When incentives backfire: Spillover effects in food choice

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Abstract

Little is known about how peers influence the impact of incentives. We study two mechanisms by which peer spillover effects can occur: through peers' actions and incentives. Using a field experiment on snack choice in the school lunchroom (choice of grapes versus cookies), we randomize who receives incentives, the fraction of peers incentivized, and whether or not it can be observed that peers' choices are incentivized. We show that, while peers' actions – picking grapes – have a positive spillover effect on children's take-up of grapes, seeing that peers are incentivized to pick grapes has a negative spillover effect on take-up. When incentivized choices are public, incentivizing all children to pick grapes has no statistically significant effect on take-up, as the negative spillover offsets the positive impacts of incentives.

Keywords: food choice, incentives, spillovers, field experiment
JEL Classification: C93, I1, J13

1 Introduction

Incentives are a cornerstone of economics. As such, their use is frequent in many domains, including education, health, pro-social behavior, and the labor market.[1] While often being successful at improving behaviors or outcomes, incentives can backfire. This is because incentives can act as both prices and signals. While the price effect of a higher incentive increases take-up, the signaling effect may increase or decrease take-up.

For example, incentives can signal about the difficulty of the task (Bénabou & Tirole, 2003) or the quality of the good incentivized (e.g., Nelson (1970); Shapiro (1983); Milgrom & Roberts (1986)), may make a subject feel controlled (deCharms, 1968) or, more broadly, convey "bad news" (Gneezy *et al.*, 2011). Consistent with this signaling theory, Gneezy & Rustichini (2000b) find in two separate experiments that performance on a task falls when small monetary incentives are offered, compared to offering no incentives.[2] Fischer *et al.* (2014) find that the subsequent demand for various products decreases with their introductory price.[3]

A limitation of the literature on the signalling effects of incentives is that it focuses on the effects of incentives on their recipients (the *direct* effects), neglecting the *spillover* effects of incentives.[4] For example, consider paying students to choose a healthy snack at school. The direct effect of the incentive program, absent the influence of peers, may result in students eating more of the healthy snack. At the same time, these incentives can cause two types of spillover effects. One spillover effect operates through observing peers' actions and a second works through observing peers' incentives. If I see my friends pick the healthy snack, I may think that this snack is delicious and healthy. However, if I see my friends incentivized to choose this snack, I may think that it is not tasty (and thus, why it was incentivized). These two spillover effects are not necessarily of the same sign or size nor are they constant, as their magnitudes may vary with how many of my friends are incentivized.

In sum, when we observe that incentivizing children's choice of a healthy snack affects their eating behavior, we are observing a combination of (i) the direct effect of incentives – paying

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[1] A not nearly-exhaustive list of these studies include Volpp *et al.* (2008, 2009); Charness & Gneezy (2009); Acland & Levy (2015); Babcock & Hartman (2010); Babcock *et al.* (2015); Cawley & Price (2013); John *et al.* (2011); Royer *et al.* (2015); Belot *et al.* (2013); List & Samek (2014, 2015); Loewenstein *et al.* (2016) for healthy behaviors, Angrist *et al.* (2009); Bettinger (2012); Fryer Jr (2011); Levitt *et al.* (2011b,a) for academic achievement, Ariely *et al.* (2009); Lacetera & Macis (2010); Lacetera *et al.* (2013) for pro-social behavior, and Gneezy & List (2006); Fehr & List (2004); Bandiera *et al.* (2013); Shearer (2004) for worker effort.

[2] In a separate experiment, Gneezy & Rustichini (2000a) show further evidence of the backfiring of incentives. The introduction of fines for tardiness for daycare pickups increased the fraction of parents who were late for pickups.

[3] See Deci *et al.* (1999) for a meta analysis of the signaling effects of incentives from psychology and Gneezy *et al.* (2011) and Kamenica (2012) for evidence from economics.

[4] Following the taxonomy of Angelucci & Di Maro (2016), we define spillover effects in this paper as peer social interaction effects.

a child to pick a healthy snack, (ii) the effect of peers' action – changing peers' choice of the healthy snack, and (iii) the effect of peers' incentives – watching that peers are incentivized to pick the healthy snack. These effects may not necessarily have the same sign. In addition,1) these spillover effects are of indeterminate sign, 2) the overall effect (i.e., the sum of the direct and spillover effects) may differ from the direct effect, and 3) the overall effect may vary with the fraction of peers incentivized, if each spillover effect is non-linear with respect to the fraction incentivized.

The goal of this paper is to study the impact of incentives through peers' actions and peers' incentive status and, specifically, to test whether spillover effects can undo the direct effect of incentives. To do that, we design and conduct a field experiment that lets us measure the total effect of incentives and decompose it into its direct effect and its spillover effects that occur through peer actions and incentives. We offer grapes and cookies and incentivize the choice of grapes versus cookies of 1,600 children in grades K-8 in a low-income Chicago neighborhood.[5]

The experiment has two stages. In stage 1, children choose grapes or a cookie simultaneously, without observing their peers' choices. We define their peers in this case as other children sitting at their lunch room table. In stage 2, we allow children to switch their snack of choice after observing peers' initial choices and, in some cases, peers' incentive status. We call the stage 1 decision the direct effect of incentives, because this choice is unaffected by peers' actions and incentive status, unlike the choice in stage 2, which encompasses direct and spillover effects.

To identify the direct effect of incentives, we randomize who is incentivized to choose grapes. To separate the spillover effects of peers' actions from the spillover effects of peers' incentive status, we randomize both the fraction incentivized at each table and whether a student's choice of incentivized grapes is public knowledge (our public treatment). Randomizing the fraction of tablemates incentivized allows us to identify the spillover effects under weaker assumptions than much of the prior literature.[6]

While our experiment occurs in a controlled environment, it has some important real-life

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[5]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). Thus, incentivizing the choice of healthy food may be one policy tool to reduce the rates of overweight and obesity.

[6]As summarized by Baird *et al.* (2014), the previous literature identifies spillover effects in the following ways. First, by not treating some group members (e.g., Angelucci & De Giorgi (2009); Barrera-Osorio *et al.* (2011); Bobonis & Finan (2009); Duflo & Saez (2003); Lalive & Cattaneo (2009); Guiteras *et al.* (2015)). For example, Angelucci & De Giorgi (2009) measure the effect of a cash transfer on consumption by looking at ineligible households in treatment and control villages. Second, by using plausible exogenous variation in the fractions of peers treated (e.g., Babcock & Hartman (2010); Beaman (2012); Conley & Udry (2010); Duflo & Saez (2002); Munshi (2003)). For example, Munshi (2003) exploits the variation in migrant networks to look at their effect on employment outcomes. Lastly, by looking at differential treatment effects within a predetermined peer group (e.g., Banerjee *et al.* (2013); Chen *et al.* (2010); Macours & Vakis (2008); Neumark-Sztainer *et al.* (2012)). For example, Banerjee *et al.* (2013) map social networks in Indian villages and then study how the infusion of information about microfinance affects knowledge and loan take up in the village.

features. First, children choose their tablemates, so that the peer groups are not created artificially by the researchers. Second, children can infer that not everybody is incentivized to choose grapes, but the actual fraction of incentivized children is unknown. This feature mimics many real-life policies and programs (e.g., conditional cash transfers and other means-tested programs): their existence and broad features are known, but there is imperfect information about the proportion and identity of beneficiaries.

Our main finding is that making incentives visible can reduce the take-up of grapes: incentives *can* backfire. The direct effect of incentives is positive, meaning that the initial take-up of grapes increases with the proportion of children incentivized both in the public treatment, in which both choice and incentivized status are visible, and in the private treatment, in which only choice is visible. However, the indirect effect of incentives differs by treatment. In public tables, some children in the 100% incentivized tables switch from grapes to cookie after observing that all children who chose grapes were incentivized to do so. In these tables, incentivizing all children has no statistically significant effect on grape take-up relative to incentivizing no children because the negative indirect effect offsets the positive direct effect. This degree of switching does not occur in private tables in which all children are incentivized. In fact, we find that in private tables the spillover effects are positive for all fractions of incentivized children. Conversely, there are non-linear spillover effects of incentives with respect to the fraction incentivized in the public treatment. The overall effect of incentives (i.e., the sum of the direct and spillover effects) is positive when we incentivize up to two thirds of children, while it becomes statistically insignificant when we incentivize all children.[7]

Imagine that our experiment consisted of stage 1 only, that is, we randomly offered incentives and we forced the choice to occur simultaneously. If we had done this, we would have measured the direct effects only, and concluded that incentives have a strong positive effect on the take-up of grapes, while, in fact, this is not always the case. Similarly, imagine that we had not separated stages 1 and 2 and compared the final grape take-up in tables with 0 and 100% of children incentivized. In that case, we would not have been able to separate the direct and the spillover effects of incentives and may have concluded that our subjects do not respond to our incentives, while, in fact, they do, but in offsetting ways. Lastly, if we had not let the fraction of children incentivized vary across tables, we would not have been able to measure the non-monotonicity in the spillover effects of incentives.

In sum, our experiment shows that spillover effects can be i) large, ii) positive or negative, depending on the relative salience of peers' action and incentive status, and iii) big enough to

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[7]These findings echo the results in Bursztyn & Jensen (2015) and Bursztyn *et al.* (2016), who find that take-up rates of a SAT preparatory course are lower when take-up is observable.

offset any direct effects of incentives. The finding that the spillover effect of peers' incentive status is negative is consistent with the hypothesis that incentives are "bad news." We also rule out that the effects of peers' incentives are driven by envy or fairness issues, by a desire to conform differently to one's best friends, popular kids, or kids of the same gender than to other types of children, and by changes in the perceived value of the prizes. Lastly, our findings do not seem driven by social signaling, as the direct effect of incentives does not vary when incentives are public or private.

Our findings have three broad implications. First, to understand the full impact of incentives, one should design experiments to capture spillover effects. Ignoring the spillover effects might result in imprecise policy recommendations because the direct and spillover effects could possibly offset one another. For example, in our experiment, the direct effect of incentives can be larger than the overall effect of incentives, when incentives are public.

Second, the presence of non-monotonicities with respect to the fraction incentivized makes extrapolation and policy scale-up from field experiments challenging. The existence of "social multipliers" (Glaeser *et al.*, 2003) is well known. However, the implicit assumption – backed by abundant empirical evidence – is that the multiplier is monotonic and that, therefore, the direct effect of incentives is a lower bound of its net effect (in absolute level).[8] This is not the case in our setting, since the overall effect of incentives in the public treatment is positive when we incentivize up to around two thirds of tablemates, but zero when we incentivize all subjects. The existence of non-linearities implies that field experiments incentivizing different fractions of the subject pool may come to different conclusions about the effects of the same type of incentive. For example, consider the effect of PROGRESA's conditional cash transfers for human capital accumulation. These transfers were given to about 75% of the population in treated villages, and have been shown to have positive direct and spillover effects on school enrollment (Angelucci *et al.*, 2010; Bobonis & Finan, 2009; Lalive & Cattaneo, 2009). If the spillover effects vary non-monotonically with the fraction of the treated population, the estimated effects in the evaluation villages may differ in both sign and magnitude from the effect at the national level, because different fractions of the population are treated after the program is rolled out.

Third, if observing others' incentive status reduces take-up, private incentives may be preferable. This may not be feasible in most settings, as people communicate and interact.

One should be cautious in generalizing our results. Different settings or populations may lead to different spillover effects of incentives. The main conclusion of this paper, nevertheless,

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[8]Examples include the take-up of welfare (Borjas & Hilton, 1996; Bertrand *et al.*, 2000), employer-sponsored health insurance (Sorensen, 2006), retirement plans (Duflo & Saez, 2003), public prenatal care (Aizer & Currie, 2004), disability insurance (Rege *et al.*, 2009) and movie attendance (Moretti, 2011), among others.

remains valid (and valuable): spillover effects *can* undo the direct effect of incentives. Further research should study whether different designs and contexts deliver similar results.

This caveat notwithstanding, we believe that evaluating the spillover effects of incentives may 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). In such settings, incentives allow subjects to learn from peers' and policymakers' actions. However, as the signalling effects of incentives can be both negative and positive, careful consideration should be made when invoking incentive schemes for public policy. For example, the Physician Payment Sunshine Act, which requires drug 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. 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).[9] 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 (see, e.g., Frey & Oberholzer-Gee (1997)). Finally, the fourth possible setting is in interesting or pleasant activities, where incentives may reduce interest in the task.[10]

2   Background and Experimental Design

To measure the direct and spillover effects of incentives, we designed an artefactual field experiment (Harrison & List, 2004) in which we offer grapes and cookies to children and randomly offer incentives to choose grapes.[11] This experiment took place in school cafeterias during lunch. All 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 grade (e.g., 4th, 5th, and 6th graders could eat lunch separately from other grade levels). Children arrive for lunch during their designated period together with their class. They go through a line where they receive a school lunch and then sit at a table in the cafeteria. Except for kindergarteners, students can typically sit with any other children from their grade and tend to form groups of 3-10 students

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[9]Gneezy *et al.* (2011) review the evidence of incentives backfiring in this type of setting.

[10]Kamenica (2012) review the evidence of incentives backfiring for the last two settings.

[11]Grapes, but not cookies, are sometimes served at lunch. No school offered grapes while the experiment was run.

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. We are not aware of specific programs at these schools promoting healthy eating habits.

We conducted the experiment after children had collected their lunch trays and sat down to eat at their table, as they normally do. Once children chose where to sit, members of the research team came to the table and read a script (Appendix A), which described the procedures of the experiment. We treated adjacent tables simultaneously. This, and the fact that children are required to stay seated at their table throughout the entire lunch period, minimized cross-table contamination. To ensure compliance, we assigned one research assistant to one table at a time and ensured that adjacent tables could not easily see what was happening at nearby tables.

The experiment took place in two rounds. Children receive two cards - one grape card (some of which will have incentives) and one cookie card. In round one, simultaneously children choose grapes or cookies by placing one of the cards on the table. After observing their tablemates' choices and, depending on the treatment, their tablemates' incentives, children again simultaneously play one of their cards. We provide further details below.

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 choose only 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. Children complied with these requirements. After the initial choice, children had twenty seconds to play a different card after having observed their peers' choices.

We randomized 1) whether a child received an incentive to choose grapes, 2) the fraction of each lunch table that received an incentive to select grapes, and 3) at which tables choosing incentivized grapes are visible by peers (public treatment) or not (private treatment). In particular, for each table, we had a stack of cards, which had either 0, 50, or 100% of cards with incentives. At tables with 50% of cards incentivized, because the incentivized cards where randomly stacked in the decks and the number of occupants fluctuates by table, the actual fraction of children receiving incentives varies between 11 and 80 percent, as shown in Table 1. This

Table show the distribution of the fraction of each table incentivized.

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 each prize was roughly 50 cents. The prizes included glow-in-the-dark bouncy balls, small trophies, and bracelets and pens of different types. We chose several different prizes that the children were familiar with and prizes that many of them already have. Our reasoning for this choice is to limit the possibility that the children's value of their possible prize would be strongly related to the number of other children choosing that prize.

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 six table types, depending on (i) whether 0, 50, or 100% of the cards for a table is incentivized, and (ii) whether the incentivized choices are public or private. If we further group children based on their incentive status, we end up with eight groups:

- Private-0-no incentive: Children in the Private treatment in which none of the grape cards were incentivized.

- Public-0-no incentive: Children in the Public treatment in which none of the grape cards were incentivized.

- Private-50-no incentive: Children in the Private treatment in which 50% of the grape cards were incentivized but the child's own card was not incentivized.

- Public-50-no incentive: Children in the Public treatment in which 50% of the grape cards were incentivized and the child's own card was not incentivized.

- Private-50-incentive: Children in the Private treatment in which 50% of the grape cards were incentivized and the child's own card was incentivized.

- Public-50-incentive: 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: Children in the Private treatment in which all of the grape cards were incentivized.

- Public-100-incentive: 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 in 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.

We record both the initial food choice, G_1, and the final one, G_2. We use G_1 to measure the direct effect of incentives because this choice occurs simultaneously and before children can observe their peers' choices and incentives. We use G_2 to measure the spillover effect of incentives because this final choice occurs after having observed peers' choices and incentives.

Our experimental design makes advances in the peer effects literature along 4 dimensions. First, by recording both initial and final snack choice, we can both measure the overall effect of incentives and decompose it into the direct and the spillover effects of incentives. The existing literature typically focuses on measuring either the overall effects, without being able to decompose them (as in, e.g., Royer *et al.* (2015)) or the direct effect only, without being able to study how incentives would affect behavior once spillover effects are allowed to operate (as in, e.g., Just & Price (2013)).

Second, by randomly varying the fraction of treated peers and allowing people to switch snack after observing their peers' actions and incentives, we can measure spillover effects on both incentivized and non-incentivized subjects. In many papers that measure spillover effects, this is not possible unless one is willing to make (potentially unrealistic) assumptions. This occurs because papers that measure spillover effects typically do so by looking at the effect of a treatment on untreated subjects and not from a treatment on both treated and untreated subjects. If treated and untreated subjects are randomly selected (as in, e.g., Duflo & Saez (2003)), the spillover effects on the treated can be identified from the untreated under the assumption that these effects are additive, but such an assumption is not necessarily backed by any theory. If treated and untreated subjects are not randomly selected (as in, e.g., Angelucci & De Giorgi (2009)), then the spillover effects on the treated can be identified from the untreated under the assumption that these effects are the same for treated and untreated people.

Third, by randomly varying the fraction of treated peers, we can measure potential non-linearities in spillover effects. In many papers that measure spillover effects, this variation is not

random. For example, in Babcock & Hartman (2010), some subjects have more treated friends than others. However, these subjects may also have more friends to begin with, so the variation in treated friends is not exogenous. Therefore, while these papers can measure the combined effects of treating *different subjects* and having *different numbers of treated peers*, we can isolate the latter effect under weak identification assumptions.

Fourth, by having private and public treatments, we can separate the spillover effect of peers' actions (observed both in the private and public treatment) from the spillover effects of peers' incentives (observed in the public treatment only). To our knowledge, this is the first time such a decomposition has been done.

3   The data

3.1   Sample

A total of 1,771 children participated in the experiment. We exclude tables of size 10 or larger (14 tables) because at larger tables, it is difficult for children to see all others' decisions. Results are qualitatively similar if we do not drop these tables.

We complemented 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. There were also questions about each child's perceived social status relative to other children and the most popular kid boy and girl in their class. A total of 1,286 (73%) 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] The size of each group varies because some of the tables in the cafeteria were empty.

3.2   Descriptives, balance tests, and food choice

Table 2 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 (and some more qualify for lunch at a reduced price). We test that the variables are balanced across groups in the lower panel of Table 2, which shows the F-test of joint significance of the 8 group dummies,

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[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.

when regressed on each of these variables together with school-by-period strata. None of the F-tests are significant at conventional levels, consistent with random assignment.

We also check for balance using the actual fraction of student incentivized as opposed to these discrete groups considered in Table 2. 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 by chance. 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, and to improve the precision of the estimates, we control for all the aforementioned variables in all our specifications. The results are qualitatively unchanged whether we add these variables or not.

4 Total effect of incentives

Our goal is to estimate the total effect of incentives on grape take-up and to decompose this total effect into the direct effect of incentives and the spillover effects due to peers' actions and incentives. To do that, we first consider the effect on final grape take-up, the variable G_2, which is the sum of the initial grape choice, G_1, and the revised choice, ΔG. We then proceed to estimate the direct effects of incentives using the variable G_1 and the spillover effects of incentives using the variable ΔG. We show how these variables change differently in the private and public treatments as the fraction of peers incentivized varies from 0 to 100 percent.

Factoring into children's first round choice, G_1, may be their initial beliefs of the proportion of peers incentivized. These beliefs do not vary systematically across the private and public treatments because children are randomly assigned to these treatments and, at this stage, they all have the same information.

The initial beliefs can change after observing peers' initial choice. We expect these beliefs to change more for public tables, where more information is revealed, as children can see both how many peers pick grapes and how many of those are incentivized. Since, conceivably, most children's initial belief is that a fraction of children are incentivized, beliefs are more likely to change more in tables in which either zero or all the children are incentivized. In addition, we expect that, in public tables, the final grape choice will vary inversely with the fraction of

children incentivized, if the perceived value of grapes declines with the proportion of incentivized peers. The evidence from our data is consistent with our expectations, as we discuss below.

Figure 2 plots the semi-parametric total effect of table proportion incentivized on a dummy variable indicating the final choice of grapes, G_2, separately for the private and public treatments.[13] While the total effect of incentives grows with the table proportion incentivized in the private treatment, this effect is non-monotonic in the public treatment. At these tables, the total effect of incentives grows with the proportion incentivized up until about two thirds of children are incentivized, but it is considerably lower when all children are incentivized, to the degree to which there is no statistically significant difference in final grapes take-up between tables with 0 and 100% incentives. Note that, while we have few tables with 60 to 80% incentivized children (hence the larger confidence intervals), grapes take-up among tables with 60 to 80% incentivized differs statistically from tables with both 0% and 100% incentivized children.[14]

Comparing the public and private treatments suggests that the non-monotonicity in the public treatment is linked to the observability of incentives, as the effect is monotonic in the private treatment. Figure 3, which measures the direct effects of incentives, confirms this because the initial choice of grapes, made before peers' actions and incentives are observed, grows with the table proportion incentivized in both public and private treatments.

A comparison of Figures 2 and 3 shows evidence of modest spillover effects of peers' actions and negative spillover effects of children's incentive status. The spillover effects of peers' actions are modest, as initial and final grape choice in the private treatment are similar. The spillover effects of children's incentive status are negative because there is a drop from initial to final grapes take-up where all children are incentivized in the public treatment tables (where incentives are visible), but not in the private tables (where incentives are not visible). The initial choice of grapes is lower in the public than the private treatment but this difference is not statistically significant. Qualitatively, such a finding is consistent with the idea that making incentives more salient may signal "bad news" (e.g., Gneezy *et al.* (2011)) or make the short term costs of picking grapes over a cookie more prominent.[15]

To conclude, these figures provide initial evidence of direct and spillover effects of incentives

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[13]To do so, we use the Robinson's semi-parametric estimator (Robinson, 1988) to control for the effect of the predetermined covariates (school-by-period strata, table size, child age, gender, race, grade, and school lunch status) 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, as well as to using the table, rather than the child, as the unit of analysis. We cluster the standard errors by table. We use the same empirical approach also for the next figure.

[14]Comparing group means reinforces 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.

[15]Children in these two arms have the same initial priors, since they know the same information, so the differences in initial choice cannot be driven by differences in priors.

of opposite signs in the public treatment: as conjectured, public incentives can backfire. Our next step is to decompose the total effect of incentives into its direct effect and the spillover effects to study their sign and magnitude.

5 Direct effects of incentives

We measure the direct effect of incentives on grape choice, that is, whether receiving the incentives changes the recipients' likelihood of initially choosing grapes, by comparing the *initial* grape choice of incentivized and non-incentivized children.[16] 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 and 0 otherwise. To improve the precision of the estimates, we condition on the variables X: dummies for school-by-period strata, table size, child age, gender, race, grade, and school lunch status.

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

The coefficient α_1 identifies the average treatment effect of incentives on initial grape choice. This parameter is identified under the assumptions that (i) the variable I and the error term ϵ are independent, which follows from random assignment, and that (ii) one child's potential outcomes are unaffected by the treatment status of others, which follows by keeping treatment status private at this stage. We estimate the parameters of this equation by OLS, clustering the standard errors by table. We use the same controls and method of clustering throughout the paper.

We can also interact the incentive dummy by a dummy for the public ($P = 1$) treatment:

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

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 ($\lambda_2 + \lambda_3 = 0$).

Column (1) of Table 3 shows the direct effects of incentives on the initial choice of incentivized children (the estimate of α_1 from equation (1)). Incentives increase initial grape take-up by 26 percentage points, a statistically significant increase of about 53%, compared to a 49.5% take-up

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[16]A potential concern is that children may use their initial choice strategically, either as a signaling device, or to affect their peers choices (e.g., if they derive positive or negative utility for social conformity). We think this is unlikely because the final choice seems to be a more credible signal than the initial choice, and because only about 13% of the sample makes a switch between the initial and final choice.

rate among non-incentivized children.

These findings are comparable in size to Just & 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 & 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 *et al.* (2013), whose piece-rate incentives to choose an extra vegetables side dish have a small, statistically insignificant effect.

Column (2) shows the parameter estimates from equation (2). The direct effects of incentives do not differ in the public and private treatment, as the point estimate of λ_2 is 0.013 and statistically insignificant, while the initial grape choice is 8.4 and 7.1 percentage points lower in public treatments for both non-incentivized and incentivized children (the estimates of λ_2 and $\lambda_2 + \lambda_3$ from equation (2)), as we already noticed above. These findings are inconsistent with social signaling. For example, if children wanted to reveal (conceal) to others that they are incentivized, the effect of incentives on grape choice would be higher (lower) in the public treatment. Moreover, in Section 8.3 we will show that kids' choices are not affected differently by the choices of their best friends, popular kids, or kids of their same gender, which is also likely inconsistent with social signaling.

6 Spillover effects of incentives

We now proceed to look at the second stage decisions to tease out the two spillover effects: one from seeing other children pick grapes and one from observing other children picking incentivized 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:

$$\Delta G_i = \beta_0 + \beta_1 TP_i + \beta_2 TP_i * P_i + \beta_3 I_i + \beta_4 P_i + \beta_5 I_i P_i + \beta_6 X_i + \epsilon_i \qquad (3)$$

We condition on being incentivized (I) and on the public treatment dummy (P) because they affect the initial grape choice, which, in turn, affects the likelihood of ending up with grapes. The parameter β_1 identifies the marginal effect of the proportion of incentivized children at one's table in the private treatment, while β_2 identifies the difference in the effect of this proportion between the public and private treatments. β_1 and β_2 are two separate spillover effects on one's

own choice: β_1 is the reduced-form effect of observing peers' choices and β_2 is the reduced-form effect of observing whether peers' choices are incentivized.

We add the interaction of I and P to allow for the possibility that the effects of the incentives differ across the public and private treatments. The results do not change qualitatively whether we interact by public treatment or not, or whether we estimate the parameters of equation 3 or of equation $G_{2i} = \beta_0 + \beta_1 TP_i + \beta_2 TP_i * P_i + f(\beta_{IPG_1} I_i P_i G_{1i}) + \beta_3 X_i + \epsilon_i$, where the term $f(\beta_{IPG_1} I_i P_i G_{1i})$ is the sum of all the interactions of the incentive treatment, public treatment, and initial grape choice dummies.[17] Note in Appendix B, we consider heterogeneity in these treatment effects by the type of switch (e.g., grapes to cookies versus cookies to grapes), gender, school grade, and table size.

Children do not observe the variable TP but instead observe the fraction choosing incentivized grapes in the first round. However, this variable is exogenous and under the control of the policy maker, and thus its impact is of policy relevance. Moreover, it is positively correlated with the fraction of table mates choosing grapes, which is endogenous.[18]

Table 4 shows the estimates of our parameters of interest, β_1 and β_2 from equation (3). Column 1 shows that a 1 percentage point increase in the proportion incentivized in the private treatment increases the likelihood of switching to grapes by 0.09 percentage points (s.e. 0.05). 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. The second row of estimates in column 1 shows that the effect of the proportion incentivized changes when the incentives are public. Relative to private incentives, a 1 percentage point increase in the proportion incentivized additionally decreases one's likelihood of switching to grapes by 0.18 percentage points (s.e. 0.08). 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 ($-0.09 = 0.09 - 0.18$; s.e. 0.06). Overall, in column 1, the spillover effects are of opposite sign: a positive direct effect of incentives, positive effects of peer's actions, and negative effects of peers' incentive status. This has important implications for scaling up experiments. In particular, the total effect of incentives (i.e., the sum of the direct effect and two spillover effects) can be non-monotonic with respect to the fraction incentivized, meaning that it is particularly challenging to determine the magnitude but more importantly the sign of the total effect of incentives when scaling up.

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[17]In unreported regressions, we replace the table proportion incentivized with the table proportion incentivized *other than self* and the results are qualitatively unchanged.

[18]The correlation coefficient is 0.27.

Non-linearities. The specification highlighted above in equation (3) specifies that the effect of the proportion incentivized as being linear. We also consider possible non-linearities alternatively truncating the sample and using a quadratic function of the table proportion incentivized. Both approaches yield a similar message: non-linearities matter.

First, we restrict the sample to tables with a positive proportion of incentivized children (column 2 of Table 4) and with at least 50% of incentivized children (column 3), in which case the average table proportion incentivized increases from 50% to 66% (column 2) and to 80% (column 3).[19] When we do that, we find that the two marginal spillover effects become considerably larger (in absolute value), especially the negative effect of observing other children's incentivized choices.

Second, we estimate equation (3) 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 4 shows the marginal effects of fraction incentivized from this equation (i.e., estimates of $\beta_1 + \beta_2 * P_i + 2\beta_4 TP_i + 2\beta_5 TP_i * P_i$). If the effects were linear, each of those graphs would depict a horizontal line, which they do not. The figure confirms that the marginal effects grow of the table proportion incentivized 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.

7   Combining the direct and spillover effects

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 we found to be positive, and the two spillover effects on changing snack, ΔG, which we found to be one positive and the other negative. We can now compare the estimates of the direct and spillover effects from Tables 3 and 4, as well as compute their sum, which is the total effect of incentives.

The combined evidence of the direct and spillover effects matches our initial findings from Figure 2. A 1 percentage point increase in the proportion of incentivized children has a direct effect on the likelihood of choosing grapes of 0.26 percentage points (from table 3, column 1) 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 percentage point 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% (table 4, row 1). Therefore, the fraction incentivized that maximizes the likelihood of

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[19]The sample restrictions in columns 2 and 3 drop approximately the first quartile and the first two quartiles of the fraction incentivized distribution, respectively.

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 because, in many settings, spanning from conditional cash transfers such as PROGRESA to incentives for student performance (such as Levitt *et al.* (2016)), 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 persons choosing 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% (table 4, row 2).

Using all the aforementioned estimates and calculating the total effect of incentives, we find that a 1 percentage point increase in the proportion incentivized overall increases grapes take-up by 0.17 (0.26+0.09-0.18) percentage points when the fraction incentivized is 50% and by 0.16 (0.26+0.12-0.22) percentage points when the fraction incentivized is 66% but reduces grapes take-up by -0.03 (0.26+0.16-0.45) percentage points when the fraction incentivized is 80%. Therefore, while incentivizing either half or two thirds of children increases grapes take-up in the public treatment, incentivizing 80 percent of children does not increase take-up relative to no incentives.

8 Stylized Model

Our empirical analysis reveals two main findings. First, in the public treatment, the effect of incentives on take-up is non-monotonic: the fraction choosing grapes in 100% incentivized tables is not statistically different from the fraction selecting grapes among the 0% incentivized tables, while lower fractions of incentivized children increase take-up. Second, for the private treatment, the effect of incentives on take-up is positive and monotonic.

The purpose of this section is to outline a model to demonstrate that negative spillover effects of incentives, as we see in our data, are theory-consistent. Other candidate models are possible, some of which we can provide empirical evidence against.

The intuition behind our model is the following. Consider an agent who is choosing one action about which she has incomplete information and, as in Banerjee (1992), the agent is more likely to make a choice if her peers have done it as well. A principal with private information about the

action may incentivize the choice. Incentives may signal that the action is costly to the agent and the principal may be uncertain about this signaling effect of the incentive on the agent, as in Bénabou & Tirole (2003). Introducing incentives to undertake the action reduces the signal from peers' actions: the agent cannot tell if the peers chose the action because they value it or because they were paid to undertake it. Moreover, the incentive may signal the principal's view about the product. In some cases, this incentive may signal that the action is costly. Therefore, the total effect of incentives can be negative even in cases in which the direct effect of incentives increases the take-up of the action, because the spillover effect of incentives can be negative. In this case, the total effect of incentives is ambiguous.

More formally, consider the choice of grapes, $G_i = 1$, versus cookies, $G_i = 0$, 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, B_i, exceed its costs, C:

$$E[U(G_i = 1) - U(G_i = 0)] = B_i - C. \tag{4}$$

The monetary (or cash-equivalent) cost of choosing grapes over cookies, C, is a function of incentives, I. 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 have private information about the relative value of grapes over cookies, such as their relative health benefits and taste. The child observes the behavior of her peers and of the experimenter to infer their private information. This setting does not require the experimenter and peers to have more or better information than the child, and similarly, it does not necessitate that the child have no information about grapes or cookies. However, it requires that observing peers' actions and incentives can lead to changes in the child's belief. For example, in our empirical setting, cookies are not typically part of the lunch menu, so some children may not know how good the offered cookies taste.[20] Moreover, the experimenter may have seen other children make this choice before, and, therefore, be expected to have information about children's relative preferences over the snacks.

In our setting, the experimenter announces that an unspecified fraction of children will be incentivized to pick grapes, as is often the case in public policy settings. This announcement affects the child's beliefs of the benefits, B_i. Then, the child observes whether she is incentivized to pick grapes, I, which also affects her beliefs of B_i, and makes an initial snack choice simultaneously with her peers. At this point, she can see the fraction of her peers who choose grapes

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[20]In other settings, farmers might not know how good a new crop, seed, fertilizer, pesticide, or technology might be.

over cookies, $\bar{G}_{-i}$, and, in some cases, also the fraction of her peers who choose incentivized grapes, $\bar{I}_{-i}$. $\bar{I}_{-i}$ is a lower bound of the fraction of peers who were incentivized to choose grapes, TP. This additional information may lead her to revise her initial beliefs, and, subsequently, her snack choice. In sum, the expected utility of choosing grapes over cookies can be expressed as:

$$E[U(G_i = 1) - U(G_i = 0)] = B_i(\tau_i, \bar{G}_{-i}(TP), \bar{I}_{-i}(TP), I) - C(I) \qquad (5)$$

We do not have to assume that the experimenter and peers have private information, as done above, to generate model predictions consistent with our empirical results. Alternatively, a model with symmetric information would generate the same predictions with the following assumptions: (1) there is a short-term cost of choosing grapes over cookies, (2) there is a long-term benefit of selecting grapes over cookies, and (3) the public incentive makes the short-term cost of choosing grapes more prominent.

8.1 Direct Effect of Incentives

Returning to our original model, consider first the direct effect of incentives, I, on the incentivized person:

$$\frac{\partial E[U(G_i = 1) - U(G_i = 0)]}{\partial I} = \frac{\partial B_i}{\partial I} - \frac{\partial C}{\partial I} \qquad (6)$$

The first right-hand side term, $\frac{\partial B_i}{\partial I}$, 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 Bénabou & Tirole (2003), being incentivized (or having a higher-valued incentive) signals "bad news" – e.g., that the experimenter and the other children perceive grapes to be unpopular or unpleasant.[21] This may make her revise her prior beliefs about the benefits of grapes 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.[22]

Conversely, the second right-hand side term, $\frac{\partial C}{\partial I}$, which represents the effect of the incentives on cost, is negative, as compensating the child to pick grapes over cookies reduces its cost. In

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[21]In Bénabou & 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 the unpleasant tasks.

[22]Announcing that there will be incentives has the same ambiguous effect on beliefs. We do not discuss it further because, since all children receive this announcement, this effect cancels out in our empirical analysis.

sum, the sign of the direct effect is unknown, due to the ambiguity of the sign of $\frac{\partial B_i}{\partial I}$. In our application, we estimated it to be positive.

8.2 Spillover Effects of Incentives

Now consider the effect of the fraction incentivized. This is what we call 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, $\bar{I}_{-i}$.

$$\frac{\partial E[U(G_i = 1) - U(G_i = 0)]}{\partial TP} = \frac{\partial B_i}{\partial \bar{G}_{-i}} \frac{\partial \bar{G}_{-i}}{\partial TP} + \frac{\partial B_i}{\partial \bar{I}_{-i}} \frac{\partial \bar{I}_{-i}}{\partial TP} \tag{7}$$

The first right-hand side term, $\frac{\partial B_i}{\partial \bar{G}_{-i}} \frac{\partial \bar{G}_{-i}}{\partial TP}$, is the spillover effect of incentives arising from watching others pick grapes and has an indeterminate sign theoretically. 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.[23] Therefore, the sign of this first term depends on how increasing the proportion incentivized affects the proportion picking grapes initially, $\frac{\partial \bar{G}_{-i}}{\partial TP}$. This has the same sign as the direct effect of incentives.

The second right-hand side term, $\frac{\partial B_i}{\partial \bar{I}_{-i}} \frac{\partial \bar{I}_{-i}}{\partial TP}$, 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 and, therefore, has the same sign as $\frac{\partial B_i}{\partial I}$. $\frac{\partial \bar{I}_{-i}}{\partial TP}$ has the same sign as the direct effect. Overall, taking into account the spillover effects and their possible signs (detailed below), the sign of the overall effect of incentives can be ambiguous.

There are, therefore, the following 3 cases, also summarized in Table 5:

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 two spillover effects of incentives are also positive. That is, in this case the spillover effects through peers' actions and

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[23]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.

incentive status 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 children, $\frac{\partial B_i}{\partial I} > \frac{\partial C(I)}{\partial I}$. If the incentive sends a negative signal on the value of grapes, 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, increasing the proportion incentivized increases the proportion choosing grapes, incentivized or not, ($\frac{\partial \bar{G}_{-i}}{\partial TP} > 0$ and $\frac{\partial \bar{I}_{-i}}{\partial TP} > 0$). It follows that the sign of the spillover effect is indeterminate: the first term is positive, the second negative. That is, in this case the spillover effects may either reinforce or offset the direct effects. This is the case consistent with our data.

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 the value of grapes, 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 $\frac{\partial \bar{I}_{-i}}{\partial TP} < 0$). It follows that the sign of the spillover effects is indeterminate: the first term is negative, the second positive. That is, in this case the spillover effects may either reinforce or offset the direct effects.

In sum, we have 3 broad conclusions. First, incentives may have both a positive and a negative direct effect.[24] Second, when incentives are "bad news" ($\frac{\partial B_i}{\partial I} < 0$), the spillover effects of incentives can have both a positive and negative component. Third, when incentives are "bad news," the direct and spillover effects of incentives may offset each other. Therefore, the direct effect may be a poor approximation of the *overall* effect of incentives.

8.3 Alternative models

There are multiple models that generate direct and spillover effects of incentives of opposite signs and thus, lead to an overall effect of incentives of indeterminate sign. For example, a model in which there is both an intrinsic dislike for incentives, as, e.g., they make subjects feel controlled (deCharms, 1968) or cause envy or fairness issues (Sherif, 1937; Asch, 1958; Feldman & Kirman, 1974; Fehr & Schmidt, 1999; Goeree & Yariv, 2014; Haun *et al.*, 2014) and social conformity to peers' actions would have the same set of predictions. Below we consider alternative models of behaviors - social conformity, fairness, and envy. We bring to bear some empirical tests for the

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

existence of such effects. Support for these models is limited in our data, but others may be possible. The goal of this paper is not to identify the exact behavior generating these effects, but to show theoretically and empirically that these effects can exist.

Fairness or envy. Two mechanisms that could explain the negative spillover effects are fairness or envy (Feldman & Kirman, 1974; Fehr & Schmidt, 1999). If non-incentivized people felt unfairly treated because their peers have been incentivized while they have not, or envious of their incentivized peers, they may be induced to switch from grapes to cookies after observing their peers' incentivized choice. However, we observe the largest negative effects of incentives in public tables in which all children are incentivized. Therefore, the children who switch back from grapes to cookies in these tables cannot feel unfairly treated, because they are being incentivized to pick grapes too.

Perceived value of the incentives. A possible mechanism for the negative spillover effects in tables in which most or all children are incentivized is linked to the perceived value of incentives. In these tables, most or all children who initially pick grapes are incentivized to do so. Therefore, we expect prior beliefs about the proportion incentivized to be revised up the most. This revision may reduce the perceived value of the incentives: if offered to fewer children, the awards are scarcer, and, therefore, more valuable. While possible in theory, this mechanism seems unlikely in our setting, since the incentives – bouncy balls, pens, small trophies, etc., valued roughly 50 cents – are common, easy-to-obtain items.

Social conformity. A possible mechanism for the positive spillover effects is social conformity, which occurs if children derive utility from conforming to their peers' behavior (Sherif, 1937; Asch, 1958; Goeree & Yariv, 2014; Haun *et al.*, 2014). 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. While conformity cannot explain both the positive and the negative spillover effects, 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.[25] This test is based on the premise that children want to conform differently to their best friends, to children they perceive as being popular, and to children of their own gender, than to other children. One could come up with arguments why children may want to conform either more or less to these subsets of children. Regardless of the specific case, the choices of best friends, popular kids, and children of own gender should affect ones' choice differently than the effect of the table's choices as a whole. Conversely, if behaviors are consistent with our model, then the choices of peers may be equally

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

weighted leaving the choices of best friends, popular kids, and children of own gender having no additional effect.

To test these hypotheses, 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 (3), adding variables for the table proportions of best friends (or popular kids, or children of own gender) incentivized:[26]

$$\Delta G_i = \delta_0 + \delta_1 TP_i + \delta_2 TP_i P_i + \delta_3 TP_i^{BF} + \delta_4 TP_i^{BF} P_i + \delta_5 I_i + \delta_6 P_i + \delta_7 I_i P_i + \delta_8 X_i + \epsilon_i, \quad (8)$$

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.[27] Under social conformity of the type described above, the parameter δ_3 is different from zero.

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, $TP^{BF} * I$:

$$\begin{aligned} \Delta G_i &= \theta_0 + \theta_1 TP_i + \theta_2 TP_i P_i + \theta_3 TP_i^{BF} + \theta_4 TP_i^{BF} P_i + \theta_5 TP_i^{BF} I_i + \theta_6 TP_i^{BF} I_i P_i \\ &\quad + \theta_7 I_i + \theta_8 P_i + \theta_9 I_i P_i + \theta_{10} X_i + \epsilon_i \end{aligned} \quad (9)$$

Under social conformity of the type described above, the parameter θ_6 is different from zero.

Table 6 reports the estimates from these regressions, using, alternatively, the entire sample, 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 (i.e., δ_3 and θ_6) is statistically different from zero. We interpret this evidence as being inconsistent with a theory of social conformity in which the children have preferences for conforming differently to their best friends, to the children they perceive as being popular, or to children of their own gender differently than from other children.

To conclude, the data reject the possibility that our results are explained by envy, fairness, changes in the perceived value of the incentive, or social conformity of the type described above. Other mechanisms may be possible, although our experiment was not designed to identify them.

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[26]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.

[27]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 $\frac{1}{5} = 0.2$.

The important notion is that our main conclusion that negative spillover effects can undo the positive effects of incentives does not depend on any specific mechanisms.

9   Discussion

This paper studies the spillover effects of incentives when incentives act as signals. To do that, we designed a unique experiment to decompose these spillover effects into two components: one due to peers' actions and the other due to peers' incentive status. We postulate that peers' incentive status can have negative spillover effects even if the other two effects are positive, leading to an overall effect of incentives of indeterminate sign.

We study these effects in one context in which we expect the spillover effects of incentives to be large: children's food choices – specifically, grapes versus cookie – during the school lunch. The direct effects of incentives are large, increasing grapes take-up by about 50%. However, the spillover effects of incentives are also large, especially the negative effect caused by observing peers' incentivized choices. When peer incentives are visible, the positive effect of seeing peers choose grapes is more then offset by the negative effect of seeing peers *incentivized* to pick grapes. The overall effect of incentives (i.e., combining the direct and spillover effects) is positive when half or two thirds of children are incentivized, but declines beyond that, to the point that take-up of grapes for the 100% incentivized group is not statistically different from that of the 0% incentivized group.

Our findings have several implications. First, the possibility of negative spillover effects that counteract the positive direct effects of incentives should not be overlooked. Since these negative spillover effects can occur in response to learning about peers' incentives, it may be preferable not to make incentives public, when possible, although this may not be feasible in many settings. Spillover effects of this kind may occur in environments where the value of the incentivized action is not well-known (e.g., adoption of new technologies or new behaviors), there is a tradeoff between short-run costs and long-run benefits (e.g., exercise), or pro-social behaviors are present. Second, separately measuring spillover effects and how these effects vary with the fraction treated is important for understanding how experimental results may scale-up when introduced more broadly.

Online Appendices – Not for Publication

Appendix 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.

Appendix B: Heterogeneity by child and peer gender and age

This Appendix considers 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 B1 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.

We also 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 B2 shows that we do not detect any gender or age difference in the direct effect of incentives, Table B3 shows

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 & Lavy (2009); Angrist *et al.* (2009); Croson & Gneezy (2009); Dohmen & Falk (2011)). However, this finding is not consistent in the literature (as, e.g., neither Lacetera *et al.* (2013) nor Royer *et al.* (2015) find evidence of gender differences in the effects of incentives for pro-social or healthful behavior).

In addition, Table B4 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.

Lastly, 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).

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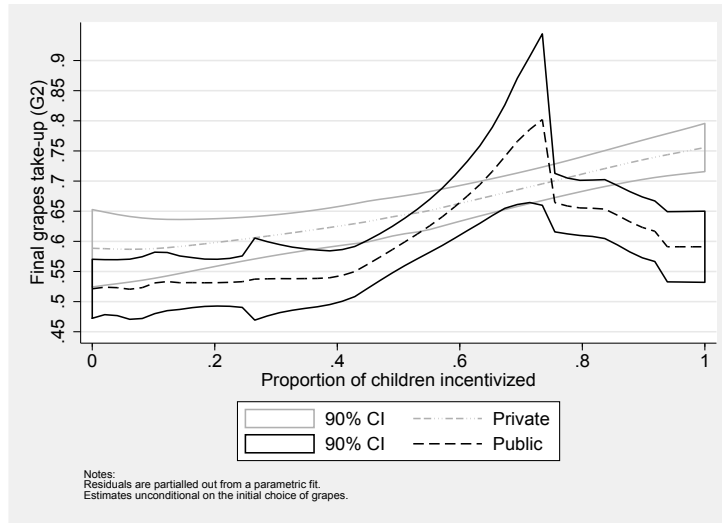Figure 1: Cookie card, fruit card, and fruit card with token.

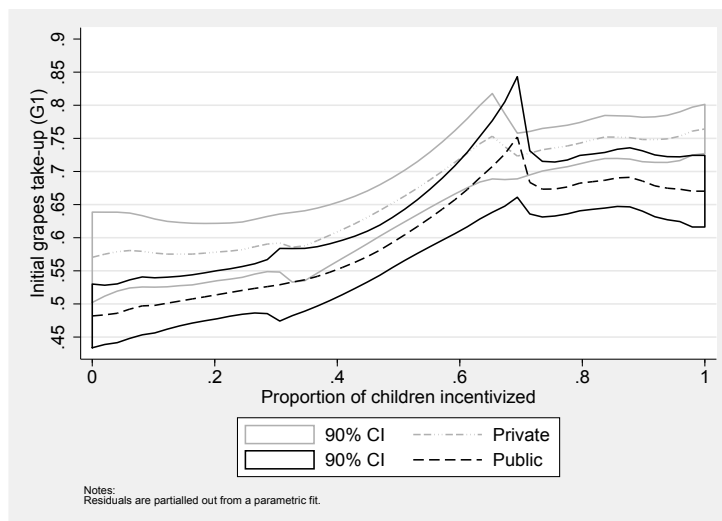Figure 2: Total effect (direct and spillover) of proportion incentivized on final grapes take-up (G_2)

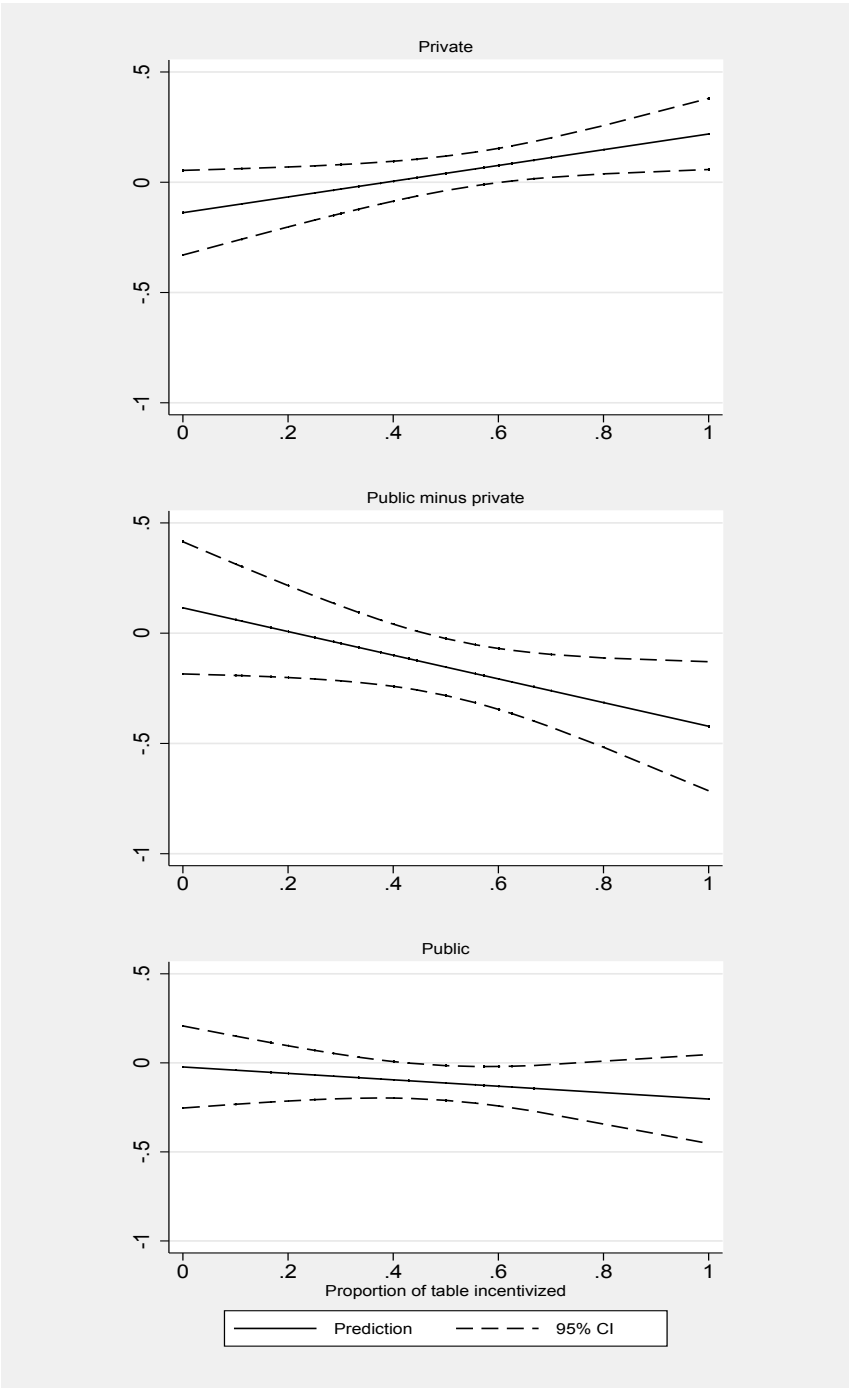Figure 3: Direct effect of proportion incentivized on initial grapes take-up (G_1)

Figure 4: Marginal spillover effects of proportion table incentivized on the conditional likelihood of ending up with grapes. Estimates from a quadratic version of equation (3)

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Table 1: Table proportion incentivized - distribution by child incentive status

| | Observations by Child Type: | |
Table Proportion Incentivized	Not Incentivized	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 2: 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 3: Direct effect of incentives (I) and public treatment (P) on initial grape choice (G_1)

$G_{1i} = \alpha_0 + \alpha_1 I_i + \alpha_2 X_i + \epsilon_i$		(1)
$G_{1i} = \lambda_0 + \lambda_1 I_i + \lambda_2 P_i + \lambda_3 I_i P_i + \lambda_4 X_i + \epsilon_i$		(2)
	Initial choice of grapes (1)	Initial choice of grapes (2)
Direct effect of incentives (α_1 in (1) or λ_1 in (2))	0.259 [0.031]***	0.241 [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]
Average take-up for non-incentivized children	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 4: Spillover effects of proportion of table incentivized on switching to grapes

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

	(1) All children	(2) % of incentivized children > 0%	(3) % 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 5: 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 6: Testing for conformity in the proportion of incentivized children

	(1) All kids	(2) % of incentivized children > 0%	(3) % of incentivized children ≥ 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: 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 B2: 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 B3: 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) 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)	0.174	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.

43

Table B4: 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)	(2)	(3)
	All children	% of incentivized children >0%	% 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.