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Incentives and Unintended Consequences: Spillover Effects in Food Choice†

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Little is known about how peers influence the impact of incentives. We study how peers' actions and incentives can lead to peer spillover effects. 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 of 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, relative to incentivizing none, has no statistically significant effect on take-up, as the negative spillover offsets the positive impacts of incentives. (*JEL* C93, D12, I21, J13)

Incentives are a cornerstone of economics. As such, they are frequently used in many domains.[1] While incentives are often successful at improving behaviors or outcomes, they may also have unintended effects. This is because incentives can act as both prices and signals. While the price effect of a higher incentive induces individuals to do the incentivized action, the direction of the signaling effect of incentives is ambiguous.

For example, incentives can be a signal about the difficulty of the task (Bénabou and Tirole 2003) or the quality of the good incentivized (e.g., Nelson 1970, Shapiro

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[1] A not nearly exhaustive list of these studies include Volpp et al. (2008, 2009); Charness and Gneezy (2009); Acland and Levy (2015); Babcock et al. (forthcoming); Babcock et al. (2015); Cawley and Price (2013); John et al. (2011); Royer, Stehr, and Sydnor (2015); Belot, James, and Nolen (2016); List and Samek (2015a,b); Loewenstein, Price, and Volpp (2016) for healthy behaviors; Angrist, Lang, and Oreopoulous (2009); Bettinger (2012); Fryer (2011); Levitt et al. (2016); Levitt, List, and Sadoff (2016) 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); and Shearer (2004) for worker effort.

1983, Milgrom and Roberts 1986). They may also make a subject feel controlled (deCharms 1968) or, more broadly, convey that the principal does not think that the agent will be intrinsically motivated to complete the incentivized task (Gneezy, Meier, and Rey-Biel 2011). Consistent with above, Gneezy and Rustichini (2000b) finds that task performance falls when small monetary incentives are offered, compared to offering no incentives.[2] Fischer et al. (2014) finds that the subsequent demand for various products decreases as their introductory price decreases.[3]

The literature on the signaling effects of incentives focuses on the effects of incentives on their recipients (the *direct* effects), neglecting the *spillover* effects of incentives. Consider paying children to choose a healthy snack. The direct effect of the incentive (i.e., absent influences from peers) may increase the chances of the children choosing 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 the choice is incentivized because, e.g., the snack is not tasty. These two types of spillover effects may differ in sign, size, and may also vary with how many of my friends are incentivized.

In sum, when we observe that incentivizing a healthy snack affects children's food choices, we are observing a combination of the direct effect of incentives, the effect of peers' actions, and the effect of peers' incentives. Since the direct and spillover effects may not necessarily have the same sign, the overall effect may differ from the direct effect. If the spillover effects vary with the fraction of peers incentivized, so will the overall effect.

The goal of this paper is to establish whether spillover effects can undo the direct effect of incentives in the context of nutritional choices. This is an environment where incentives are frequently used. For example, food subsidies, a type of incentive, are a key component of Women, Infants, and Children (WIC) and nutritional policies in developing countries (Behrman and Deolalikar 1988). In this study, we design and conduct a field experiment that lets us decompose the total effect of incentives into its direct effect and its spillover effect from peers' actions and incentives. We offer grapes and cookies and incentivize the choice of grapes versus cookies for 1,631 children in grades K–8 in a school lunchroom in a low-income Chicago neighborhood.[4]

The experiment has two stages. In stage 1, children choose grapes or a cookie simultaneously, without observing their peers' choices. We define peers as other children sitting at the lunchroom table. In stage 2, we allow children to switch their choice after observing peers' initial choices and, in some cases, peers' incentive

[2] Gneezy and Rustichini (2000a) shows further evidence of the backfiring of incentives. The introduction of fines for tardiness for day care pickups increased late pickup frequency.

[3] See Deci, Koestner, and Ryan (1999) for evidence of the signaling effects of incentives from psychology and Gneezy, Meier, and Rey-Biel (2011) and Kamenica (2012) for evidence from economics.

[4] 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 those overweight and obese.

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 of children incentivized at each table and whether a child's choice of incentivized grapes is public knowledge. Randomizing the fraction of table mates incentivized allows us to identify the spillover effects under weaker assumptions than much of the prior literature.[5]

While our experiment occurs in a controlled environment, it has some important real-life features. First, children choose their table mates, 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. Third, children often face choices between food items such as grapes and cookies. For example, in the setting studied by Belot, James, and Nolen (2016), children choose a main dish, a side, and a dessert. Fruits and vegetables are part of the side and dessert portion of the meal.

Our main finding is that making incentives visible can reduce the take-up of grapes: that is, incentives can have unintended negative spillover effects. 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. At public tables, some children in the 100 percent 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 at private tables in which all children are incentivized. In fact, we find that at private tables, the spillover effects are positive for all fractions of incentivized children. Conversely, there are nonlinear 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 and statistically significant

[5] 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 and De Giorgi 2009; Barrera-Osorio et al. 2011; Bobonis and Finan 2009; Duflo and Saez 2003; Lalive and Cattaneo 2009; and Guiteras, Levinsohn, and Mobarak 2015). Second, by using plausible exogenous variation in the fractions of peers treated (e.g., Babcock et al. forthcoming, Beaman 2012, Conley and Udry 2010, Duflo and Saez 2002, and Munshi 2003). Lastly, by looking at differential treatment effects within a predetermined peer group (e.g., Banerjee et al. 2013; Chen, Humphries, and Modi 2010; Macours and Vakis 2008; and Oster and Thornton 2012).

when we incentivize up to two-thirds of children. The overall effect becomes statistically insignificant when we incentivize all children.[6]

Imagine that our experiment consisted of stage 1 only—that is, we randomly offered incentives and 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 at tables with 0 and 100 percent 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.

A negative spillover effect of peers' incentive status is consistent with the hypothesis that the act of providing incentives, at least in this context, has negative psychological non-price effects (i.e., incentives act as a signal between the principal and the agent). The effects of peers' incentives do not appear to be driven by envy or fairness issues; 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 changes in the perceived value of the prizes. Lastly, our findings are not consistent with social signaling as an explaining factor, as the direct effect of incentives does not vary when incentives are public or private.

In sum, our experiment shows that spillover effects can be large, positive, or negative (depending on the relative salience of peers' action and incentive status), and big enough to offset any direct effects of incentives. Taking the nature of spillover effects into consideration may affect the design of nutrition programs, and of incentive-based policies more broadly, and increase the programs' effectiveness.

I. Experimental Design

To measure the direct and spillover effects of incentives, we designed an artefactual field experiment (Harrison and List 2004) in which we offered grapes and cookies to children and randomly offered incentives to choose grapes.[7] This experiment took place in school cafeterias during lunch. All nine elementary schools in Chicago Heights, IL, participated. Lunch is administered in much the same way in each of these schools. Depending on their size, schools hold either two, three, or four lunch periods each day, assigning kids to periods based on their grade (e.g., fourth, fifth, and sixth 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, children can typically sit with any other children from their grade and tend to form groups of 3–10 children at each table.

[6] These findings echo the results in Bursztyn and Jensen (2015) and Bursztyn, Egorov, and Jensen (2019), who find that take-up rates of a SAT preparatory course are lower when take-up is observable.

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

In this school district, children do not have a choice about what foods to have in their lunch. Moreover, Chicago Heights, IL, 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 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 (online Appendix A), which described the procedures of the experiment. To ensure that the children had understood the instructions, children were asked a series of questions and were shown how cards could be played. 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 received two cards—one grape card (some of which had incentives) and one cookie card. In round one, children simultaneously chose grapes or cookies by placing one of the cards on the table. All cards were placed on the table at the same time and we have no recorded instances of children deviating from this instruction. After observing their table mates' choices and, depending on the treatment, their table mates' incentives, children again simultaneously played 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 20 seconds to play a different card after having observed their peers' choices.

We randomized whether a child received an incentive to choose grapes, the fraction of children at each lunch table who received an incentive to select grapes, and at which tables choosing incentivized grapes were visible to 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 percent of cards with incentives. Because the incentivized cards were randomly stacked in the decks and the number of occupants fluctuated by table, the actual fraction of children receiving incentives at tables with 50 percent of cards incentivized varied between 11 and 80 percent in the 50 percent condition. Table 1 shows 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 drew, and a poster with all possible prizes was displayed to the kids. The value of each prize was roughly 50 cents. The prizes

Figure 1. Cookie Card, Fruit Card, and Fruit Card with Token

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

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. We focused on relatively common items to reduce the possibility that a child would drive utility from a prize's uniqueness.

If children 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 played 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, however, children played 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 whether 0, 50, or 100 percent of the cards for a table is incentivized, and 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 percent 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 percent 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 percent 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 percent 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.

Within each school by lunch period, we randomized the assignment of each lunch table such that half of tables were allocated to the 0 percent and 100 percent treatments, and the remaining half was designated to the 50 percent treatment. We cross randomized the public and private treatment such that each treatment was equally represented in the 0 percent, 50 percent, and 100 percent treatments.

We recorded both the initial food choice, G_1, and the final choice, 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 observing peers' choices and incentives.

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 five 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.

Our experimental design makes advances in the peer effects literature along four 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 effect with decomposing them (as in, e.g., Royer, Stehr, and Sydnor 2015) or the direct effect only, without studying how incentives would affect behavior once spillover effects are allowed to operate (as in, e.g., Fryer, Levitt, and List 2008).

Second, by randomly varying the fraction of treated peers and allowing children to switch snacks after observing their peers' actions and incentives, we can measure spillover effects on both incentivized and non-incentivized children. 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 and 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 and 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 nonlinearities in spillover effects. In many papers that measure spillover effects, this variation is not random. For example, in Babcock et al. (forthcoming), 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.

II. The Data

A. *Sample*

A total of 1,771 children participated in the experiment. A total of 1,286 (73 percent) children filled out the questionnaire. We exclude tables of size 10 or larger

Table 2—Descriptive Statistics by Group

Group	Observations	Table size	Percent 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	1,631	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

Note: Standard deviations are 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.

(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.

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.[8] The size of each treatment group varies because some of the tables in the cafeteria were empty.

B. *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 percent of children at each table are African American and 52 percent are Hispanic. Eighty-seven percent of the children at each table are on the free lunch program (and 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 eight group dummies, when regressed on each of these variables together with school-by-period

[8] Nonparticipation 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.

strata. None of the F-tests are significant at conventional levels, which is consistent with random assignment.

We also check for balance using the actual fraction of children incentivized as opposed to the discrete groups considered in Table 2. Recall that the grape cards for the 50 percent incentivized tables were drawn from a deck where half of the cards were incentivized, the actual fraction incentivized deviated from 50 percent. 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 no reduction), 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 sixth 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.

III. 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.

Children's initial beliefs of the proportion of peers incentivized may factor into their first round choice, G_1. 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 choose grapes and how many of those choices are incentivized. Conceivably, since most children's initial belief is that a fraction of children are incentivized, beliefs are more likely to change more at tables in which either zero or all the children are incentivized. In addition, we expect that, at 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.[9] While the total effect of incentives grows with the table

[9] 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)

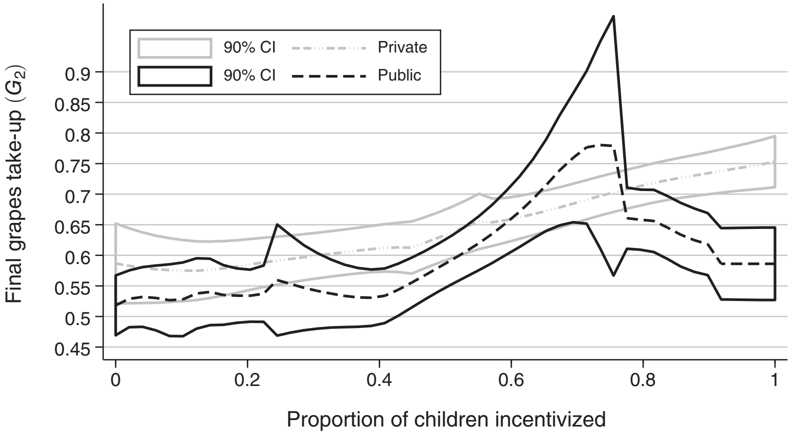

Figure 2. Total Effect (Direct and Spillover) of Proportion Incentivized on Final Grapes Take-up (G_2)

Notes: Residuals are partialled out from a parametric fit. Estimates are unconditional on the initial choice of grapes.

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 grape take-up between tables with 0 and 100 percent incentives. Note that, while we have few tables with 60 to 80 percent incentivized children (hence the larger confidence intervals), grapes take-up among tables with 60 to 80 percent incentivized differs statistically from tables with both 0 percent and 100 percent incentivized children.[10]

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 grape take-up where all children

and then smooth the effect of incentive proportion on final grape choice using a local linear regression with a Epanechnikov kernel and a rule-of-thumb bandwidth (which gives bandwidths of 0.24 and 0.12 for private and public treatments). The results are robust to both changes in the kernel and varying the bandwidth by increasing by one-half or decreasing it by one-third, 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 (which have similar bandwidths of 0.21 and 0.12 for private and public treatments and are equally robust to bandwidth changes of the magnitudes described above).

[10] 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.

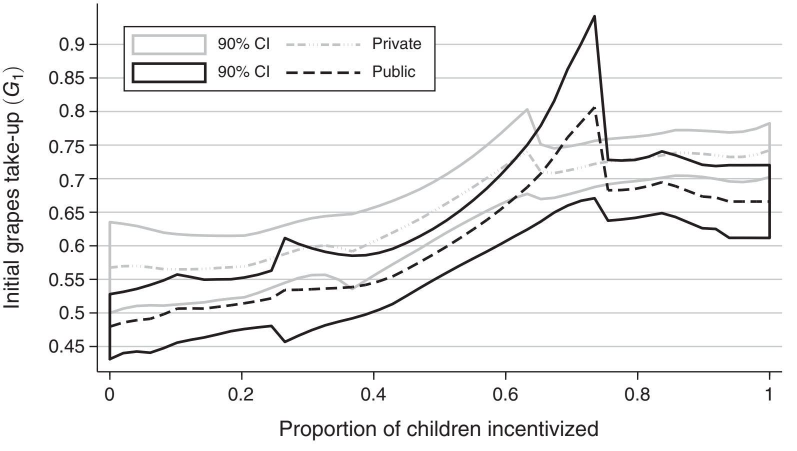

Figure 3. Direct Effect of Proportion Incentivized on Initial Grapes Take-up (G_1)

Notes: Residuals are partialled out from a parametric fit.

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, Meier, and Rey-Biel 2011) or make the short-term costs of picking grapes over a cookie more prominent.[11]

To conclude, for the public treatment, Figures 2 and 3 show direct and spillover effects of opposite signs: as conjectured, public incentives can work less well than intended. Our next step is to decompose the total effect of incentives into its direct effect and spillover effects and to study their sign and magnitude.

IV. Direct Effects of Incentives

We measure the direct effect of incentives on grape choice, that is, whether receiving the incentives change the recipients' likelihood of initially choosing grapes. We do this by comparing the *initial* grape choice of incentivized and non-incentivized children.[12] 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;

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

[11] 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.

[12] 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 percent of the sample makes a switch between the initial and final choice.

The coefficient α_1 identifies the average treatment effect of incentives on initial grape choice. This parameter is identified under the assumptions that the variable I and the error term ϵ are independent, which follows from random assignment, and that one child's potential outcomes are unaffected by the treatment status of others, which follows from 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 whether the direct effects of incentives are identical in the public and private treatment $(\lambda_3 = 0)$ and 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 percent, compared to a 49.5 percent take-up rate among non-incentivized children.

These findings are comparable in size to some related work. Just and Price (2013) increased children's consumption of salad by 80 percent after offering up to \$0.25 (or a lottery ticket with the same expected value). The incentives in List and Samek (2015a,b) led to a two- to four-fold increase in the choice of healthy snacks. Our effects exceed those in Belot, James, and Nolen (2016), who offer piece-rate incentives to choose an extra vegetables side dish and find 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_3 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)). Figure 3 shows that this happens regardless of the table fraction incentivized, including at tables with no incentives. These findings are consistent with the idea that making incentives salient signals "bad news" or highlights the short-term cost of picking grapes over cookies over its long-term benefit, as we noted before. Moreover, 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 VIIC, 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.

V. 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.

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	0.241
	[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]
Average take-up for non-incentivized children	0.495	0.495
Number of observations (children)	1,631	1,631

Notes: Column 1 depicts OLS estimates of equation (1) listed on the table, and column 2 depicts OLS estimates of equation (2) listed on the table. Standard errors are clustered by tables. Regressions control for school-by-period strata, table size, grade, sex, race, and lunch type.

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:

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

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. Parameters 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.

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 (standard error 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 (standard error 0.08). Therefore, the

TABLE 4—SPILLOVER EFFECTS OF PROPORTION OF TABLE INCENTIVIZED ON SWITCHING TO GRAPES

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

	All children (1)	Percent of incentivized children $> 0\%$ (2)	Percent of incentivized children $\geq 50\%$ (3)
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	0.50	0.66	0.80
Observations (children)	1,631	1,233	872

Notes: OLS estimates control for school-by-period strata, table size, grade, sex, race, and lunch type. Standard errors are clustered by table.

net spillover effect of public incentives (i.e., the effect from increasing the table proportion who is incentivized), in the third row, is negative ($-0.09 = 0.09 - 0.18$; standard error 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.

The table proportion incentivized, *TP*, is a policy-relevant variable, as it is under the control of the policymaker. However, children do not necessarily observe *TP*. At private tables, children observe no incentives (but know that they exist), while at public tables they observe the fraction of children *playing* grapes in the first stage, which is a subset of *TP*.

As a robustness check, we can regress the change in choice, ΔG, on the fraction of children initially *playing* grapes, *TG*, using *TP* as an instrument since *TG* is endogenous. The resulting 2SLS estimate of the *TG* coefficient is -0.12 (standard error 0.06).[13] This implies that a 1 percentage point increase in the observed fraction of incentivized grapes in the first stage reduces the likelihood of switching to grapes by 0.12 percentage points. This point estimate is of comparable magnitude to the -0.18 effect (β_2) from Table 4, confirming our earlier results.

Nonlinearities.—The specification highlighted in equation (3) models the effect of the proportion incentivized as being linear. We also consider possible nonlinearities

[13] The first stage F-statistic is 701.70.

by truncating the sample and by using a quadratic function of the table proportion incentivized. Both approaches yield a similar message: nonlinearities 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 percent of incentivized children (column 3), in which case the average table proportion incentivized increases from 50 percent to 66 percent (column 2) and to 80 percent (column 3).[14] 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) by adding the square of the table proportion incentivized and interacting it with the public dummy: $\beta_4 TP_i^2 + \beta_5 TP_i^2 \times P_i$. Figure 4 shows the marginal effects of fraction incentivized from this equation (i.e., estimates of $\beta_1 + \beta_2 \times P_i + 2\beta_4 TP_i + 2\beta_5 TP_i \times P_i$). 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 with the table proportion incentivized. The marginal effects become statistically different from zero when 40 to 50 percent of the table is incentivized.

Other Checks.—Equation (3) includes 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 $G_{2i} = \beta_0 + \beta_1 TP_i + \beta_2 TP_i \times P_i + f(\beta_{IPG} I_i P_i G_{1i}) + \beta_3 X_i + \epsilon_i$, where the term $f(\beta_{IPG_1} I_i P_i G_{1i})$ is the sum of all the interactions of the incentive treatment, public treatment, and initial grape choice dummies.[15] In online 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.

VI. 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

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

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

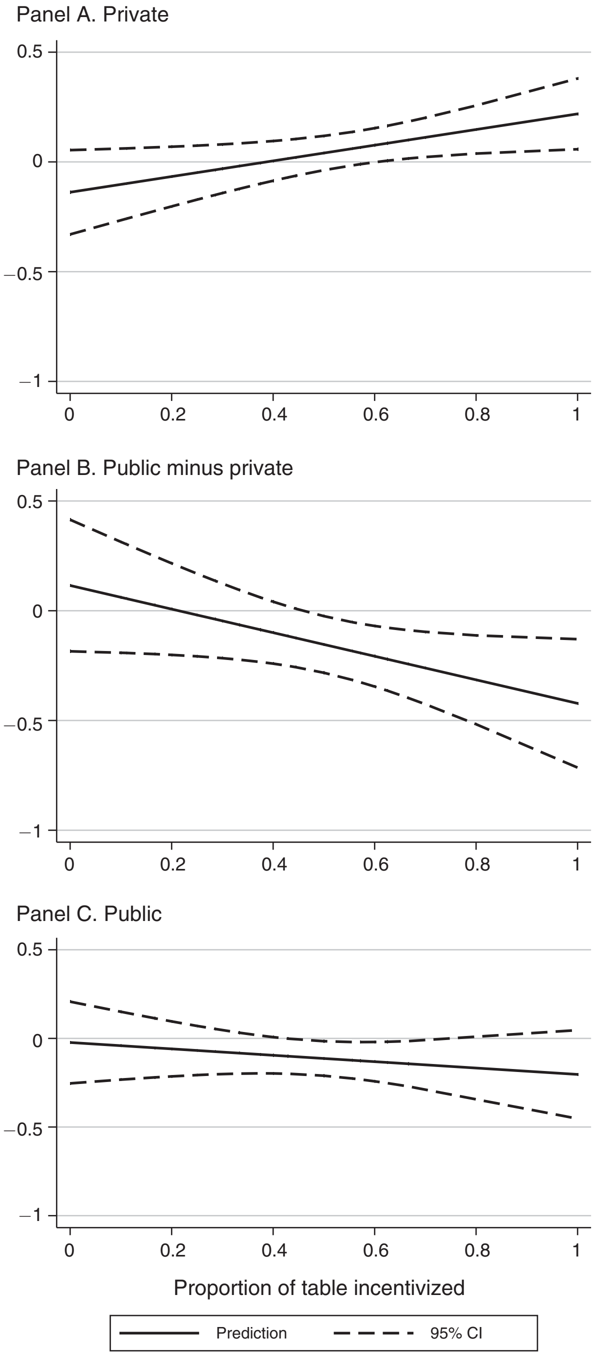

Figure 4. Marginal Spillover Effects of Proportion Table Incentivized on the Conditional Likelihood of Ending up with Grapes

Note: Estimates from a quadratic version of equation (3).

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 percent (Table 4, row 1). Therefore, the fraction incentivized that maximizes the likelihood of ending up with grapes in the private treatment is 100 percent, 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 most settings, 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, observing that some peers 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 percent (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 percent and by 0.16 (0.26 + 0.12 − 0.22) percentage points when the fraction incentivized is 66 percent but reduces grapes take-up by −0.03 (0.26 + 0.16 − 0.45) percentage points when the fraction incentivized is 80 percent. 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.

VII. 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 percent incentivized tables is not statistically different from the fraction selecting grapes among the 0 percent incentivized tables, while smaller 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 are consistent to theory. 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. The agent is more likely to make a choice if her peers have done it as well, as in Banerjee (1992). A principal may incentivize the choice to make a specific action more appealing to the agent. However, this incentive may also signal that the action is costly to the agent, and the principal may be uncertain about this negative signaling effect of the incentive on the agent, as in Bénabou and 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 (e.g., the quality or the cost of undertaking) or the principal's objectives. As we show below, 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:

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

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 social acceptability, relative health benefits, quality, or taste and/or the objectives of the experimenter. The child observes the behavior of her peers and of the experimenter to infer their private information.

For peers' actions and incentives to influence the child's behavior, we require the child to believe that the experimenter, her peers, or both have private information about the choice. In such a setting, observing peers' choices and incentives can change the child's beliefs about the relative value of grapes and cookies.[16]

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 over cookies, $\bar{G}_{-i}$, and, in some cases, also the fraction of her peers who choose incentivized grapes, $\bar{I}_{-i}$. Here, $\bar{I}_{-i}$ 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

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

[16] Our setting does not require the experimenter and peers to have more or better information than the child, or any actual private information. Similarly, it does not necessitate that the child have no information about grapes or cookies. 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, or what the social value of picking a snack over the other is. They may, therefore, believe that they can learn about these features of the two goods by observing peers' behavior. 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. Or alternatively, the experimenter may have the goal of inducing more children to choose healthy options, so the provision of incentives may act as a signal of the strength of that intention.

A. *Direct Effect of Incentives*

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

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

The first right-hand side term, $\partial B_i/\partial I$, is the effect of introducing (or increasing) incentives on the child's belief about the relative value of grapes over cookies. The sign of this effect is indeterminate. For example, in Bénabou and 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 or that the experimenter wants to control the choices of the children.[17] This may make her revise her prior beliefs about the benefits of grapes downward. However, 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.[18]

Conversely, the second right-hand side term, $\partial C/\partial I$, which represents the effect of the incentives on cost, is negative, as compensating the child to pick grapes over cookies reduces its cost. In sum, the sign of the direct effect is unknown, due to the ambiguity of the sign of $\partial B_i/\partial I$. In our application, we estimated it to be positive.

B. *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, $\bar{I}_{-i}$:

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

The first right-hand side term, $(\partial B_i/\partial \bar{G}_{-i})(\partial \bar{G}_{-i}/\partial TP)$, is the spillover effect of incentives arising from watching others pick grapes and has an indeterminate sign theoretically. The sign of $\partial B_i/\partial \bar{G}_{-i}$ is positive if an increase in the proportion picking grapes sends a positive signal about the value of grapes.[19] Therefore, the sign of this first term depends on how increasing the proportion incentivized affects the

[17] In Bénabou 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 the unpleasant tasks.

[18] 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.

[19] The sign of $\partial B_i/\partial \bar{G}_{-i}$ 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 $\partial B_i/\partial \bar{G}_{-i}$ is positive in our data, so modeling this option is not essential in this application.

TABLE 5—DIRECT, SPILLOVER, AND OVERALL EFFECTS OF INCENTIVES

	Sign of direct effect	Spillover effect $\left(\frac{\partial B_i}{\partial \bar{G}_{-i}} \times \frac{\partial \bar{G}_{-i}}{\partial TP}\right)$		$\left(\frac{\partial B_i}{\partial \bar{I}_{-i}} \times \frac{\partial \bar{I}_{-i}}{\partial TP}\right)$	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 $-)$

proportion picking grapes initially, $\partial \bar{G}_{-i}/\partial TP$. This has the same sign as the direct effect of incentives.

The second right-hand side term, $(\partial B_i/\partial \bar{I}_{-i})(\partial \bar{I}_{-i}/\partial TP)$, 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 $\partial B_i/\partial \bar{I}_{-i}$ depends on how children interpret the experimenter's intent to incentivize children to pick grapes and, therefore, has the same sign as $\partial B_i/\partial I$. Note that $\partial \bar{I}_{-i}/\partial TP$ 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 three cases, also summarized in Table 5.

Case 1: Incentives send a weakly positive signal on the value of grapes $(\partial B_i/\partial I \geq 0)$. When this happens, the direct effect of incentives is positive, as $(\partial B_i/\partial I) - (\partial C(I)/\partial I) > 0$. If the direct effect is positive, then increasing the proportion incentivized increases the proportion choosing grapes, incentivized or not, $(\partial \bar{G}_{-i}/\partial TP > 0$ and $\partial \bar{I}_{-i}/\partial TP > 0)$. 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, $\partial B_i/\partial \bar{I}_{-i} \geq 0$, and, therefore, the two spillover effects of incentives are also positive. That is, in this case, the spillover effects through peers' actions and incentive status reinforce the direct effects.

Case 2: Incentives send a negative signal on the value of grapes $(\partial B_i/\partial I < 0)$, but the direct effect is positive, because the cost reduction more than offsets the negative signal for incentivized children, $\partial B_i/\partial I > \partial C(I)/\partial I$. 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, $\partial B_i/\partial \bar{I}_{-i} < 0$. Moreover, if the direct effect is positive, increasing the proportion incentivized increases the proportion choosing grapes, incentivized or not, $(\partial \bar{G}_{-i}/\partial TP > 0$ and

$\partial \bar{I}_{-i}/\partial TP > 0)$. 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 $(\partial B_i/\partial I < 0)$ and the direct effect is negative because the cost reduction is offset by the negative signal for incentivized children, $\partial B_i/\partial I < \partial C(I)/\partial I$. 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, $\partial B_i/\partial \bar{I}_{-i} < 0$. Moreover, if the direct effect is negative, then increasing the proportion incentivized reduces the proportion choosing grapes, incentivized or not, $(\partial \bar{G}_{-i}/\partial TP < 0$ and $\partial \bar{I}_{-i}/\partial TP < 0)$. 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 three broad conclusions. First, incentives may have both a positive and a negative direct effect.[20] Second, when incentives are "bad news" $(\partial B_i/\partial I < 0)$, 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.

C. *Alternative Models*

We can generate predictions consistent with our empirical results without assuming that the child believes the experimenter or peers to have private information. A model with the following assumptions would generate the same predictions: there is a short-term cost of choosing grapes over cookies; there is a long-term benefit of selecting grapes over cookies; and the public incentive makes the short-term cost of choosing grapes more prominent, while observing peers pick grapes makes the long-term benefit of grapes more salient. In the derivations above, it would only change the interpretation of the part of the spillover effect attributable to the fraction of peers incentivized. Specifically, $\partial B_i/\partial \bar{I}_{-i}$, the perceived benefit of choosing grapes as a function of the fraction of the public incentive group choosing incentivized grapes would be negative because the public incentive makes the short-term cost of choosing grapes more salient.

There are additional 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 and Kirman 1974; Fehr and Schmidt 1999; Goeree and Yariv 2015; and Haun, Rekers, and Tomasello 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

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

bring to bear some empirical tests for the 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 and Kirman 1974, Fehr and 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 at tables in which most or all children are incentivized is linked to the perceived value of incentives. At 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 at these tables. 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 at roughly $0.50—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 and Yariv 2015; and Haun, Rekers, and Tomasello 2014). Since incentives increase initial grape take-up, the larger the initial take-up, the more children will want to conform, by 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.[21] 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 weighted leaving the choices of best friends, popular kids, and children of own gender to having no additional effect.

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

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:[22]

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

where the variable TP^{BF} is the table proportion of incentivized best friends (or a dummy variable for incentivized popular kids, or children of own gender), while the other variables are as discussed before.[23] 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, $TP^{BF} \times I$:

$$\Delta G_i = \theta_0 + \theta_1 TP_i + \theta_2 TP_i P_i + \theta_3 TP_i^{BF} + \theta_4 TP_i^{BF} P_i + \theta_5 TP_i^{BF} I_i + \theta_6 TP_i^{BF} I_i P_i + \theta_7 I_i + \theta_8 P_i + \theta_9 I_i P_i + \theta_{10} X_i + \epsilon_i. \tag{9}$$

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. 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.

[22] 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.

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

TABLE 6—TESTING FOR CONFORMITY IN THE PROPORTION OF INCENTIVIZED CHILDREN

	All kids (1)	Percent of incentivized children $>$ 0% (2)	Percent of incentivized children $\geq$ 50% (3)
Panel A. Effect of same-gender incentivized kids			
Spillover effect of peers choosing grapes			
Effect of table proportion incentivized (δ_3)	−0.010	0.006	−0.082
	[0.073]	[0.075]	[0.070]
Spillover effect of peers choosing incentivized grapes			
Effect of table proportion of group incentivized × incentive × public (θ_6)	−0.265	−0.069	−0.044
	[0.185]	[0.207]	[0.243]
Observations (children)	1,187	895	636
Panel B. Effect of incentivized best friends			
Spillover effect of peers choosing grapes			
Effect of table proportion incentivized (δ_3)	−0.023	−0.017	−0.123
	[0.060]	[0.063]	[0.094]
Spillover effect of peers choosing incentivized grapes			
Effect of table proportion of group incentivized × incentive × public (θ_6)	0.062	0.193	0.041
	[0.137]	[0.148]	[0.206]
Observations (children)	814	616	438
Panel C: Effect of incentivized popular kids			
Spillover effect of peers choosing grapes			
Effect of table proportion incentivized (δ_3)	−0.020	−0.035	0.001
	[0.056]	[0.059]	[0.047]
Spillover effect of peers choosing incentivized grapes			
Effect of table proportion of group incentivized × incentive × public (θ_6)	0.030	0.083	0.221
	[0.151]	[0.157]	[0.172]
Observations (children)	590	455	323

Notes: We report the estimates of the 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. Sample restricted to observations with data on best friends and popular kids. We also keep only children with best friends sitting at their table (for panel B) or with popular kids sitting at their table (for panel C).

VIII. 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 spillover effects in the context of children's food choices—specifically, grapes versus cookie—during school lunch. The direct effects of incentives are large, increasing grape take-up by about 50 percent. 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 than 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 to two-thirds of children are incentivized, but declines beyond that, to the point that take-up of grapes for the 100 percent incentivized group is not statistically different from that of the 0 percent incentivized group.

A. *Application of Our Results to Other Studies*

To gauge the importance of our findings, we take our estimates, apply them to related nutritional interventions, and postulate how the results in those related studies would have differed under two counterfactual scenarios: making the incentives private as opposed to public, and reducing the proportion incentivized from 100 percent to 66 percent. In our experiment, each of these scenarios increased grape take-up by 31 percent. Consider Just and Price (2013), who incentivize children to eat fruits and vegetables. In that paper, treating 100 percent of children publicly resulted in 48–62 percent of children choosing to eat at least one serving of fruits or vegetables (depending on the type of incentive considered). We would predict that take-up would increase to 63–88 percent under either of the counterfactual scenarios. In List and Samek (2015a), who leverage behavioral economics to encourage healthy food consumption among children, treating 100 percent of children publicly resulted in 84 percent of children choosing the healthier dried fruit cup over the cookie. All children would have chosen the dried fruit cup if they had treated only 66 percent of the children or they had made the incentives private. In Loewenstein, Price, and Volpp (2016), who examine whether a short-run incentive leads to habit formation, publicly incentivizing all children resulted in 80 percent of children choosing at least 1 serving of fruits and vegetables. Similar to the List and Samek (2015a) study, our two counterfactual scenarios would have resulted in complete take-up. Under the second counterfactual scenario, one might expect that significant cost savings would accrue due to the fewer proportion of children incentivized. However, the increase in the number of children taking up the healthier option (and therefore receiving an incentive) actually causes the overall incentive program to be slightly more expensive (between \$1–\$6 for every 100 children).

In sum, successful interventions could have even larger impacts under our counterfactuals scenarios. We speculate that, similarly, unsuccessful interventions may become effective by either switching from public to private incentives or reducing the fraction incentivized.

B. *Implications of Our Work*

These findings may contribute to the discussion about the use of incentives in the health domain. Our findings highlight the importance of considering the signaling/psychological effects of such incentives. Under the Affordable Care Act, employers are permitted to tie more of the cost of health insurance premiums to health behaviors, and, consequently, there has been tremendous growth in the number of employers incentivizing employees to lose weight, exercise, and stop smoking.

Such efforts have had limited success (Jones, Molitor, and Reif 2019)—perhaps in part to the non-price effects of these incentives. That is, employees may think that the firms have ulterior motives (e.g., attracting new healthy employees versus making the existing pool of employees healthier) or feel controlled or manipulated.

More specific to our context, incentives are commonplace in nutritional policy. Food subsidies are often used to encourage healthy food consumption. The federal program Women, Infants, and Children (WIC) provides recipients vouchers to pay for certain nutritional items. As only specific items are WIC-eligible, stores often label these WIC products to help recipients find them. If our results hold in this environment, it is possible that making these incentives public may lead to fewer non-WIC-recipients purchasing these goods if there is a perception that WIC products are of low quality or only for low-income individuals. Similarly, developing countries often subsidize certain foods (e.g., rice in China), and researchers studying such policies (e.g., Jensen and Miller 2008) often consider the possibility that those price reductions have signaling effects and undo their intended effects.

More generally, our results are also informative about incentive-based randomized-controlled trials. First, to understand the full impact of incentives, consideration of the spillover effects is essential. Ignoring the spillover effects might result in inefficient policy recommendations because the direct and spillover effects could possibly offset each other. 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, Sacerdote, and Scheinkman 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).[24] 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 table mates but zero when we incentivize all children. The existence of nonlinearities 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. Third, an entity without the funds or resources may benefit from incentivizing some and not all individuals. Fourth, when incentivizing behavior, the motivation behind providing incentives should be taken into careful consideration as the act of providing incentives may act as a signal. Fifth, 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, 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.

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

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