Public Health Nutrition: 19(3), 520–529 doi:10.1017/S1368980015001469

Buying health: assessing the impact of a consumer-side vegetable subsidy on purchasing, consumption and waste

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Submitted 19 November 2013: Final revision received 6 March 2015: Accepted 11 March 2015: First published online 9 June 2015

Abstract

Objective: To measure the impact of a reimbursement-based consumer subsidy on vegetable expenditures, consumption and waste.

Design: Two-arm randomized controlled trial; two-week baseline observation period, three-week intervention period. Participants' vegetable expenditures, consumption and waste were monitored using receipts collection and through an FFQ. During the intervention period, the treatment group received reimbursement of up to 50 US dollars ($) for purchased vegetables.

Setting: Participants were solicited from Palo Alto, CA, USA using materials advertising a 'consumer behavior study' and a small participation incentive. To prevent selection bias, solicitation materials did not describe the specific behaviour being evaluated.

Subjects: One hundred and fifty potential participants responded to the solicitations and 144 participants enrolled in the study; 138 participants completed all five weekly surveys.

Results: Accounting for the control group (n 69) and the two-week baseline period, the intervention significantly impacted the treatment group's (n 69) vegetable expenditures (+$8·16 (SD 2·67)/week, $P < 0·01$), but not vegetable consumption (+1·3 (SD 1·2) servings/week, $P = 0·28$) or waste (–0·23 (SD 1·2) servings/week, $P = 0·60$).

Conclusions: The consumer subsidy significantly increased participants' vegetable expenditures, but not consumption or waste, suggesting that this type of subsidy might not have the effects anticipated. Reimbursement-based consumer subsidies may therefore not be as useful a policy tool for impacting vegetable consumption as earlier studies have suggested. Moreover, moderation analysis revealed that the subsidy's effect on participants' vegetable expenditures was significant only in men. Additional research should seek to determine how far reaching gender-specific effects are in this context. Further research should also examine the effect of a similar consumer subsidy on high-risk populations and explore to what extent increases in participants' expenditures are due to the purchase of more expensive vegetables, purchasing of vegetables during the study period that were consumed outside the study period, or a shift from restaurant vegetable consumption to grocery vegetable consumption.

Keywords
Public health
Vegetable subsidy
Behavioural psychology
Price intervention

The ongoing obesity epidemic has spurred a significant body of research examining the impact of food cost on food purchasing and consumption. Both modelling and natural studies have observed strong relationships between the price of healthy goods and consumer purchasing of those goods[1–8]. These results have been supported by research from randomized controlled trials[9–15], which remain the gold standard in experimental reliability[16].

Most of the existing randomized controlled trial research has concentrated on examining the impact of various point-of-purchase price manipulations on food purchasing[10–18]. These studies mimic the many environmental and economic effects that influence food prices as well as the various government interventions – such as producer-side subsidies and taxes – that can raise or lower food prices[19].

Less research has been conducted on the impact of consumer-side subsidies. Randomized controlled trials to examine consumer subsidies have largely, but not exclusively, been structured around existing programmes, such as WIC (Special Supplemental Nutrition Program for Women, Infants, and Children) vouchers[6,9,19–26]. Vouchers and coupons, however, are not the only means

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by which a consumer-side subsidy can be provided. Tax rebates, which subsidize through reimbursement, may serve as an additional existing avenue for subsidizing consumers.

Moreover, most of the existing research examines purchasing or consumption, with only a select few looking at the two in conjunction[6,9,18,21]. Yet purchasing, although objectively measurable, is an imperfect proxy for consumption. Consumption data may be even less reliable due to the reliance on participant self-reporting. Food waste presents an additional variable that, when considered in conjunction with purchasing and consumption, may help explain participant behaviour. But no existing food subsidy research assesses food waste together with purchasing and consumption. Unsurprisingly, researchers have called for more research into consumer subsidies[14–17,27,28].

The present randomized controlled trial expands on the previous research by assessing a reimbursement-based consumer subsidy specifically directed at vegetables. In addition to assessing the intervention's impact on participants' vegetable expenditures, the present study also examines its impact on vegetable consumption and waste.

Methods

Participants were solicited from Palo Alto, CA, USA using posters and emails advertising a 'consumer behavior study' over a one-week period. Posters were placed in high-traffic areas of Palo Alto such as parking garages and residential development common areas, and emails were sent to community email lists. Study participation was promoted by offering a cash incentive of 10 US dollars (hereafter, $) and a chance to win one raffled iPad2. Solicitation materials explained that participation required weekly completion of surveys and receipt collection, but, to prevent selection bias, did not describe the specific behaviour evaluated. Demographic characteristics were collected via a short online survey emailed before the study period. Participants self-identified according to age, gender, race and whether they financially supported another individual (partner, child or family member). All prospective participants who completed either the demographic survey or the first weekly consumption survey were enrolled.

The study was conducted according to the guidelines laid down in the Declaration of Helsinki and all procedures involving human subjects/patients were approved by the Stanford IRB (Protocol 21452). Written informed consent was obtained from all participants.

The primary outcome measure, vegetable purchasing, was estimated in $ per week expenditures using annotated receipts from supermarkets, convenience stores, farmers' markets and other vendors of vegetables. This method (collecting receipts) has previously been validated and found to be an effective measure of household food purchases[29]. The varied nature and many sources of the receipts did not allow for vegetable purchases to be measured in grams or servings. The secondary outcome measures, vegetable consumption and waste, were estimated in servings per week from the Fred Hutchinson Cancer Center FFQ, a validated FFQ adapted from the Women's Health Initiative and other studies[30–33]. Both individual and aggregated vegetable expenditures, consumption and waste were tracked using the grocery receipts and FFQ data.

The study employed a baseline to intervention period, control to treatment group comparison: the subsidy's impacts were measured by comparing changes in the treatment group's weekly vegetable expenditures, consumption and waste from the baseline to the intervention period with changes in the control group's weekly vegetable expenditures, consumption and waste. The baseline period was two weeks and the intervention period lasted three weeks, which are standard time periods that have been shown to be sufficient for price intervention research[9,34,35].

The intervention consisted of reimbursement of up to $50 total for vegetable purchases made over the three-week intervention period, after a $10 floor of spending had been exceeded. The treatment group was informed only after the second week of the baseline period that they would receive this reimbursement. In addition to collecting receipts, participants provided a weekly estimate of their spending on all vegetables throughout the study. The $10 subsidy floor and $50 ceiling were determined from participants' baseline vegetable expenditure estimates and were tailored to maximally impact participants' marginal spending: the floor was set to exclude fewer than 5 % of participants and was rounded to the nearest $5; the subsidy ceiling was set at the highest level allowed by the study budget, and was also rounded to the nearest $5[5].

Participants collected their grocery receipts in provided pre-addressed and postage-paid envelopes throughout the duration of the study and returned the envelopes to the researcher for coding at the conclusion of the study. Reimbursement was provided only for vegetables that were not pre-cooked or otherwise processed (e.g. restaurant food was not reimbursed nor were vegetable flours, oils, sauces made from vegetables, etc.). Although participants were permitted to black out other identifying personal information on their submitted receipts, they were required to leave their names in order to prevent participants from claiming reimbursements for the purchases of others.

Participants reported their vegetable consumption and waste on the vegetable portion of the FFQ, which they accessed through a unique link emailed at the same time each week throughout the study. Unresponsive participants received email reminders on three consecutive days at the end of each week before that week's link expired. Participants who did not complete all weekly surveys before the close of the survey period were excluded from

the study and are not included in the analysis. The FFQ was modified only to add a question asking how many servings of each vegetable were thrown out or composted that week[36,37].

All participants self-identified as male or female on the initial demographic survey. For race, participants were coded as White, Asian or African-American/Latino, according to self-identification; too few participants self-identified as African-American or Latino to yield reliable distinct analyses.

The statistical software package STATA version 10.1 MP (2007) was used to conduct all statistical analyses. The intervention and control groups were randomized using permuted block randomization with a 1:1 ratio ($m = 144$)[38]. *t*-Test power analyses ($P = 0{\cdot}05$, power = 0·8) were completed using STATA to determine the sample size needed to detect the subsidy's expected consumption effects of 14 %[39] (101 participants), 21 %[32] (forty-five participants) and 40 %[23] (thirteen participants), as well as the subsidy's expected purchasing effect of 200 %[11] (twenty participants). Based on this power analysis, the solicitation goal was set at 125 participants with a retention goal of 80 % (100 participants). A *post hoc t*-test power analysis taking into account the treatment and control group as well as the baseline and treatment periods was also conducted with STATA to find out whether the design was sufficiently powered to detect an effect of negation.

Spending, consumption and waste data were analysed using multivariate regression comparing the treatment and control groups after adjusting for the baseline, and through a two-sample *t* test with equal variances, with and without clustering standard errors by participant[40]. For the primary analysis, a regression was run on the results controlling for age, gender, race, undergraduate/graduate/professional programme of study (or lack thereof), whether participants supported others in the household ('supports others'), pre-existing differences between the treatment and control groups, and week-by-week variations in behaviour, with each demographic characteristic included as a dummy variable in the regression (e.g. 'female', 'White', 'supports others', etc.). Additionally, moderator analyses were run on race, gender and 'supports others' to determine whether any demographic-specific interactions occurred. Regressions were also run separately on men and women, to determine whether the subsidy had any significant impact on either gender, as well as to determine the quantitative impact of the subsidy on each gender individually.

Participants' weekly estimates of the amount they spent on all vegetables were compared with participants' receipts-documented spending on reimbursable vegetables to determine whether the intervention may have impacted participants' spending on vegetables in restaurants, which would be reflected in participants' spending estimates but not receipts. This comparison was similarly regressed with and without clustering of standard errors.

Midpoint price elasticity was calculated using the treatment group's baseline and intervention-period spending and the subsidy's actual discount of purchases as follows: $\{(Q2-Q1)/[(Q1+Q2)/2)]\}/\{(P2-P1)/[(P1+P2)/2]\}$, where Q1 is the treatment group's expenditure on un-subsidized vegetables, Q2 is the treatment group's expenditure on subsidized vegetables, P1 is the un-subsidized (full) price of vegetables and P2 is the effective subsidized price of the vegetables. An estimate of the subsidy's efficiency was also prepared by dividing the subsidy's regressed impact on spending by participants' claimed reimbursement.

Finally, to determine the usefulness of the consumer subsidy as a health policy tool, results are reported for the entire study group as well as after excluding high vegetable consumers, defined as participants who reported consuming more than the recommended five servings of vegetables daily (thirty-five servings weekly), in each week of the baseline period.

Results

One hundred and fifty potential participants responded to the study solicitations, and 144 participants enrolled. The demographic composition of the treatment group closely resembled that of the control group (Table 1). Participants' response to weekly surveys was timely: 80 % of participants completed their surveys within 24 h of survey distribution. Moreover, study retention was robust: 96 % of participants completed all five weekly surveys, and 83 % completed all surveys and returned their receipts. Participation rates for the control and treatment groups were identical for survey completion (96 % for each) and similar for receipts collection (83 % treatment group, 81 % control group). Among the 4 % of participants who did not complete all five consumption surveys and were therefore excluded from analysis were several participants who completed the demographic survey but no weekly consumption surveys.

Participants in the treatment group estimated spending an average of $17·85/week on vegetables during the two-week baseline, and the subsidy was tailored to impact the marginal expenditures of 85 % of the treatment group. Most participants reported consuming less than the vegetable recommendation (5 servings/d) during the baseline period, with only eleven total participants reporting higher-than-recommended weekly consumption (>35 servings/week in both weeks 1 and 2): five in the control group (7 %) and six in the treatment group (9 %). Excluding these high vegetable consumers from the analysis did not affect any of the significance findings, although it did influence the *P* value coefficients.

Primary outcome measure

Effect on expenditures

The subsidy accounted for a statistically significant increase in participants' vegetable expenditures (Fig. 1). As

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Table 1 Characteristics of the control (*n* 64) and treatment (*n* 66) group participants in the study (assessing the impact of a consumer-side vegetable subsidy on purchasing, consumption and waste, Palo Alto, CA, USA, 2011)

	Total (*n* 130)		Control group (*n* 64)		Treatment group (*n* 66)	
	n	%	*n*	%	*n*	%
Mean age (years)	25·9		25·9		25·8	
SD	2·6		2·4		2·9	
Gender						
Female	73	56·2	36	56·3	37	56·1
Male	57	43·8	28	43·7	29	43·9
Race						
White	76	58·5	39	60·9	37	56·0
Asian	23	17·7	12	18·8	11	16·7
'Other' (non-Asian, non-White)	31	23·8	13	20·3	18	27·3
Household characteristics						
Supports only self in household	111	85·4	57	89·1	54	81·8
Supports others in household	19	14·6	7	10·9	12	18·2

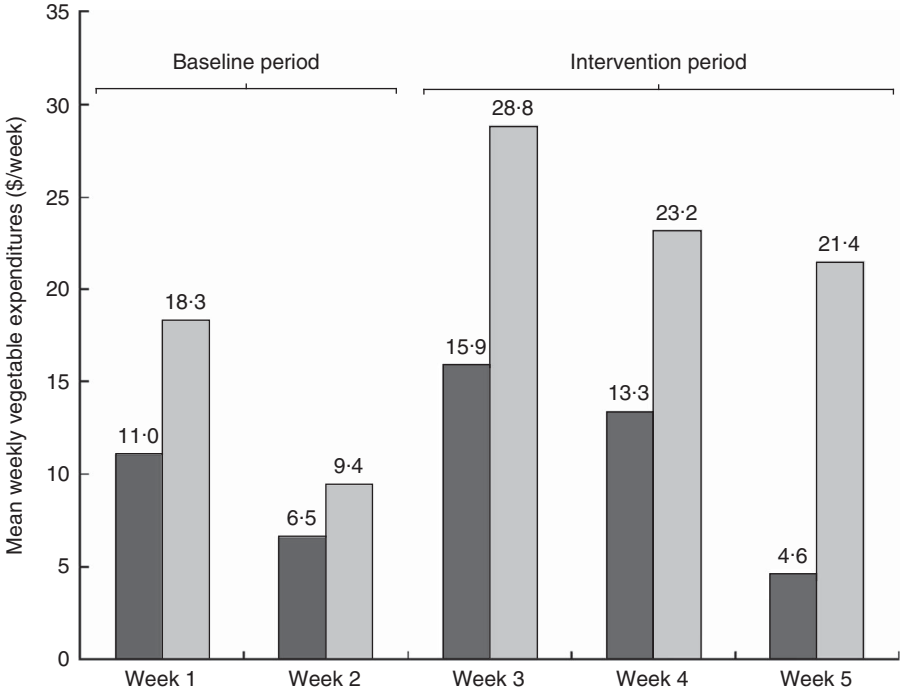

Fig. 1 Mean weekly vegetable expenditures (in US dollars ($) per week) for control (*n* 64; ■) and treatment (*n* 66; □) groups, as reflected in collected grocery receipts, for the baseline observation period and intervention period in the study (assessing the impact of a consumer-side vegetable subsidy on purchasing, consumption and waste, Palo Alto, CA, USA, 2011)

will be discussed below, a moderation analysis indicated that this effect was significant only in men.

Regression analysis indicated that the subsidy accounted for an increase of $8·16 (SD 2·67)/week in the treatment group's vegetable spending from the baseline to the intervention period as compared with controls ($P < 0·01$). Excluding baseline high vegetable consumers, the subsidy accounted for an increase of $9·35 (SD 3·72)/week ($P < 0·01$).

The treatment and control groups differed significantly in mean weekly vegetable expenditures over the baseline period ($P = 0·04$). The control group's mean weekly vegetable expenditures increased insignificantly ($P = 0·27$) from the baseline ($8·79 (SD 13·68)/week) to the intervention period ($11·26 (SD 19·81)/week). During the same period, the treatment group's mean weekly vegetable expenditures increased significantly ($P < 0·01$) from the baseline ($13·85 (SD 20·92)/week) to the intervention period ($24·47 (SD 26·97)/week).

Secondary outcome measures

Effect on consumption

The subsidy did not significantly impact participants' vegetable consumption (Fig. 2).

Regression analysis indicated that the subsidy accounted for an increase of 1·3 (SD 1·2) servings/week in the

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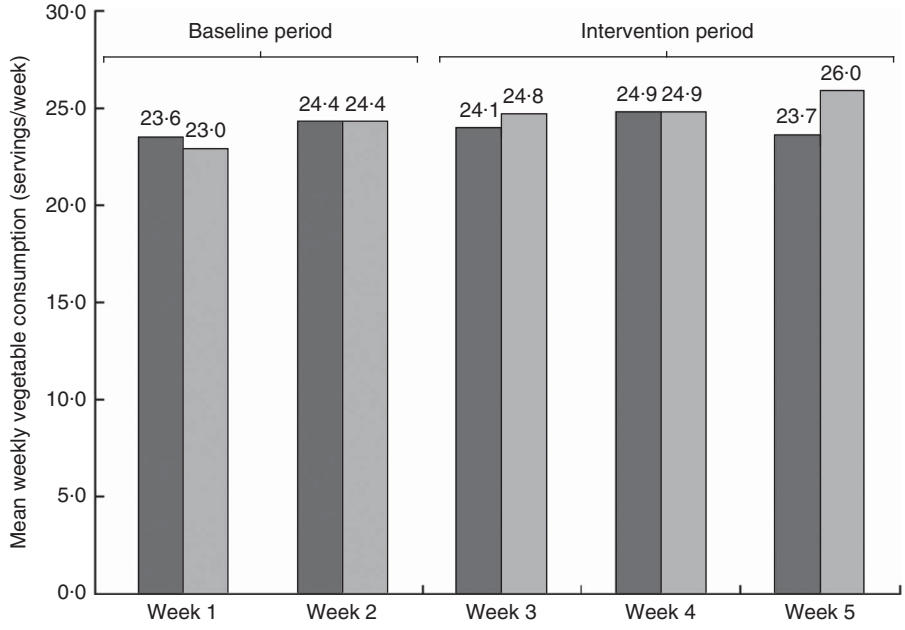

Fig. 2 Mean weekly vegetable consumption (in servings per week) for control (*n* 64; ■) and treatment (*n* 66; □) groups, as reported in FFQ responses, for the baseline observation period and intervention period in the study (assessing the impact of a consumer-side vegetable subsidy on purchasing, consumption and waste, Palo Alto, CA, USA, 2011)

treatment group's vegetable consumption from the baseline to the intervention period as compared with controls ($P = 0{\cdot}28$). Excluding baseline high vegetable consumers, the subsidy accounted for an increase of 0·9 (SD 1·1) servings/week in weekly vegetable consumption ($P = 0{\cdot}41$).

Control and treatment mean vegetable consumption did not differ significantly during the baseline period ($P = 0{\cdot}99$). The control group's mean weekly vegetable consumption increased insignificantly ($P = 0{\cdot}86$) from the baseline (24·0 (SD 13·2) servings/week) to the intervention period (24·2 (SD 12·0) servings/week). The treatment group's mean weekly vegetable consumption also increased insignificantly ($P = 0{\cdot}25$) from the baseline (23·7 (SD 10·6) servings/week) to the intervention period (25·2 (SD 12·7) servings/week). Differences in mean consumption between groups during the intervention period were similarly not statistically significant (control 24·2 (SD 12·0) servings/week, treatment 25·2 (SD 12·7) servings/week, $P = 0{\cdot}42$).

Effect on waste

The subsidy did not significantly impact participants' mean vegetable waste (Fig. 3).

Regression analysis indicated that the subsidy accounted for a 0·23 (SD 1·2) servings/week decrease in the treatment group's vegetable waste from the baseline to the intervention period as compared with controls ($P = 0{\cdot}60$). Excluding baseline high vegetable consumers, the subsidy accounted for a 0·50 (SD 0·47) servings/week decrease in weekly vegetable waste ($P = 0{\cdot}29$).

Control and treatment mean vegetable waste did not differ significantly during the baseline period ($P = 0{\cdot}83$). The control group's mean weekly vegetable waste increased insignificantly ($P = 0{\cdot}34$) from the baseline (1·77 (SD 2·50) servings/week) to the intervention period (2·10 (SD 3·32) servings/week). The treatment group's mean weekly vegetable waste also increased insignificantly ($P = 0{\cdot}75$) from the baseline (1·70 (SD 2·78) servings/week) to the intervention period (1·80 (SD 2·74) servings/week). Differences in mean waste between groups during the intervention period were similarly not statistically significant ($P = 0{\cdot}33$).

Additional outcome measures

Effect on specific vegetables

The subsidy did not significantly impact participants' consumption or waste of any individual vegetable. However, the subsidy did significantly increase participants' expenditures on salad greens ($1·14 (SD 0·50)/week, $P = 0{\cdot}02$), tomatoes ($0·49 (SD 0·24)/week, $P = 0{\cdot}04$), dark leafy greens ($0·43 (SD 0·18)/week, $P = 0{\cdot}02$), carrots ($0·40 (SD 0·12)/week, $P < 0{\cdot}01$), onions ($0·18 (SD 0·09)/week, $P = 0{\cdot}05$) and peas ($0·22 (SD 0·09)/week, $P = 0{\cdot}02$). Sixteen additional vegetables were coded but were not significantly impacted by the subsidy.

Vegetable subsidy elasticity and efficiency

Participants in the treatment group qualified for an average of $13·08 of vegetable reimbursement per week, $39·24 of the $50 available subsidy over the course of the study. Only eight participants (14 %) qualified for the full $50 subsidy. Midpoint price elasticity was calculated to be 0·76 for the treatment group (0·79 excluding high vegetable consumers). Similarly, comparing the subsidy's estimated

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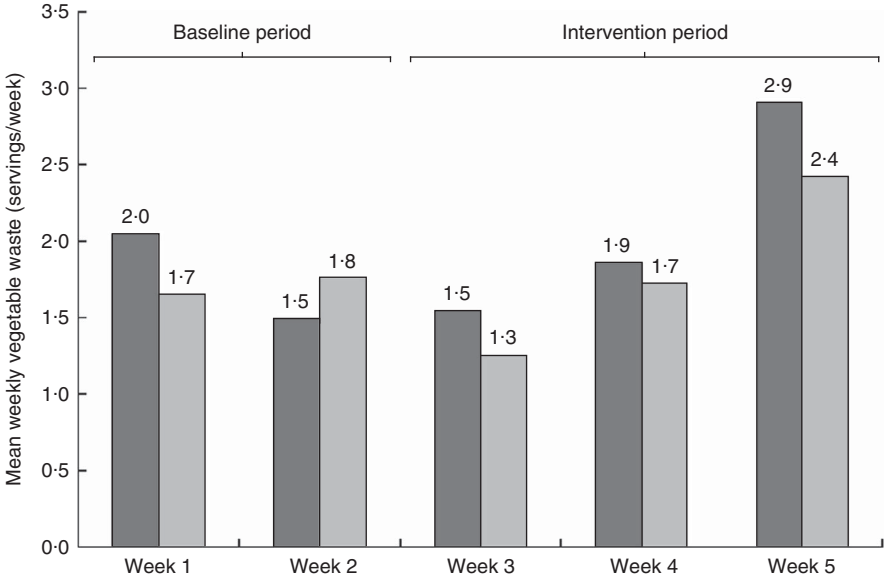

Fig. 3 Mean weekly vegetable waste (in servings per week) for control (*n* 64; ■) and treatment (*n* 66; □) groups, as reported in FFQ responses, for the baseline observation period and intervention period in the study (assessing the impact of a consumer-side vegetable subsidy on purchasing, consumption and waste, Palo Alto, CA, USA, 2011)

impact on expenditures with participants' actual reimbursement earned indicates that the subsidy motivated the treatment group to spend $0·62 more on vegetables for every $1 provided in reimbursement.

Comparison of participants' estimated spending with actual spending

The subsidy significantly impacted the relationship between participants' estimates of their total weekly vegetable spending and their actual receipts-documented spending on reimbursable vegetables. Multivariate regression indicated that the subsidy accounted for an average decrease of $7·30 (SD 2·68)/week in the treatment group's estimated total vegetable spending *v.* their actual receipts-documented reimbursable vegetable spending from the baseline to the intervention period, as compared with controls ($P < 0·01$).

Heterogeneity

Gender

The subsidy had a significant effect on males' vegetable expenditures ($12·60 (SD 3·88)/week, $P < 0·01$), but not consumption (2·73 (SD 1·86) servings/week, $P = 0·15$) or waste (−0·44 (SD 0·52) servings/week, $P = 0·40$). The subsidy had no significant effect on females' vegetable expenditures ($4·70 (SD 3·77)/week, $P = 0·22$), consumption (0·14 (SD 1·48) servings/week, $P = 0·92$) or waste (−0·07 (SD 0·68) servings/week, $P = 0·92$).

Regression analysis indicates that the subsidy's effect on men was significantly greater than on women for vegetable consumption (4·67 (SD 1·81) servings/week, $P = 0·01$), but not expenditures ($5·50 (SD 3·89)/week, $P = 0·16$) or waste (0·77 (SD 0·64) servings/week, $P = 0·23$).

Race

No race-specific effects of the subsidy were observed, although, controlling for other variables, Asian men ate significantly more vegetables than white men (10·29 (SD 3·19) servings/week, $P < 0·01$).

Supports others

At just 14·6 % of the study population, the number of participants who reported financially supporting others may be too limited to yield substantive sub-analyses. Participants who reported supporting others were not significantly more influenced in their vegetable consumption (−0·47 (SD 2·74) servings/week, $P = 0·86$) or waste (−0·66 (SD 0·54) servings/week, $P = 0·23$) than participants who did not so report, although they may have been influenced to decrease their vegetable expenditures by the subsidy (−$4·73 (SD 2·87)/week, $P = 0·10$).

Power analysis

Because 119 participants completed all of the consumption surveys in a timely manner and submitted purchasing receipts, the study was sufficiently powered to detect even the smallest expected effects of a 14 % increase in consumption[40], as well as the expected effects of a 200 % increase in purchasing[2] at $P = 0·05$ and a power of 0·8: the study population exceeded the respective calculated minimums of 101 and twenty participants. Moreover, the *post hoc* power analysis indicated that the study was sufficiently powered to rule out the null for the observed purchasing effects (0·99 power), but not the observed consumption or waste effects (respectively 0·31 and 0·18 power). Nevertheless, the study was sufficiently powered to rule out the null for the increase in consumption

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necessary to bring study participants into compliance with minimum daily vegetable intake recommendations – a 40 % increase in vegetable consumption (at $P = 0{\cdot}5$ and a power of 0·8).

Discussion

As was expected, the intervention significantly impacted participants' vegetable expenditures. Unexpectedly, this increase was not accompanied by observed increases in participants' vegetable consumption or waste. This is an unprecedented set of findings: of the few studies to examine an intervention's effect on purchasing and consumption, not one has observed a significant effect on purchasing without also observing a significant effect on consumption[6,9,18,21].

This may be due to the intervention design employed: the present study is unique in that it provided a subsidy via a cash reimbursement scheme rather than via coupons or vouchers[6,9,18,21]. This design choice was made to maximize the subsidy's effectiveness and to mimic a tax credit-based subsidy. Because participants were not limited to any specific set of vendors, participants did not need to modify their place of purchase to take advantage of the subsidy. And because participants were reimbursed for all vegetable purchases after the first $10 spent, for up to $50 total, the marginal cost of each additional dollar spent on vegetables to participants who did not exhaust their reimbursements (and 86 % of participants did not) was $0. This, however, also meant that most participants had no incentive to restrain their vegetable spending. Coupon- or discount-based subsidies, on the other hand, traditionally still require participants to spend at least some of their own money to purchase the subsidized food[6,9,18,21]. This distinction could explain this study's divergence from prior research. Moreover, because reimbursement was provided based on vegetable register receipts from a variety of vendors, which did not consistently provide information on vegetable weight or quantity, purchasing was measured in expenditures rather than in grams or servings. Participants in the present study could therefore spend more money on vegetables without purchasing greater quantities of vegetables, and the study design may have inadvertently provided incentive to do just that.

Some empirical support for this hypothesis exists in participants' increased purchasing of vegetables with readily substitutable expensive counterparts: salad greens ('salad'), dark leafy greens ('greens') and tomatoes. Both salad and greens may be purchased loose ($0·12/oz) or pre-bagged (respectively $0·55/oz and $0·27/oz), and tomatoes are sold vine-ripened ($0·25/each), organic vine-ripened ($0·57/each) and organic heirloom vine-ripened ($2·00/each)[41]. Controlling for other variables, the subsidy accounted for a $1·14 weekly increase in salad expenditures ($P = 0{\cdot}02$), a $0·43 weekly increase in greens expenditures ($P = 0{\cdot}02$) and a $0·49 weekly increase in tomato expenditures ($P = 0{\cdot}04$), without accounting for significant increases in salad (−0·18 servings/week, $P = 0{\cdot}54$), greens (−0·18 servings/week, $P = 0{\cdot}49$) or tomato (0·28 servings/week, $P = 0{\cdot}13$) consumption (or waste). Therefore, the intervention may have motivated participants to spend more money on organic or pre-packaged vegetables without purchasing more of those vegetables. This effect could be ascertained in a future study by measuring purchasing by the quantity of food purchased (grams or servings) instead of by expenditures, or by coding for organics or other indicators of price. The store-printed receipts used in the present study were not sufficiently detailed to allow for such measurement or coding.

Several other hypotheses for this study's unexpected findings also have some support.

First, the intervention may have motivated participants to purchase vegetables at grocery stores (where purchases were reimbursed) instead of at restaurants (where purchases were not reimbursed). Such a restaurant-to-grocery substitution may be reflected in the data. Performing the regression of participants' estimated total vegetable spending (which included both restaurant and grocery store purchases) *v.* participants' receipts-documented actual spending (which only included grocery purchases) indicates that the subsidy accounted for a $7·30 (SD 2·68)/week decrease in participants' estimated total spending as compared with their reimbursable actual spending ($P < 0{\cdot}01$). Thus, the study's observed $8·16 (SD 2·67)/week increase in reimbursed vegetable spending may have been accompanied by a similar, $7·30 (SD 2·68)/week, decrease in non-reimbursed vegetable spending. If this is the case, this substitution may have accounted for much of the intervention's observed expenditures effect. Substitution effects of this nature due to healthy food subsidies, however, have not otherwise been observed in the literature[6,29]. Nevertheless, the cash-based reimbursement scheme used in the present study may have inadvertently encouraged more substitution: prior research has examined only point-of-purchase coupons and discounting[6,9,18,21]. Future research could confirm this by asking participants to collect restaurant receipts in addition to grocery receipts.

Second, the intervention may have motivated participants to hoard vegetables purchased during the study. Such an effect may be observed here in participants' increased expenditures on vegetables with long shelf lives: carrots, potatoes, onions, peas, etc. Controlling for other variables, the subsidy caused participants to significantly increase their weekly spending on carrots ($0·40, $P < 0{\cdot}01$), peas ($0·22, $P = 0{\cdot}02$) and onions ($0·18, $P = 0{\cdot}05$). Potatoes, another long-lasting vegetable, were not significantly impacted by the subsidy ($0·10, $P = 0{\cdot}22$). This implies either that study participants hoarded some vegetables but not others, or that another unknown factor contributed to participants' purchase of carrots, peas and

onions. Future research including a longer intervention period would help ascertain whether a hoarding effect was present, although two weeks has been shown to be sufficient to measure the effects of a vegetable subsidy[6].

Third, consumption self-reporting provides only an inconsistent measure of absolute levels of food intake, which could have biased the results. However, consumption self-reporting has been proved useful in measuring relative changes in food intake – which is the relevant metric for the present study – and has been widely used in other health food intervention studies[6,9,18,21,42,43]. Alternative measurement devices also have limitations: metabolic laboratory analysis is incredibly resource intensive and introduces significant laboratory bias, and 24 h recalls do not provide a comprehensive picture of consumption. Consequently, although imperfect for some purposes, the FFQ used was well suited for the present study, where a comprehensive picture of relative changes in participant consumption was needed[6]. Future research making use of other reporting metrics could nevertheless help illuminate whether the study's findings are due in any part to the measurement device used.

Also unexpected was the intervention's different effect on men than on women. Moderation analysis indicated that the subsidy had a significant effect on males' vegetable expenditures ($P < 0{\cdot}01$), but that it did not have a significant effect on females' vegetable expenditures ($P = 0{\cdot}22$). Moreover, although the subsidy's impact on males' vegetable consumption was nevertheless not statistically significant ($P = 0{\cdot}15$), the subsidy had a significantly greater effect on males' vegetable consumption than females' ($P = 0{\cdot}01$). This together implies that the null hypothesis could be rejected for male consumption with a larger study population. These findings are particularly interesting in the context of Waterlander *et al.*'s study of a discount-based intervention, which observed significant purchasing and consumption effects in a predominantly female participant group (96 %)[9]. One explanation for this difference, that high pre-intervention vegetable expenditures and vegetable consumption among women in the present study contributed to a ceiling effect, may be rejected: only six participants in the treatment group consumed the daily recommended servings of vegetables during the baseline period and these 'high vegetable consumers' were not predominantly female. Instead, it may be that the reimbursement-based intervention employed had a unique gendered effect. Further research is needed to examine gender-targeted interventions.

Limitations

Although the study design and results are robust overall, several factors may limit the conclusions that can be drawn from the present study.

First, the study population may not be representative of the population at large; participants were solicited from the Palo Alto community and therefore a significant percentage of participants were likely associated with Stanford University. The relatively small percentage of participants who financially supported others (14·6 %) and the relatively young mean age of the study population (25·86 (SD 2·64) years) also suggest the possibility of a substantial number of student participants. This concern, however, is diminished because the purchasing effects observed in the study are similar to those observed in prior research[10–12]. Moreover, demographic variables, race, gender, programme of study (or none) and 'supports others' were tracked and separately regressed, with no demographic-specific effects observed other than on gender. Thus, although additional research would be useful to examine the impact of a vegetable subsidy on different populations, the present study results do not give rise to suspicion of substantially different impacts.

Second, pre-existing differences in the randomized treatment and control groups – the treatment group spent significantly more during the baseline period – may bias the results. The baseline to intervention study design, however, should mitigate any such bias. Moreover, because participants in the treatment group were not informed about the subsidy until after the baseline period and participants in the control group were not substantially less likely to turn in their receipts than participants in the treatment group (83 % treatment group, 81 % control group), it appears unlikely that the treatment to control distinction impacted participants' receipts collection. Pre-existing spending differences, instead, were likely due to the size of the sample group and could actually have masked an even greater impact on expenditures, as there may have been a ceiling effect for the already higher-spending treatment group. Some evidence for a ceiling effect exists in the subsidy's diminished impact on participants who reported consuming high numbers of vegetables in the baseline period.

Finally, it is possible that the lack of observed consumption and waste effects was due to non-differential misclassification error stemming either from the FFQ or from the different measurements used for food expenditures (dollars) and for consumption and waste (servings). Several considerations, however, mitigate this final concern. First, a growing body of statistical scholarship suggests that non-differential exposure misclassification can increase, as well as decrease, the observed strength of an association[44,45]. Second, using different measurement indices (dollars and servings), although imperfect, may be preferable to alternatives. There are not yet validated means to measure food consumption or waste in dollars, and designing a reliable measurement may be difficult given the challenges of estimating vegetable value. Measuring food purchasing in servings may be similarly impractical where, as was the case here, grocery receipts and other sources of purchasing information do not provide sufficient information to determine quantities of vegetables purchased.

Point-of-purchase measurements, which do provide such information, introduce bias by restricting participants to limited shopping options. Where participants already shop exclusively at stores that provide such information, however, purchasing should be measured using food quantities rather than food expenditures[9,15]. Future research that asks participants to estimate both consumption and purchasing may help discern whether non-differential misclassification error influenced the results observed in the present study. Third, measuring consumption in servings and purchasing in dollars captures important policy data: the number of servings of vegetables consumed per dollar expended. Finally, any remaining concerns about non-differential misclassification error in the present study are moderated by the robust sample size, which was more than sufficiently powered to detect even the smallest expected consumption and purchasing effects[32,34,40]. Although the *post hoc* power analysis could not rule out the null for consumption or waste effects, the power analyses indicate that the intervention's influence on participants' consumption or waste could only have been small – smaller than the effect necessary to bring participants into compliance with the minimum recommended daily vegetable servings, and smaller even than the smallest effects observed in previous studies. Therefore, although future research using longer observation periods or larger participant groups could help illuminate whether a vegetable consumer subsidy has any small effect on vegetable purchasing or waste, the present study is sufficiently powered to have immediate practical salience: not even a consumer-side vegetable subsidy approaching 100 % of participants' vegetable spending was sufficient to bring participants into compliance with daily intake recommendations.

Conclusion

Reimbursement-based consumer subsidies may be less useful in encouraging vegetable consumption than previously believed. It appears from the present study that the subsidy influenced participants to shift their behaviours in multiple ways, none of which led to increased vegetable consumption. This does not necessarily imply that consumer subsidies cannot be a successful means of increasing vegetable consumption, but rather that any effective consumer subsidy should be designed to minimize these various forms of substitution. For example, a consumer subsidy could subsidize restaurant purchases as well as grocery purchases, or provide a per-item reimbursement. Additional research could help determine whether these substitution effects are present in other populations.

Acknowledgements

Acknowledgements: The author thanks Charlie Wysong and Professors Joseph Bankman and Christopher Gardner for their assistance with the study. *Financial support:* This study was sponsored by the Stanford Law School Office of the Dean (no specific grant number). The Office of the Dean had no role in the design, analysis or writing of this article. *Conflict of interest:* None. *Ethics of human subject participation:* The study was approved as exempt by the Stanford IRB (Protocol 21452).

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