not received an incentive to choose grapes, 2) randomization of the fraction of each lunch table received an incentive to select grapes, and 3) randomization of whether the offer of an incentive was public or private. This randomization took place at the table level. In particular, for each table, we had a stack of cards, which 1) had either private or public incentives and 2) had either 0% of cards with incentives, 50% of cards with incentives, and 100% of cards with incentives. As table size could not be predicted beforehand, at tables with the 50% of incentivized cards, the actual fraction of children receiving incentives differs from 50%. In practice this fraction incentivized varies between 11 and 80 percent. This variation is seen in Table 2. We will use some of this variation in the fraction incentivized to identify the spillover effects, but we will also present group-level means that do not exploit this variation (i.e., treat 50% incentivized tables all the same).

In all treatments, children were alerted to the possibility that they may be eligible for a prize depending on the card they draw, and a poster with all possible prizes was displayed to the kids. The value of the prizes was roughly 50 cents. The prizes included glow-in-the-dark bouncy balls, small trophies, and bracelets and pens of different types.

If students were eligible for an incentive, their grape card depicted a small gold token. For the 50 percent incentive treatment, the cards came from a deck where 50 percent of the grape cards portrayed a gold token. In the 100 percent incentives treatment, all the grapes cards depicted the coin.

In the private treatment, children play their cards face down, so that children can observe only the color of the card, but not the presence or absence of the incentives. In the public treatment, on the other hand, children play their cards face up, so that anyone at the table can observe whether the chosen grapes are incentivized or not.

With the three levels of randomization, we can divide children into eight groups:

- Private-0 (Group 1): Children in the Private treatment in which none of the grape cards were incentivized.
- Public-0 (Group 2): Children in the Public treatment in which none of the grape cards were incentivized.
- Private-50-no incentive (Group 3): Children in the Private treatment in which 50% of the grape cards were incentivized but the child's own card was not incentivized.

- Private-50-incentive (Group 4): Children in the Private treatment in which 50% of the grape cards were incentivized and the child's own card was incentivized.
- Public-50-no incentive (Group 5): Children in the Public treatment in which 50% of the grape cards were incentivized and the child's own card was not incentivized.
- Public-50-incentive (Group 6): Children in the Public treatment in which 50% of the grape cards were incentivized and the child's own card was incentivized.
- Private-100-incentive (Group 7): Children in the Private treatment in which all of the grape cards were incentivized.
- Public-100-incentive (Group 8): Children in the Public treatment in which all of the grape cards were incentivized.

We designed the experiment by randomizing each school-by-period table is such a way as to have one quarter of the school-by-period tables assigned to the 0% and 100% treatments each, and the remaining half to the the 50% treatment, cross randomizing the public and private treatment to have half of the school-by-period tables in each group.

4 The data

4.1 Sample

A total of 1,771 children participated in the experiment, conducted during lunch in the school cafeteria. Children choose a table to sit at. We drop 14 tables of 10 from the main analysis because we do not believe that kids can see all others' decisions at such a large table. The results in the next section are qualitatively unchanged if we include tables with 10 children.

We complement the experimental data with a short survey assessing the social networks of kids (available upon request). The survey included questions asking children to name up to 5 of their friends. The survey also included questions about each child's perceived social status relative to other children, and asked children to name the most popular kid boy and girl in their class. A total of 1,286 children filled out the questionnaire.

After dropping large tables, our final sample consists of 1,631 children, of whom 1,187 completed the questionnaire, sitting at 270 school-by-period tables.[12]

[12]Non-participation in the survey is also due to a number of reasons: either children were too young, or teachers overseeing the lunch period asked us not to administer the survey, or not enough time was available for all children to complete the survey.

The final size of each of the 8 groups varies because some of the tables in the cafeteria were empty. Moreover, in various instances we ran out of time and could not reach all the occupied tables.

4.2 Descriptives, balance tests, and food choice

Table 3 shows the mean and standard deviations of several socioeconomic variables for each of the eight groups. Lunch tables have on average 6.45 children of which 47 percent are boys. The average grade is fourth grade, 39% of children at each table are African American and 52% are Hispanic, and 87% of the children at each table are on the free lunch program. Overall, looking at Table 3 it appears that these variables are balanced across groups. This is confirmed in lower panel of Table 3, which shows the F-test of joint significance of the 8 group dummies, when regressed on each of these variables together with school-by-period strata. All variables are balanced across groups, consistent with random assignment, since none of the F-tests are significant at conventional levels.

We also check for balance using the actual fraction of student incentivized. Recall, while the grape cards for the 50% incentivized tables were drawn from a deck where half of the cards were incentivized, the actual fraction incentivized deviated from 50%. We regress the proportion of children incentivized at each table on table size and children's age, gender, race, grade, and school lunch status (free, reduced, or none), as well as on school-by-period strata. The F-test of joint significance of the coefficients of the socioeconomic variables has a p-value of 0.087. This is driven by a smaller table proportion incentivized for third and six graders. Once we exclude grade, the F-test of joint significance of the coefficients of the remaining socioeconomic variables has a p-value of 0.543. For this purpose, we control for these variables in all our specifications.

Our two main variables of interest are the initial and final choice of grapes. For the initial choice, the only systematic variation in grape choice should be due to whether or not the child was incentivized. At this point of the experiment, the choices of peers are not observed. A student's final choice will possibly be the combination of incentive and spillover effects. Table 4 shows that overall 62% of children choose grapes initially (combining the incentivized and non-incentivized groups). Not surprisingly, the proportion differs for incentivized and non-incentivized children. However, the p-value of the F-test of joint significance of the group dummies regressed on inital choice and controlling for being incentivized is 0.168, consistent with random assignment. The final snack choice does differ across each of the incentivized groups and separately across the non-incentivized groups. This is our first evidence of the presence of spillover effects, which we explore further in detail.

5 Direct effects of incentives

Our first task is to establish the effect of incentives on the recipients, that is, whether receiving the incentives changes the recipients' likelihood of initially choosing grapes. This is what we refer to as the direct effect of the incentives.

We test the hypothesis that incentives have direct effects by comparing the initial grape choice of incentivized and non-incentivized children. To do so, we regress child i's *initial* grape choice, G_1, on a dummy variable, I, that equals 1 for children who receive incentives. To improve the precision of the estimates, we further condition on the following variables X: school-by-period strata, table size, child age, gender, race, grade, and school lunch status, which we have already shown not to vary across treatment groups.

$$G_{1i} = \alpha_0 + \alpha_1 I_i + \alpha_2 X_i + \epsilon_i \tag{4}$$

The coefficient α_1 identifies the average treatment effect of incentives on initial grape choice. This parameter is identified under random assignment, which ensures independence between the variable I and the error term ϵ. We estimate the parameters of this equation by OLS, clustering the standard errors by table. We use the same estimator, controls, and clustering for all the regressions in this paper.

We can also interact the incentive dummy by a dummy for the public ($P = 1$) and private ($P = 0$) treatments:

$$G_{1i} = \lambda_0 + \lambda_1 I_i + \lambda_2 P_i + \lambda_3 I_i P_i + \lambda_4 X_i + \epsilon_i \tag{5}$$

This way, we can test i) whether the direct effects of incentives are identical in the public and private treatment ($\lambda_3 = 0$) and ii) whether the initial grape choice is identical in the public and private treatments for non-incentivized children ($\lambda_2 = 0$) and incentivized children ($\lambda_2 + \lambda_3 = 0$).

Table 5 shows the direct effects of incentives on the initial choice of incentivized children (the estimate of α_1 from equation (4)). This estimate is a 26 percentage point, statistically significant increase in initial grape take-up over a 49.5% take-up rate among non-incentivized children, i.e. a 53% increase in take-up.

These findings are comparable in size with Just and Price (2013), who increase children's consumption of salad by 80% after offering up to $0.25 (or a lottery ticket with the same expected value), and smaller, but consistent, with List and Samek (2014, 2015), whose incentives have a two- to four-fold increase in the choice of healthy snacks. Conversely, our effects are larger than the ones in Belot, Jonathan and Patrick (2013), whose piece-rate incentives to choose an extra vegetables side dish have a small, statistically insignificant effect.

Children's initial choice may depend on whether others can observe whether you were eligible for an incentive. Recall that in private treatment, the cards are played face down and, therefore, one can infer other children's choices from the color of the card, but not whether a child was incentivized or not. On the other hand, for the public treatment, the cards are played face up and the incentives can be observed. To test whether children behave differently when they know others can observe whether or not they are incentivized, the second row of Table 5 provides the estimate of the difference in effect sizes in the public and private treatment (the estimate of λ_2 from equation (5)). This difference is only 0.013 and is statistically insignificant.

The third and fourth rows of the tables show that the initial grape choice is 8.4 and 7.1 percentage point lower in public treatments for both non-incentivized and incentivized children (the estimates of λ_2 and $\lambda_2 + \lambda_3$ from equation (5)). As was evident in Table 4, this occurs in the 0, 50, and 100% treatments. That is, making incentives more salient, as the public treatment does, may deter some incentivized and non-incentivized children from initially picking grapes. This is broadly consistent with the views that incentives may signal "bad news" (e.g., Gneezy, Meier and Rey-Biel (2011)).[13]

6 Spillover effects of incentives

Finding positive direct effects of incentives corresponds to cases 1 and 2 from our theory: seeing peers choose grapes may have a positive spillover effect on own grape take-up, while seeing others choose incentivized grapes may have either positive or negative spillover effects. Armed with this knowledge, we proceed to test how, conditional on one's initial choice, seeing other children pick grapes or pick *incentivized* grapes affects a child's likelihood of ending up picking grapes.

Since spillovers affect the likelihood that a child may change the card played after seeing others, our dependent variable is the difference between the final and initial grape choice, $\Delta G = G_2 - G_1$. Therefore, we begin our analysis of spillover effects by estimating how exogenously varying the table proportion incentivized, $TP \in [0, 1]$, affects ΔG:

$$\Delta G_i = \beta_0 + \beta_1 TP_i + \beta_2 TP_i * P_i + \beta_3 I_i + \beta_4 P_i + \beta_5 I_i P_i + \beta_6 X_i + \epsilon_i \tag{6}$$

[13]Note, however, that we cannot separate out the effects of making incentives salient from the effect of playing the cards face up rather than down, which is the other major difference between the public and private treatments.

The dummy variable P equals 1 for the public treatment and 0 for the private treatment. We condition on being incentivized (I) and on the public treatment dummy because they affect the initial grape choice, which, in turn, affects the likelihood of ending up with grapes. We add the interaction with the public treatment dummy because we estimate different parameters for the two treatments.[14] While looking at the reduced-form effects of the proportion of the table incentivized provides the relevant policy parameters, theory suggests that the relevant explanatory variable is rather the incentivized table proportion *other than self*. In unreported results, we have used this latter measure of the proportion incentivized as our main regressor, and the results are qualitatively unchanged.

Under random assignment, the parameters β_1 identifies the marginal effect of the proportion of incentivized children at one's table. β_2 identifies the difference in the effect of this proportion between the public and private treatment. β_1 and β_2 are separate spillover effects. β_1 is the reduced-form impact of others' choices on one's own choice and β_2 is the reduced-form effect of observing whether or not others are incentivized on one's own choice.

Table 6 shows the estimates of the reduced form spillover effects of choosing grapes and of incentives (β_1 and β_2) from equation (6). Column 1 shows that the estimated marginal effect of increasing the proportion of students who are privately incentivized is 0.09 (s.e. 0.04) in the private treatment. That is, a 1% increase in the proportion privately incentivized increases the likelihood of switching to grapes by 0.09 percentage points. A positive effect in the private treatment, in which children can observe the food choices of others but not whether these choices are incentivized, suggests that watching other children pick grapes has a positive spillover effect on the likelihood of switching to grapes. Note, however, that the estimated effect is not statistically different from zero. The second row of estimates in column 1 shows that the effect of the proportion incentivized changes when the incentives are public. That is, relative to private incentives, the effect of making the choice of incentivized grapes public is -0.18 (s.e. 0.08). That is, a 1% increase in the proportion incentivized additionally decreases one's likelihood of switching to grapes by 0.18 percentage points. Therefore, the net spillover effect of public incentives (i.e., the effect from increasing the table proportion who is incentivized), in the third row, is negative ($-0.09 = 0.09 - 0.18$).

These marginal effects mask important nonlinearities. That is, the effect of the proportion of students who are incentivized does not have to be linear as specified in equation (6). To test this

[14]The results do not change whether we interact by public treatment or not, or whether we estimate the parameters of equation 6 or of equation $G_{2i} = \beta_0 + \beta_1 TP_i + \beta_2 TP_i * P_i + f(\beta_{IPG} I_i P_i G_{1i}) + \beta_3 X_i + \epsilon_i$, where the term $f(\beta_{IPG_1} I_i P_i G_{1i})$ is the triple interaction of the incentive treatment, public treatment, and initial grape choice dummies.

hypothesis, we restrict the sample to tables with a positive proportion of incentivized children (column 2) and with at least 50% of incentivized children (column 3), in which case the overall table proportion incentivized increases from 50% to 66% (column 2) and to 80% (column 3).[15] When we do that, we find that the two marginal effects become considerably larger (in absolute value), especially the negative effect of observing other children's incentivized choices. For this reason, we estimate equation (6), adding the square of the table proportion incentivized and interacting it with the public dummy: $\beta_4 TP_i^2 + \beta_5 TP_i^2 * P_i$. Figure 2 shows the marginal effects from this equation. If the effects were linear, each of those graphs would depict a straight line, which they do not. The figure confirms that the marginal effects grow in absolute level with the proportion incentivized. The marginal effects become statistically different from zero when 40 to 50 percent of the table is incentivized.

A more non-parametric method of examining how the effects vary with the level of TP involving no extrapolation is to look at the group-level means. Appendix B shows the results from grouping tables depending on whether on average 0, 50, or 100% of the children are incentivized. The results are consistent with the findings we reported above.

6.1 Heterogeneity in the spillover effects

In this subsection, we consider how the effects can differ along several dimensions: i) the type of switch - to grapes or to cookies, ii) gender and school grade, and iii) table size.

First, recall that the parameter ΔG is the difference between switching from cookie to grapes, SG, and from grapes to cookie, SC: $\Delta G = SG - SC$. To have a better understanding of how spillover effects work in our setting, Table 7 considers both the separate choices of switching from cookies to grapes and from grapes to cookies. We also examine how these effects vary across incentivized and non-incentivized children. The estimated marginal effects are consistent with the main results: the likelihood of switching to grapes increases with the proportion choosing grapes (i.e., $\widehat{\beta}_1$ is positive) and decreases with the proportion choosing incentivized grapes (i.e., $\widehat{\beta}_2$ is negative). The opposite is true for the likelihood of switching to cookies. The primary action is on the dimension of switching to grapes and not switching to cookies, as expected.

Second, we explored how the direct and spillover effects of incentives differ by gender and grade. We discuss this in Appendix C. In short, the direct effects of incentives do not differ by gender, but the spillover effects of others' behaviors is stronger for females and the negative spillover effect of observing others' incentivized choices is more substantial for boys. The direct

[15]The sample restriction in column 2 drops the first quartile of the fraction incentivized distribution, and the sample restriction in column 3 drops the first and second quartiles of the fraction incentivized distribution.

and spillover effects do not differ significantly across grade.

Third, a priori one might expect that the effects would differ across table size. For example, there may be less interaction at a larger table. We divide the sample into two - above and below median table size. We note that table size and proportion incentivized are uncorrelated (the correlation coefficient is 0.005). There is no systematic difference by table size (results available upon request).

7 Non-linear effects and maximizing take-up

Recall that the total effect of incentives on the final grape choice is the sum of the net direct effect on the initial choice, G_1, which is positive, and the two spillover effects on changing choice, ΔG, which we found to be one positive and the other negative. We can now compare the estimates of the direct (Table 5) and spillover (Table 6) effects, as well as compute their sum, which is the overall effect of incentives.

A 1 percent increase in the proportion of incentivized children has a direct effect on the likelihood of choosing grapes of 0.26 percentage points (p-value of 0.000) and two spillover effects. First, observing other people choosing grapes in the private treatment has a positive effect on one's likelihood of ending up with grapes. A 1 percent increase in the proportion incentivized to pick grapes further increases one's likelihood of ending up with grapes by 0.09, 0.12, and 0.16 percentage points when the proportion of children incentivized are 50, 66, and 80%.[16] Therefore, the fraction incentivized that maximizes the likelihood of ending up with grapes in the private treatment is 100%, as both the direct and spillover effects of incentives are positive over all ranges of the fraction incentivized. However, the effects of this treatment are likely to have limited policy relevance. That is, in most environments, the knowledge that one's peers are being incentivized would likely diffuse. Therefore, the public, rather than the private treatment, is likely to be more realistic in a real world policy situation.

In the public treatment, besides the positive spillover effect discussed above, observing that some of the chosen grapes are incentivized has an additional negative effect on the likelihood of ending up with grapes. The corresponding point estimates are -0.18, -0.22, and -0.45 percentage points when the proportion of children incentivized are 50, 66, and 80%. Using the point estimates above, we find that a 1 percent increase in the proportion incentivized overall increases in grapes take-up of 0.17 (0.26+0.09-0.18) and 0.16 (0.26+0.12-0.22) percentage points when the mean incentivized is 50% and 66%, but reduces grapes take-up by -0.03 (0.26+0.16-0.45)

[16] These cutoffs denote the mean incentivized, the threshold for the top 3 quartiles, and the threshold for the top 2 quartiles.

percentage points when the mean incentivized is 80%. Therefore, the incentive share that maximizes grape take-up is between 66 and 80% in our experiment.

We can see these total effects (i.e., the combined direct and spillover effects) graphically in Figure 3. This figure plots the semiparametric total effect of proportion table incentivized on the likelihood of ending up with grapes for the private and public treatments.[17] While the total effect of incentives grows with the table proportion incentivized throughout the support in the private treatment, this effect is non-monotonic in the public treatment. The total effect of incentives grows with proportion incentivized up until this proportion is about 70%, and declines for higher proportions to the extent that there is no statistically significant difference in final grapes take-up between tables with 0 and 100% incentives, as we had also shown at the beginning of this empirical analysis.

Again, since these total effect figures rely on extrapolation, we can also examine group means. We do this in Figure 4, which reiterates our main findings that the final grape take-up increases with the proportion of incentivized children in the *private* treatment but not in the *public* treatment.[18]

In sum, despite finding a positive net direct effect of incentives, we find that, when incentives are public, their spillover effect is negative, because the positive effect of observing other people choosing grapes is more than offset by the negative effect of observing that some of those choices are incentivized. Since the negative effect of incentives grows with the proportion of children incentivized, the level of incentive that maximizes the proportion of children choosing grapes is less than 100%.

8 Interpretation

There are multiple channels through which spillover effects may occur. Two possible mechanisms for the positive spillover effects are learning and social conformity. The latter occurs if children derive utility from conforming to their peers' behavior (Sherif, 1937; Asch, 1958; Goeree and Yariv, 2014; Haun, Rekers and Tomasello, 2014). For example, consider the theory discussed earlier, in which we modelled the choice of grapes or cookies for child i in a setting

[17]To do so, we use the Robinson's semiparametric estimator (Robinson, 1988) to control for the effect of the covariates and then smooth the effect of incentive proportion on final grape choice using a local linear regression with a Gaussian kernel and a rule-of-thumb bandwidth. The results are robust to changes in the kernel and bandwidth. We report both the 83 and the 95% confidence intervals. If the 83% confidence intervals around two point estimates do not overlap, the parameters are statistically different from each other at the 95% level (Peyton, Greenstone and Schenker, 2003).

[18]This Figure shows the difference, conditional on baseline covariates, in the fraction ending up choosing grapes between each group and the fraction choosing grapes in group 1 (i.e., private treatment in which nobody is incentivized), which is 0.64.

with asymmetric information. Here we augment the model to include a social conformity term in the utility function. As before, the child decides to pick grapes over cookies if the expected private benefits of this choice exceed its costs. In addition, the child may also derive utility from conforming to her peers' behavior (Sherif, 1937; Asch, 1958; Goeree and Yariv, 2014; Haun, Rekers and Tomasello, 2014). In that case, her expected utility depends also on a social conformity parameter γ, which is positive.

With social conformity, the expected utility of choosing grapes over cookies becomes:

$$E[U(G_i = 1) - U(G_i = 0)] = B_i(\tau_i, \bar{G}_{-i}(TP), \bar{I}_{-i}(TP), I) + \gamma_i(\bar{G}_{-i}(TP), \bar{I}_{-i}(TP)) - C(I) \quad (7)$$

While the direct effect of incentives is as already described, the spillover effects of incentives, which affect the final choice of grapes, G_2, depends also on conformity, the third and fourth right-hand-side terms in the equations below:

$$\frac{\partial E[U(G_{2i} = 1) - U(G_{2i} = 0)]}{\partial TP} = \frac{\partial B_i}{\partial \bar{G}_{-i}}\frac{\partial \bar{G}_{-i}}{\partial TP} + \frac{\partial B_i}{\partial \bar{I}_{-i}}\frac{\partial \bar{I}_{-i}}{\partial TP} + \frac{\partial \gamma_i}{\partial \bar{G}_{-i}}\frac{\partial \bar{G}_{-i}}{\partial TP} + \frac{\partial \gamma_i}{\partial \bar{I}_{-i}}\frac{\partial \bar{I}_{-i}}{\partial TP} \quad (8)$$

Children may conform to picking grapes, and, if they are incentivized, also to picking incentivized grapes. Its first parts, $\frac{\partial \gamma_i}{\partial \bar{I}_{-i}}$ and $\frac{\partial \gamma_i}{\partial \bar{I}_{-i}}$, are positive, as children have a taste for conformity. Its second parts, as we saw, have the same sign as the direct effect. Therefore, the spillover effects of incentives from conformity have always the same sign as the direct effects of incentives and do not change the three general cases we considered earlier.

Since incentives increase initial grape take-up, the higher the initial take-up, the more children will want to conform, ending up picking grapes too. That is, conformity could explain why there are positive spillover effects in the private treatment. On the other hand, conformity cannot explain the negative spillover caused by seeing children pick incentivized grapes. Therefore, even if children have a taste for conformity, receiving a negative signal about the benefits of grapes over cookies more than dominates the effect deriving from a preference for conformity.

While conformity cannot explain our findings, we can nevertheless test specific aspects of social conformity and see to what extent it affects children's behavior. One way to test for conformity is to exploit the data collected on best friends, "popular kids," and the table gender composition.[19] This test is based on the premise that children want to conform the most to their best friends, to children they perceive as being popular, and to children of their own gender. If that is the case, the choices of best friends, popular kids, and children of own gender should affect ones' choice beyond the effect of the table's choices as a whole. Conversely, if the choices

[19] Children report the names of up to 5 best friends and of the boy and girl they consider most popular.

of others signal the experimenter's or the peers' private information, then children will aggregate them (unless they believe some children have better signals than others).

Under social conformity, seeing one's best friends (or popular kids, or children of own gender) pick grapes has a positive spillover effect over and above the effect of the proportion of children incentivized at the table, as the behavior of the best friend (or popular kid, or child of own gender) induces more conformity than the behavior of the other children at the table. In addition, seeing one's best friends (or popular kids, or children of own gender) pick the incentivized grape has an additional positive spillover effect for incentivized children.

To estimate these effects, we focus on children with at least one best friend (or popular kid, or child of own gender) sitting at their table. Because of our experiment, whether the best friend (or popular kid, or child of own gender) is incentivized is random.

To measure the spillover effect of social conformity in picking grapes, we estimate the parameters of the spillover effect equation, equation (6), adding variables for the table proportions of best friends (or popular kids, or children of own gender) incentivized:[20]

$$\Delta G_i = \delta_0 + \delta_1 TP_i + \delta_2 TP_i P_i + \delta_3 TP_i^{BF} + \delta_4 TP_i^{BF} P_i + \delta_5 I_i + \delta_6 P_i + \delta_7 I_i P_i + \delta_8 X_i + \epsilon_i, \quad (9)$$

where the variable TP^{BF} is the table proportion of incentivized best friends (or popular kids, or children of own gender), while the other variables are as discussed before.[21,22] Under social conformity of the type described above, the parameter δ_3 is positive.

To measure the additional spillover effects of social conformity due to picking incentivized grapes, we further interact the variable TP^{BF} by the child's incentive status, $TP^{BF} * I$:

$$\begin{aligned} \Delta G_i = {} & \theta_0 + \theta_1 TP_i + \theta_2 TP_i P_i + \theta_3 TP_i^{BF} + \theta_4 TP_i^{BF} P_i + \theta_5 TP_i^{BF} I_i + \theta_6 TP_i^{BF} I_i P_i \\ & + \theta_7 I_i + \theta_8 P_i + \theta_9 I_i P_i + \theta_{10} X_i + \epsilon_i \end{aligned} \quad (10)$$

Under social conformity of the type described above, the parameter θ_6 is positive.

Table 8 reports the estimates from these regressions, using, alternatively, the entire sample,

[20]Before doing that, we checked whether spillover effects vary for children who did not name any best friend or popular kid, for children who did not fill in the questionnaire, and for children without kids of the same gender sitting at the table. The effects for these subgroups do not differ statistically from the main effects. So the fact that we are dropping these children from the regressions may not be as disconcerting.

[21]For example, if in a table of size 5 there are two child j's best friends, and one of them is incentivized, for child j the table proportion of incentivized best friends is $\frac{1}{5} = 0.2$.

[22]The variable TP is the table proportion of incentivized children, which varies from 0 to 100%, and the dummy variable P equals 1 for the public treatment and 0 for the private treatment, in which the cards are played face down and, therefore, one can infer other children's choices from the color of the card, but not whether a child was incentivized or not, and one for public treatment, in which the cards are played face up and the incentives can be observed. The X variables are as defined before.

only tables with at least one incentivized child, and tables in which at least half the children are incentivized. This table shows that none of the estimates of the parameters of interest are statistically significant and that many have also a negative, rather than a positive sign. We interpret this evidence as being inconsistent with a theory of social conformity in which the children have preferences for conforming with their best friends, with the children they perceive as being popular, or with children of their own gender.

The children's behavior, and in particular the negative spillover effects, do not seem to be driven by a form of envy by which non-incentivized children would initially pick grapes, observe the incentive status of their peers, and then switch to cookies. We rule this out by noticing that the largest negative spillover effects occur in tables in which everybody is incentivized.

9 Discussion

Our paper shows that negative spillover effects can trump the strong positive direct effect of incentives, leading incentives being potentially ineffective overall. The intuition is that observing that one's peers are incentivized to undertake a certain action may signal that the action has low value. If this induces a sufficiently large number of people to not undertake the incentivized action, the net effect of the incentive may be lower than its direct effect and, as in our case, even null.

We provide evidence consistent with this hypothesis in the context of food choice in elementary schools. We incentivize different fractions of children sitting at the same table to choose grapes over cookies. We let children first make a simultaneous choice and then change their mind after observing the choices of others. While incentives increase the likelihood of initially choosing grapes, we find that this has a net indirect negative effect on the likelihood of ending up with grapes, when peers' initial incentivized choice is observed. This negative effect grows with the fraction incentivized and eventually is large enough that a further increase in incentives reduces the overall likelihood of ending up with grapes.

Our findings are consistent with the evidence from Fischer et al. (2014), who find that the demand for various products in Uganda decreases when the products is initially offered with a full-price incentive (the products are offered for free) than after a sale distribution at full price. This is consistent with our finding that incentives send a negative signal about the value of the incentivized good.

Lastly, our findings suggest that the Physician Payment Sunshine Act, which requires drugs manufacturers to disclose certain payments to physicians, may reduce the demand for drugs from the paying manufacturers, if the payments are perceived to signal that the drugs of a given manufacturer are not as effective as others.

Our research findings also have implications for policy and practice. The main policy implications are that, while there is an increasing interest in using incentives to drive behavior change (partly because of the effectiveness of incentives observed in related work), the potential negative peer effects of incentives ought not to be overlooked. Moreover, in an environment with the potential of negative spillover effects, incentives should not be made salient, if possible.

The main implications for the identification and estimation of spillover effects is that randomized control trials should be designed to measure spillover effects. Second, given that the spillover effects of incentives may be non-linear in the fraction incentivized, it may be difficult to extrapolate results from trials where some fraction of the population is eligible for incentives to policy interventions where everyone may be eligible.

While our research focused specifically on providing incentives in a school lunchroom setting, the spillover effects of incentives may be considered in broader settings. In particular, we propose that the following activities may be at risk for negative peer effects, if publicly incentivized: (i) the adoption of new technology, as the value of the incentivized action is not well known; (ii) activities to improve health and human capital, which have short-run costs but long-run benefits; (iii) pro-social behaviors, whose 'warm-glow' value exceeds the monetary value of incentives; and (iv) interesting or pleasant activities, where incentives may reduce interest in the task. More work is needed to explore the spillover effects of incentives in these disparate settings.

A Experimental instructions

We are going to play a choice game where you can win these fun prizes!

(Point to the prizes)

Each of you gets two cards. Keep your cards a secret. You cannot trade cards.

One of your cards will be a cookie card and one of them will be a grape card. The game is to play one of these cards face up (down) on the table.

If you play a cookie card, you get a cookie. If you play a grape card, you get some grapes.

(Point to grapes and cookies)

After you play your card, you will have 20 seconds to change your mind. You may look at what your neighbors played. After 20 seconds, you cannot change your choice!

Some of the grape cards might have gold tokens on them. If you get a card with a gold token on it and you play it, you get a prize with your grapes! Here are the prize choices.

(Point to prize board)

You get your prize at the end of the game.

Ok, let me ask everyone a few questions to make sure we all know how to play.

(Have students say out loud answers, and always correct at the end: either, “Yes, each person gets 2 cards” or “No, each person gets 2 cards” and “Yes, if you play a grape card you get grapes’)

1. How many cards does each person get? (answer is 2, one cookie one grape)
2. How many cards can each person play? (answer is 1 only)
3. How do you play a card? (answer is put it on the table)
4. What happens if you play a cookie card? (you get a cookie)
5. What happens if you play a grape card? (you get grapes)
6. What happens if you play a grape card with a token? (you get grapes plus a prize)

Good job! Let's play!

Here are your cards. Remember to keep them hidden.

(Wait 10 seconds)

Choose the card you are going to play now. Remember if you play a card you should put it on the table face UP (DOWN) like this (demonstrate).

(Wait for children to play their cards)

Ok is that your final choice? You can change your mind if you want to.

(Wait exactly 20 seconds)

Ok, the game is over, you can't change your choice now.

Everyone who played a card with a token on it will get a prize sheet, please fill it out to claim your prize.

B Spillover effects: comparing group means

An alternative method of understanding the spillover effects can be done via group mean comparisons.That is, if we estimate the parameters of the following regression,

$$Y_i = \mu_1 + \mu_2 Group2 + \mu_3 Group3 + \mu_4 Group4 + \mu_5 Group5 + \mu_6 Group6 + \mu_7 Group7 + \mu_8 Group8 + \epsilon_i, \tag{11}$$

where $Y = \Delta G, SG(switchtograpes), SC(switchtocookie)$, and the group dummies are listed at the foot of this page,[23] we can test the hypothesis that ΔG and SG grow with the table proportion incentivized in the private treatment for non-incentivized ($\mu_3 > 0$) and incentivized children ($\mu_7 > \mu_4 > 0$). If the spillover effect of seeing children pick incentivized grapes is negative, then ΔG grows with the table proportion incentivized less in the public treatment

[23]Group 1: Children in the Private treatment in which none of the grape cards were incentivized. Group 2: Children in the Public treatment in which none of the grape cards were incentivized. Group 3: Children in the Private treatment in which 50% of the grape cards were incentivized but the child's own card was not incentivized. Group 4: Children in the Private treatment in which 50% of the grape cards were incentivized and the child's own card was incentivized. Group 5: Children in the Public treatment in which 50% of the grape cards were incentivized and the child's own card was incentivized. Group 6: Children in the Public treatment in which 50% of the grape cards were incentivized and the child's own card was incentivized. Group 7: Children in the Private treatment in which all of the grape cards were incentivized. Group 8: Children in the Public treatment in which all of the grape cards were incentivized.

than in the private treatment ($\mu_3 > \mu_5$, $\mu_4 > \mu_6$, and $\mu_7 > \mu_8$), and opposite signs for the variable SC.

Table B1 shows the means of ΔG for each group (top panel) and tests our hypotheses on spillover effects (bottom panels) using the estimates from the equation above. The top panel shows two main findings. First, ΔG is generally lower among incentivized than non-incentivized children. This may occur because compliers, children who picked grapes because they were incentivized but would not have picked them otherwise, are most likely to change their mind after seeing peers' choices. Consistent with this idea, in unreported regressions we find that in private treatments in which 50% of the tables are incentivized (groups 3 and 4), there is a negative correlation between non-incentivized children's (group 3's) initial grape choice and incentivized children's (group 4's) ΔG. The second main finding from the top panel is that there is a large difference in spillover effects in groups 7 and 8, the private and public group in which everybody is incentivized. Specifically, ΔG is 10 percentage points lower in the public treatment.

To understand this finding within our theoretical framework, we look at the bottom panels. Recall that, to be consistent with our hypotheses, the signs of the parameter estimates should be positive in the first two columns and negative in the third one. While we find no evidence for positive spillover effects when 50% of the table is incentivized, as our estimates of μ_3, μ_4, and μ_7 are not positive, we do find that the positive spillover effects are larger in the 100% than the 50% private treatments for incentivized children, as $\mu_7 > \mu_4$, consistent with our hypotheses. Similarly, we do not find differential spillover effects of incentives between the public and private treatment in the 50% tables, as we cannot reject the hypothesis that $\mu_5 = \mu_3$ and $\mu_6 = \mu_4$. We do find differential effects in the 100% tables, as the spillover effects of incentives are 10 percentage points lower in the public than private treatment, that is $\mu_8 < \mu_7$. In sum, we detect both positive and negative spillover effects from this group mean analysis as we did in our main analysis but only in the 100% treatment tables.

C Heterogeneity by child and peer gender and age

We test whether the effects of incentives differ by gender and age, proxied by school grade, as has been found in different contexts, as we discuss below. While Table C1 shows that we do not detect any gender or age difference in the direct effect of incentives, Table C2 show that spillover effects vary by gender. Specifically, the positive effects of seeing others choose grapes is stronger for girls, while the negative effects of seeing the incentivized choices of others is stronger for boys. When we pool the two effects we find that the overall spillover effect of incentives is statistically more negative (and grows faster) for boys than for girls. The magnitude of this difference is large, with the net effect for boys being at least twice as large (in absolute value) as the effect for girls. This gender differences in the response to incentives has been also found in other contexts (e.g., Angrist and Lavy (2009); Angrist, Lang and Oreopoulos (2009); Croson and Gneezy (2009); Dohmen and Falk (2011). However, this finding is not consistent in the literature (as, e.g., neither Royer, Stehr and Sydnor (forthcoming) nor Lacetera, Macis and Slonim (2013) find evidence of gender differences in the effects of incentives for pro-social or healthful behavior).

In addition, Table C3 show weak evidence that the spillover effects of incentives are stronger for younger children: the point estimates of the positive and negative spillover effects are closer to zero for children in above median grades than for younger children. However, the differences between these two groups are not statistically significant.

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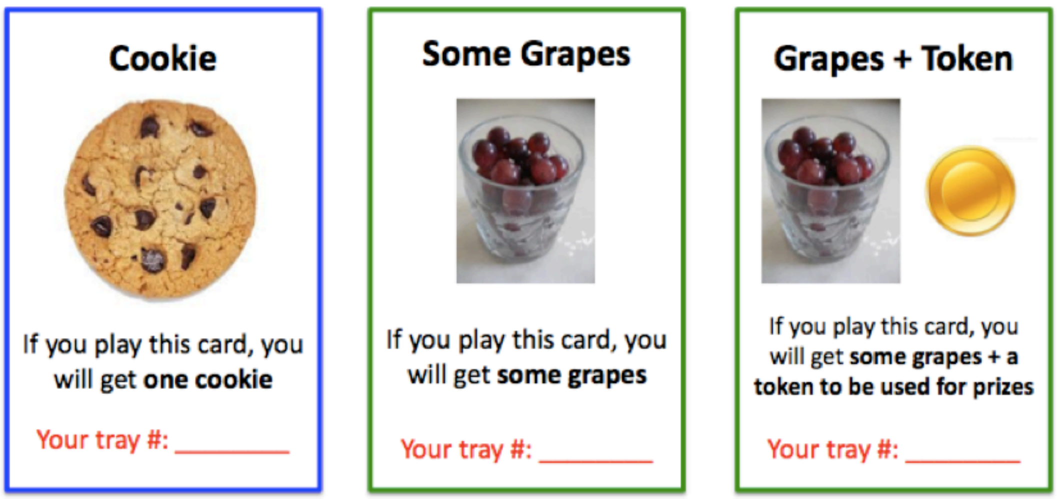

Figure 1: Cookie card, fruit card, and fruit card with token.

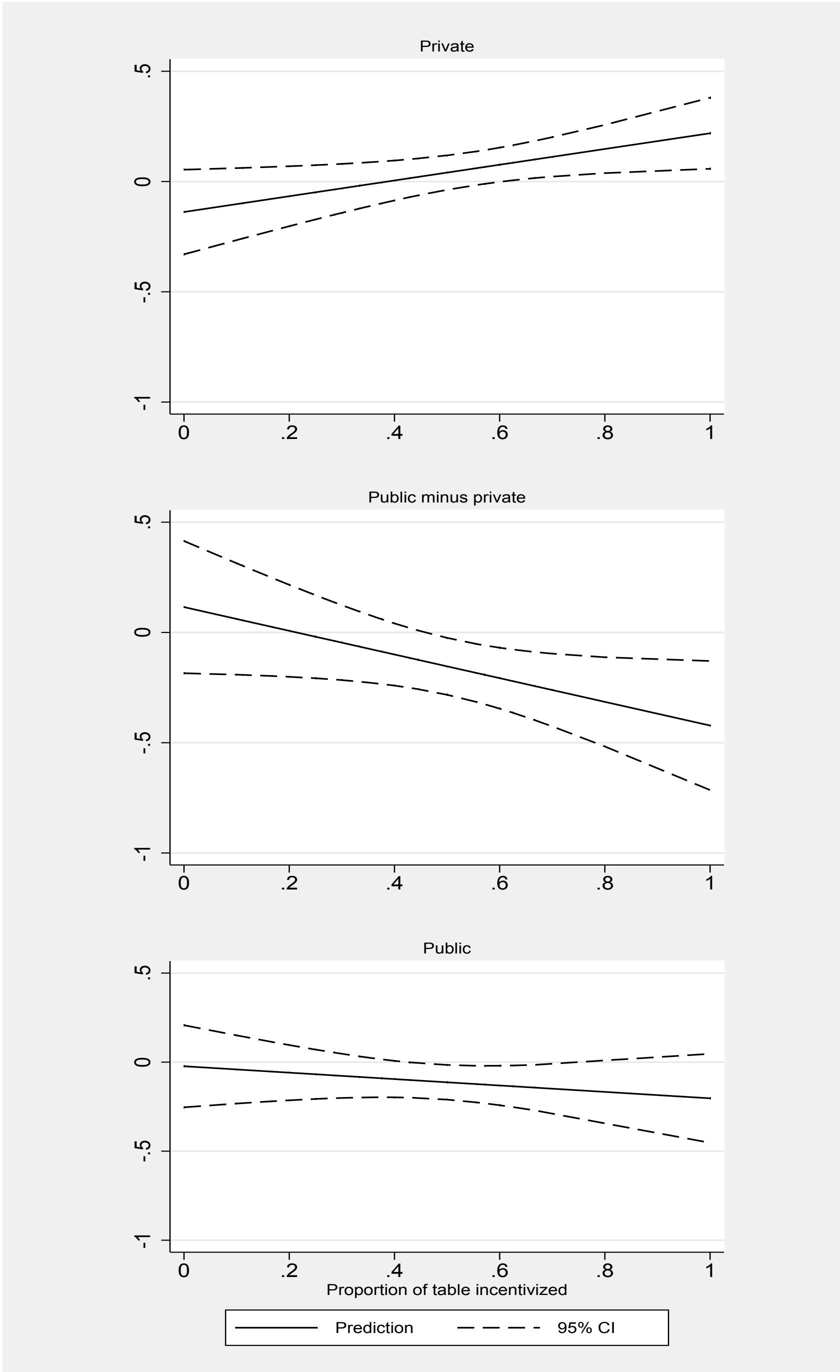

Figure 2: Marginal spillover effects of proportion table incentivized on the conditional likelihood of ending up with grapes. Estimates from a quadratic version of equation (6)

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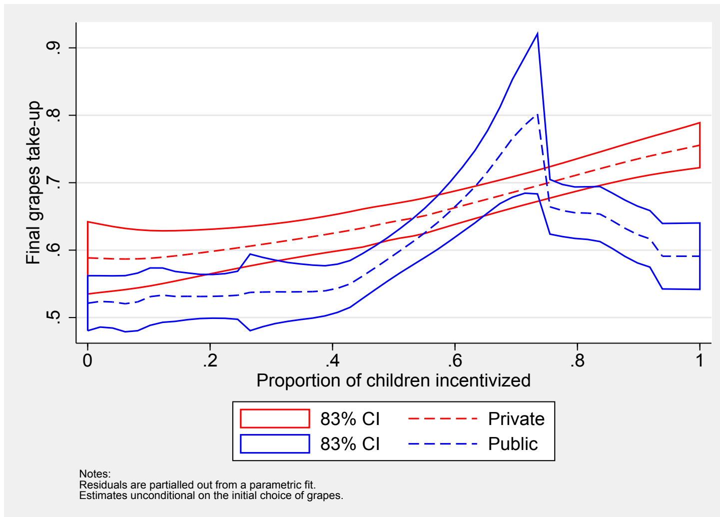

Figure 3: Net effect (direct and spillover) of proportion incentivized on final grapes take-up G_2

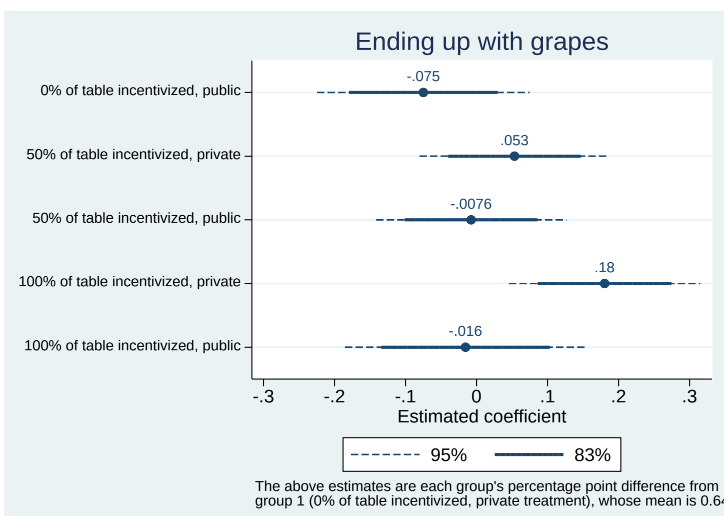

Figure 4: Ending up with grapes, differences across groups

Table 1: Direct, Spillover, and Overall Effects of Incentives

	Sign of direct effect:	Spillover effect: $(\frac{\partial B_i}{\partial \bar{G}_{-i}} \times \frac{\partial \bar{G}_{-i}}{\partial TP})+$	$(\frac{\partial B_i}{\partial \bar{I}_{-i}} \times \frac{\partial \bar{I}_{-i}}{\partial TP})$	Sign of spillover effect:	Sign of overall effect:
Case 1: $\frac{\partial B_i}{\partial I} > 0$ **and direct effect** > 0					
Signs, case 1:	(+)	$(+ \times +)$ +	$(+ \times +)$ =	(+)	(+)
Case 2: $\frac{\partial B_i}{\partial I} < 0$ **and direct effect** > 0					
Signs, case 2:	(+)	$(+ \times +)$ +	$(- \times +)$ =	(+ or -)	(+ or -)
Case 3: $\frac{\partial B_i}{\partial I} < 0$ **and direct effect** < 0					
Signs, case 3:	(-)	$(+ \times -)$ +	$(- \times -)$ =	(+ or -)	(+ or -)

Table 2: Table proportion incentivized - distribution by child incentive status

	Observations by child type:	
Table proportion incentivized	Incentivized	Not incentivized
0.000	398	0
0.111	8	1
0.167	15	3
0.200	16	4
0.250	15	5
0.286	15	6
0.333	38	19
0.375	50	30
0.400	33	22
0.429	36	27
0.444	10	8
0.500	60	60
0.556	8	10
0.571	39	52
0.600	16	24
0.625	24	40
0.667	25	50
0.800	1	4
1.000	0	459
Total observations:	807	824

Table 3: Descriptive statistics by group

Group	N. obs	Table size	% of boys	Grade	Black	Hispanic	Free lunch[a]
Private-0	130	6.23	0.34	3.86	0.39	0.55	0.92
		[1.37]	[0.48]	[2.37]	[0.49]	[0.5]	[0.28]
Public-0	268	6.51	0.47	4.07	0.34	0.58	0.87
		[1.53]	[0.5]	[1.95]	[0.47]	[0.49]	[0.34]
Private-50-no incentive	171	6.66	0.48	4.12	0.45	0.44	0.89
		[1.84]	[0.5]	[2.45]	[0.5]	[0.5]	[0.32]
Private-50-incentive	159	6.74	0.46	4.26	0.38	0.49	0.84
		[1.71]	[0.5]	[2.42]	[0.49]	[0.5]	[0.37]
Public-50-no incentive	238	6.42	0.47	3.68	0.39	0.48	0.83
		[1.3]	[0.5]	[2.58]	[0.49]	[0.5]	[0.38]
Public-50-incentive	206	6.51	0.47	3.69	0.37	0.49	0.85
		[1.26]	[0.5]	[2.66]	[0.48]	[0.5]	[0.36]
Private-100-incentive	288	6.49	0.52	4.27	0.41	0.54	0.9
		[1.44]	[0.5]	[2.28]	[0.49]	[0.5]	[0.31]
Public-100-incentive	171	5.98	0.49	3.83	0.39	0.56	0.89
		[1.66]	[0.5]	[2.6]	[0.49]	[0.5]	[0.31]
Total	1631	6.45	0.47	3.99	0.39	0.52	0.87
		[1.52]	[0.5]	[2.4]	[0.49]	[0.5]	[0.34]
		Test of balance across groups					
F-test*		0.77	0.88	1.66	0.45	0.18	0.36
p-value		0.62	0.53	0.12	0.87	0.99	0.93

Standard deviations reported in brackets.

*F-test test for joint significance of groups controlling for school-by-period strata.

[a] Child is eligible for Free/Reduced National School Lunch Program.

Table 4: Children's choices across groups

Group	N	Initial grape choice	Switching to grape	Switching to cookie	Ending up with grape
Private-0	130	0.646	0.062	0.062	0.646
		[0.480]	[0.241]	[0.241]	[0.480]
Public-0	268	0.429	0.104	0.067	0.466
		[0.496]	[0.306]	[0.251]	[0.500]
Private-50-no incentive	171	0.485	0.088	0.070	0.503
		[0.501]	[0.284]	[0.256]	[0.501]
Private-50-incentive	159	0.792	0.019	0.069	0.742
		[0.407]	[0.136]	[0.255]	[0.439]
Public-50-no incentive	238	0.466	0.076	0.059	0.483
		[0.500]	[0.265]	[0.236]	[0.501]
Public-50-incentive	206	0.752	0.058	0.073	0.738
		[0.433]	[0.235]	[0.26]	[0.441]
Private-100-incentive	288	0.760	0.052	0.052	0.760
		[0.428]	[0.223]	[0.223]	[0.428]
Public-100-incentive	171	0.655	0.029	0.105	0.579
		[0.477]	[0.169]	[0.308]	[0.495]
Total	1631	0.616	0.064	0.068	0.612
		[0.486]	[0.244]	[0.252]	[0.487]
F-test*		14.12	2.51	0.82	10.62
p-value		0.000	0.016	0.571	0.000
F-test* (conditional on being incentivized)		1.53	1.46	0.81	1.82
p-value		0.168	0.192	0.562	0.095

Standard deviations reported in brackets.

* F-test for joint significance of groups controlling for school-by-period strata, table size, grade, sex, race and lunch type.

Table 5: Direct effect of incentives (I) and public treatment (P) on initial grape choice (G_1)

$$G_{1i} = \alpha_0 + \alpha_1 I_i + \alpha_2 X_i + \epsilon_i \quad (1)$$

$$G_{1i} = \lambda_0 + \lambda_1 I_i + \lambda_2 P_i + \lambda_3 I_i P_i + \lambda_4 X_i + \epsilon_i \quad (2)$$

	Initial choice of grapes (1)	Initial choice of grapes (2)
Direct effect of incentives (α_1)	0.259	0.241
	[0.031]***	[0.048]***
Difference in incentive effect between public and private treatments (λ_3)		0.013
		[0.067]
Effect of public treatment for non-incentivized children (λ_2)		-0.084
		[0.048]*
Effect of public treatment for incentivized children ($\lambda_2 + \lambda_3$)		-0.071
		[0.051]
Control group mean	0.495	0.495
N	1631	1631

***,**,* = significant at the 1,5,10% level. Column (1) depicts OLS estimates of equation (1) listed on table and Column (2) depicts OLS estimates of equation (2) listed on table. Standard errors are clustered by tables. Regressions control for school-by-period strata, table size, grade, sex, race and lunch type.

Table 6: Reduced-form spillover effects of proportion of table incentivized on switching

$$\Delta G_i = \beta_0 + \beta_1 TP_i + \beta_2 TP_i * P_i + \beta_3 I_i + \beta_4 P_i + \beta_5 I_i P_i + \beta_6 X_i + \epsilon_i$$

	(1)	(2)	(3)
	All children	% of incentivized children > 0%	% of incentivized children ≥ 50%
Spillover effect of peers choosing grapes			
Effect of table proportion incentivized (β_1)	0.093	0.125	0.163
	[0.049]*	[0.066]*	[0.08]**
Spillover effect of peers choosing incentivized grapes			
Effect of table proportion incentivized*public (β_2)	-0.183	-0.223	-0.454
	[0.08]**	[0.107]**	[0.127]***
Total spillover effect			
Effect of table proportion incentivized for public ($\beta_1 + \beta_2$)	-0.091	-0.098	-0.291
	[0.06]	[0.08]	[0.101]***
Average proportion of table incentivized	0.505	0.668	0.802

***,**,* = significant at the 1,5,10% level. OLS estimates control for school-by-period strata, table size, grade, sex, race and lunch type. Standard errors are clustered by table.

Table 7: Spillover effects of proportion of table incentivized on switching to grapes and cookies

$$SC \text{ or } SG = \beta_0 + \beta_1 TP_i + \beta_2 TP_i * P_i + \beta_3 I_i + \beta_4 P_i + \beta_5 I_i P_i + \beta_6 X_i + \epsilon_i$$

Dependent variable:	Switching to grapes (SG)				Switching to cookies (SC)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Children:	All	Incentivized	Non-incentivized	Equality of columns (2) & (3)	All	Incentivized	Non-incentivized	Equality of columns (6) & (7)
Spillover effect of peers choosing grapes								
Effect of table proportion incentivized (β_1)	0.073	0.101	0.034	p-value	-0.020	-0.080	0.061	p-value
	[0.036]**	[0.044]**	[0.063]	0.365	[0.041]	[0.054]	[0.057]	0.065
Spillover effect of peers choosing incentivized grapes								
Effect of table proportion incentivized*public (β_2)	-0.157	-0.172	-0.134	p-value	0.026	0.087	-0.057	p-value
	[0.056]***	[0.069]***	[0.090]	0.609	[0.062]	[0.093]	[0.082]	0.207
Total spillover effect								
Effect of table proportion incentivized for public	-0.085	-0.070	-0.100	p-value	0.006	0.007	0.005	p-value
($\beta_1 + \beta_2$)	[0.039]**	[0.051]	[0.063]	0.878	[0.045]	[0.074]	[0.055]	0.851

***,**,* = significant at the 1,5,10% level. OLS estimates control for school-by-period strata, table size, grade, sex, race and lunch type. Standard errors are clustered by table.

Table 8: Testing for conformity in the proportion of incentivized children

	(1) All kids	(2) % of incentivized children > 0%	(3) % of incentivized children $\geq$ 50%
Panel A: Effect of same-gender incentivized kids			
Spillover effect of peers choosing grapes			
Effect of table proportion incentivized (δ_3)	0.061	0.123	0.054
	[0.137]	[0.136]	[0.154]
Spillover effect of peers choosing incentivized grapes			
Effect of table proportion of group incentivized*incentive*public (θ_6)	-0.047	0.013	-0.126
	[0.219]	[0.218]	[0.24]
Number of observations	1549	1163	825
Panel B: Effect of incentivized best friends			
Spillover effect of peers choosing grapes			
Effect of table proportion incentivized (δ_3)	0.041	0.037	-0.163
	[0.077]	[0.079]	[0.111]
Spillover effect of peers choosing incentivized grapes			
Effect of table proportion of group incentivized*incentive*public (θ_6)	0.27	0.29	0.157
	[0.161]	[0.163]	[0.17]
Number of observations	1631	1233	872
Panel C: Effect of incentivized popular kids			
Spillover effect of peers choosing grapes			
Effect of table proportion incentivized (δ_3)	-0.029	-0.041	0.015
	[0.091]	[0.095]	[0.093]
Spillover effect of peers choosing incentivized grapes			
Effect of table proportion of group incentivized*incentive*public (θ_6)	0.085	0.077	0.27
	[0.169]	[0.168]	[0.144]
Number of observations	1631	1233	872

***,**,* = significant at the 1,5,10% level. We report the estimates of the estimates of δ_3 and θ_6 from the following equations: $\Delta G_i = \delta_0 + \delta_1 TP_i + \delta_2 TP_i P_i + \delta_3 TP_i^{BF} + \delta_4 TP_i^{BF} P_i + \delta_5 I_i + \delta_6 P_i + \delta_7 I_i P_i + \delta_8 X_i + \epsilon_i$ and $\Delta G_i = \theta_0 + \theta_1 TP_i + \theta_2 TP_i P_i + \theta_3 TP_i^{BF} + \theta_4 TP_i^{BF} P_i + \theta_5 TP_i^{BF} I_i + \theta_6 TP_i^{BF} I_i P_i + \theta_7 I_i + \theta_8 P_i + \theta_9 I_i P_i + \theta_{10} X_i + \epsilon_i$. OLS estimates control for school-by-period strata, table size, grade, sex, race and lunch type. Standard errors are clustered by table.

Table B1: Means of outcomes by group

	Outcome:		
	ΔG	Switch to grapes	Switch to cookie
Panel A: Means of variables by group			
Group 1: 0-private	0.000	0.062	0.062
	[0.027]	[0.02]	[0.02]
Group 2: 0-public	0.037	0.104	0.067
	[0.031]	[0.023]	[0.021]
Group 3: 50-private-not incentivized	0.018	0.088	0.07
	[0.024]	[0.021]	[0.019]
Group 4: 50-private-incentivized	-0.05	0.019	0.069
	[0.021]	[0.011]	[0.019]
Group 5: 50-public-not incentivized	0.017	0.076	0.059
	[0.024]	[0.018]	[0.017]
Group 6: 50-public-incentivized	-0.015	0.058	0.073
	[0.03]	[0.021]	[0.019]
Group 7: 100-private	0.000	0.052	0.052
	[0.023]	[0.017]	[0.017]
Group 8: 100-public	-0.076	0.029	0.105
	[0.034]	[0.016]	[0.031]
Panel B: Testing whether watching other children pick grapes changes choice of grapes versus cookies (Regression adjusted)			
Group 3 - Group 1	-0.002	0.024	0.027
	[0.040]	[0.032]	[0.028]
Group 4 - Group 1	-0.076	-0.04	0.036
	[0.038]**	[0.027]	[0.028]
Group 7 - Group 1	0.009	0.01	0.001
	[0.037]	[0.028]	[0.027]
Group 7 - Group 4	0.085	0.051	-0.034
	[0.030]***	[0.022]**	[0.025]
Panel C: Testing whether watching other children pick incentivized grapes increase the likelihood of ending up with grapes (Regression adjusted)			
Group 3 - Group 5	-0.01	0.003	0.013
	[0.035]	[0.028]	[0.025]
Group 4 - Group 6	-0.043	-0.039	0.003
	[0.037]	[0.024]*	[0.028]
Group 7 - Group 8	0.112	0.047	-0.065
	[0.040]***	[0.025]*	[0.034]*

***,**,* = significant at the 1,5,10% level. The OLS estimates fin the two lower panels control for school-by-period strata, table size, grade, sex, race and lunch type. The standard errors are clustered by table. In the two lower panels, under the hypotheses that the spillover effects of watching peers pick grapes are positive while the spillover effects of watching peers pick incentivized grapes has an additional negative effects, the estimates are positive in the first two columns and negative in the third one.

Table C1: Heterogeneity in the direct effects of incentives on initial grape choice, by gender and grade

	Initial grape choice	Initial grape choice
Incentive dummy	0.260	0.284
	[0.040]***	[0.039]***
Incentive*male	0.007	
	[0.055]	
Incentive*above median grade		-0.045
		[0.062]
Control group mean	0.495	0.495
N	1631	1631

***,**,* = significant at the 1,5,10% level. OLS estimates control for school-by-period strata, table size, grade, sex, race and lunch type. Standard errors are clustered by table. Column (1) estimates also include a dummy for male and column (2) estimates include a dummy for grades above the median grade.

Table C2: Heterogeneity in spillover effect of proportion of table incentivized on switching to grapes, by gender

$\Delta G_i = \eta_0 + \eta_1 TP_i + \eta_2 TP_i P_i + \eta_3 TP_i male_i + \eta_4 TP_i P_i male_i + \eta_5 male_i + \eta_6 I_i + \eta_7 P_i + \eta_8 I_i P_i + \eta_9 X_i + \epsilon_i$	(1)	(2)	(3)
	All children	% of incentivized children >0%	% of incentivized children $\geq$ 50%
Spillover effect of peers choosing grapes			
Effect of table proportion incentivized (η_1)	0.174	0.252	0.279
	[0.077]**	[0.104]**	[0.123]**
Effect of table proportion incentivized*male (η_3)	-0.143	-0.232	-0.241
	[0.104]	[0.118]*	[0.149]
Spillover effect of peers choosing incentivized grapes			
Effect of table proportion incentivized*public (η_2)	-0.226	-0.217	-0.367
	[0.117]*	[0.154]	[0.168]**
Effect of table proportion incentivized*public*male (η_4)	0.08	-0.028	-0.13
	[0.148]	[0.178]	[0.245]
Total spillover effect			
Effect of table proportion incentivized for public ($\eta_1 + \eta_2$)	-0.052	0.034	-0.088
	[0.084]	[0.114]	[0.121]
Effect of table proportion incentivized for public male ($\eta_3 + \eta_4$)	-0.063	-0.26	-0.371
	[0.106]	[0.135]*	[0.195]*
Average proportion table incentivized	0.505	0.668	0.802

***,**,* = significant at the 1,5,10% level. OLS estimates control for school-by-period strata, table size, grade, sex, race and lunch type. Standard errors are clustered by table.

Table C3: Heterogeneity in spillover effect of proportion of table incentivized on switching to grapes, by grade

$$\Delta G_i = \eta_0 + \eta_1 TP_i + \eta_2 TP_i P_i + \eta_3 TP_i g_i + \eta_4 TP_i P_i g_i + \eta_5 g_i + \eta_6 I_i + \eta_7 P_i + \eta_8 I_i P_i + \eta_9 X_i + \epsilon_i$$

	(1) All children	(2) % of incentivized children >0%	(3) % of incentivized children ≥50%
Spillover effect of peers choosing grapes			
Effect of table proportion incentivized (η_1)	0.169	0.195	0.188
	[0.069]**	[0.102]*	[0.135]
Effect of table proportion incentivized*above median grade (η_3)	-0.135	-0.139	-0.07
	[0.098]	[0.134]	[0.155]
Spillover effect of peers choosing incentivized grapes			
Effect of table proportion incentivized*public (η_2)	-0.245	-0.377	-0.538
	[0.107]**	[0.147]**	[0.195]***
Effect of table proportion incentivized*public*above median grade (η_4)	0.121	0.339	0.233
	[0.162]	[0.21]	[0.265]
Total spillover effect			
Effect of table proportion incentivized for public ($\eta_1 + \eta_2$)	-0.076	-0.182	-0.351
	[0.077]	[0.108]*	[0.143]**
Effect of table proportion incentivized for public above median grade	-0.014	0.200	0.163
($\eta_3 + \eta_4$)	[0.122]	[0.165]	[0.208]
Average proportion of table incentivized	0.505	0.668	0.802

***,**,* = significant at the 1,5,10% level. g_i is a dummy variable equal to 1 if above median grade and 0 otherwise. OLS estimates control for school-by-period strata, table size, grade, sex, race and lunch type. Standard errors are clustered by table.