Cash Transfers and Temptation Goods

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Since the introduction of cash transfer programs, both conditional and unconditional, a major concern has been that households will misuse the cash. In Nicaragua, a senior government official expressed concern that "husbands were waiting for wives to return in order to take the money and spend it on alcohol" (Moore 2009, 35). Interviews with stakeholders in Kenya revealed the "widespread belief that cash transfers would either be abused or misdirected in alcohol consumption and other non-essential forms of consumption" (Ikiara 2009, 22). A broader survey highlights that "there is a widely held belief that cash given to poor people (especially to men) will be squandered on alcohol and other non-essentials" (Devereux 2002, 12). Governments and aid agencies may worry that "men could control the cash provided and spend it on alcohol and cigarettes, rather than food for hungry children" (Harvey 2007, 3). These concerns may explain why many countries prefer in-kind transfer programs, even though economic reasoning would suggest that cash transfers are more efficient (Case and Deaton 1998): households can more easily meet their heterogeneous needs with cash than with other, less easily convertible goods.

Alcohol and tobacco have been referred to as "temptation goods" (Dasso and Fernandez 2013), a term used by Banerjee and Mullainathan (2010, abstract) to refer to "goods that generate positive utility for the self that consumes them, but not for any previous self that anticipates that they will be consumed in the future." In an earlier literature, Musgrave (1959) used a related (but more normatively charged) term, "demerit goods," to refer to goods that were so demeritorious (either to the consumer or to others) that the government may be correct in regulating their use. That term is sometimes used in reference to alcohol and tobacco in cash transfer studies.

In this study, we use the term "temptation goods" principally to refer to alcohol and tobacco.[1] This study makes no normative assumption as to the value of alcohol and tobacco expenditures but merely seeks to systematically characterize the literature on the impact of cash transfers on these goods. Although alcohol and tobacco are the principal goods under consideration, some studies report other items as part of the same category, from doughnuts (Aker 2015) to soft drinks and Chinese food (Dasso and Fernandez 2013). The poor may wish to reduce spending on these items, as evidenced by a survey in Hyderabad, India, that asked households if they would like to eliminate any expenses in their budget: 28% of households identified at least one item, and the top items (44% of those) that households wanted to cut were alcohol and tobacco (Banerjee and Duflo 2007). Even so, alcohol and tobacco are unlikely to strictly fit into the definition from Banerjee and Mullainathan (2010). Households may wish to reduce total spending on these goods, but they may view a major proportion of the spending as normal good spending, in which case it would be unsurprising for part of a general increase in income to be spent on their consumption. "Temptation goods" serves as a shorthand in this article.

Most cash transfer programs are not focused on either increasing or decreasing consumption of these goods specifically, and so most evaluations and the subsequent reviews have not focused on these. Rather, reviews have focused on outcomes in schooling (Saavedra and García 2013; Baird et al. 2014), health (Leroy, Ruel, and Verhofstadt 2009; Ranganathan and Lagarde 2012), consumption (Fiszbein and Schady 2009), or a combination of these (IEG 2011). At the same time, many individual evaluations of cash transfer programs have included analysis of the impact on some set of temptation goods within their consumption analysis.

Across 50 estimates from 19 studies, we find that almost without exception, studies find either no significant impact or a significant negative impact of transfers on expenditures on alcohol and tobacco. Moreover, our meta-analysis for those studies reporting impacts of transfers on total temptation good expenditure yields a negative, significant average effect. Several robustness checks, including restricting to randomized trials alone, likewise yield negative (insignificant) average effects. Similarly, subgroup analysis shows average decreases in temptation good spending in each region and for beneficiaries of each type of cash transfer program, while analysis for Latin America and for conditional cash

[1] Consumption of these goods may in some cases serve positive social purposes. For example, one study recounts the anecdote of demobilized soldiers returning home in Mozambique and using some of their demobilization grant on alcohol in the context of a village celebration to assist in their reintegration (Harvey 2007).

transfers—each with a relatively high concentration of studies—specifically yields a statistically significant decrease. Likewise, studies that have tried to quantify the proportion of beneficiaries who spend transfers on temptation goods find negligible effects. The evidence suggests that cash transfers are not used for alcohol and tobacco at any significant levels, irrespective of region or program design.

This finding informs the policy debate in many low-income countries as to whether to introduce cash transfer programs. The vast majority of countries in Sub-Saharan Africa have formally discussed, planned, or piloted some form of cash transfer program (Garcia and Moore 2012). It also informs policy surrounding the choice between cash and in-kind transfers, as has been debated in India over the last several years (Drèze 2011; *Times of India* 2013), insofar as that debate is at least partly driven by public concern that recipients are more likely to spend cash transfers on temptation goods than they are to spend in-kind transfers (Khera 2014).[2] By demonstrating that cash transfers do not in fact increase spending on temptation goods, we implicitly answer this question and further dispel concerns surrounding cash transfers.

Cash, Spending, and Consumption of Temptation Goods

If alcohol and tobacco are normal goods, then as incomes rise, consumption of these goods will likewise rise (i.e., the income effect), implying a positive marginal propensity to consume. Evidence from the United States suggests that alcohol is a normal good, whereas tobacco is an inferior good (Decker and Schwartz 2000); evidence from the United Kingdom suggests that alcohol expenditures rise with income, at least to a point (Banks, Blundell, and Lewbel 1997). Banerjee and Duflo (2006, 2007) show expenditures for households living on under $1.08 per day and for households living on under $2.16 per day in 11 countries divided into urban and rural areas, resulting in 21 country-urban or country-rural combinations, due to missing data on urban Guatemala (table 1). Of these 21 combinations, 14 increased or maintained the same percentage of spending on alcohol and tobacco combined, when comparing $1.08 to $2.16 daily income, suggesting a likely increase in the total spending.[3] That number rises to 20 out of 21 if one includes settings with minor decreases in

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[2] Gentilini (2016) conducts a comprehensive review of 12 randomized trials across four continents that quantitatively estimate the relative impact of cash transfers versus in-kind food transfers. On average, he finds cash and food transfers to have similar effects on food security, with food interventions being at least twice as costly to implement as their cash equivalents.

[3] These percentages are best for distinguishing luxury goods (for which the proportion of spending increases with income) from necessity goods (for which the proportion falls), but they are suggestive of an increase in absolute spending (i.e., normal goods).

TABLE 1
CONSUMPTION OF ALCOHOL AND TOBACCO AS A SHARE OF TOTAL CONSUMPTION (%)

	Household Living on Less Than		
	$1 per Day	$2 per Day	Ratio ($2/$1)
Rural:			
Côte d'Ivoire	2.7	2.2	81.5
Guatemala	.4	.5	125.0
India—Uttar Pradesh/Bihar	3.1	3.0	96.8
Indonesia	6.0	6.8	113.3
Mexico	8.1	6.5	80.2
Nicaragua	.1	.6	600.0
Pakistan	3.1	2.9	93.5
Papua New Guinea	4.1	5.1	124.4
Peru	1.0	1.3	130.0
South Africa	2.5	3.4	136.0
Timor-Leste	.0	.0	100.0
Urban:			
Côte d'Ivoire	3.5	3.3	94.3
India—Hyderabad	2.5	2.7	108.0
Indonesia	5.5	6.3	114.5
Mexico	3.6	4.2	116.7
Nicaragua	1.0	.7	70.0
Pakistan	3.0	2.9	96.7
Papua New Guinea	.6	4.4	733.3
Peru	.2	.8	400.0
South Africa	5.0	5.1	102.0
Timor-Leste	.0	.0	100.0

Note. Adapted from Banerjee and Duflo (2006).

the proportion, still consistent with increases in the total spending on these goods given the rise in income. These numbers suggest that alcohol and tobacco, when examining regular income, are normal goods.

Beyond the income effect, there are at least three reasons that cash transfer income may affect spending on temptation goods differently from other income. First, cash transfers may induce a substitution effect. For conditional cash transfers in particular, this may come by increasing the value of schooling and health investments relative to all other goods, which may shift households away from consumption of temptation goods (Fiszbein and Schady 2009). Moreover, the substitution effect may affect the consumption of temptation goods more than that of other goods because, not only do temptation goods not contribute to schooling and health in the way that other goods—such as nutrient-rich foods—might, but they may actually detract from investments in schooling and health by conceivably negatively affecting the home environment if consumed beyond certain levels. The relative strength of the income and substitution effects will vary across households, depending on their baseline school-

ing and health investments. Those beneficiary households that make sufficient investments in their children's education to satisfy the conditions of the program, before the program, will be less affected by the substitution effect. But, those who—before the program—are investing in education and health at levels below those required by program conditions will be more affected by the substitution effect. For both conditional and unconditional programs, cash transfers may induce a substitution effect away from temptation goods by increasing the marginal return on investment due to nonlinearity in the investment function. For example, by giving households sufficient cash at one time to open a small retail business, cash transfers create the opportunity to invest cash that might otherwise be spent in small doses on temptation items.

Second, while few cash transfer programs have explicit spending restrictions, they often come with strong social messaging. For example, Ecuador's unconditional cash transfer program (Bono de Desarrollo Humano) was accompanied by an advertising campaign encouraging households to invest in their children's human capital (Schady and Rosero 2008). In Zimbabwe, recipients of a cash transfer program were "instructed not to 'waste' the cash on drinks and other unproductive items" (Román 2010, 9). In Nicaragua, a task of the community coordinators for the program was "promoting the use of cash transfers to buy goods and services which improve the nutritional, educational and health status of beneficiary families" (Adato and Roopnaraine 2004, 18). Program officers often communicate to households that these resources are intended to improve education or health outcomes. As a result, households may be more likely to use the resources for expenditures related to education and health than on temptation goods, a manifestation of what has been termed the labeling effect (Kooreman 2000).

Finally, transfer income is often targeted at women, particularly in Latin America (Fiszbein and Schady 2009). This design choice is driven by the long-held idea that women are more likely to invest in children than are men. The actual evidence on this is mixed. On one hand, researchers found that higher proportions of household income controlled by women led to greater food expenditures in Côte d'Ivoire (Hoddinott and Haddad 1995), greater expenditures on food and children's goods in Mexico (Bobonis 2009), and improved child health in Brazil (Thomas 1990). In Macedonia, randomly assigning cash transfers to mothers (vs. the household head) significantly increased education expenditures as well as secondary school enrollment and achievement, but only when parents' perceived returns to education were high (Armand 2014). On the other hand, an unconditional cash transfer program in Kenya was randomly assigned to either the female or the mail head of household, and

the evaluation identified no increase in the consumption of temptation goods, irrespective of the gender of the beneficiary (Haushofer and Shapiro 2013). Similarly, two cash transfer programs that randomized recipient gender found no significant differences in outcomes for children. The outcome in the first study was health clinic visits for children in Burkina Faso (Akresh, de Walque, and Kazianga 2016), and in the second study it was school participation in Morocco (Benhassine et al. 2015).

If men are indeed more likely to purchase temptation goods (as was explicitly documented in Côte d'Ivoire but not in Kenya), then providing transfer income to women could reduce spending on those goods, a household bargaining effect. The net effect—between the income effect, the substitution effect, the labeling effect, and the household bargaining effect—is unclear theoretically: this article seeks to characterize it empirically.

A large literature has examined the impact of cash transfers on consumption, with a few studies explicitly contrasting transfer income with earned income. For example, Schady and Rosero (2008) show that food expenditures were much higher for transfer recipients than nonrecipients in the Ecuador program, even when controlling for per capita expenditures (i.e., the income effect of cash transfers). This finding is contrary to Engel's law, which states that "the proportion of income spent on food declines as income rises" (Houthakker 1957, 532) and which has been empirically identified across many countries. In Nicaragua, Macours, Schady, and Vakis (2012) use a similar strategy and find that cash transfer recipients shifted the composition of food expenditures to more expensive foods (i.e., more protein, fruits, and vegetables and fewer staples), even though total food expenditures were not different from other households with similar per capita expenditures. Case and Deaton (1998) demonstrate that pension income in South Africa increased food consumption and may have reduced alcohol and tobacco consumption, depending on the specification. Attanasio et al. (2005) find in Colombia that food consumption increases proportionately to income. With the exception of the last, these studies suggest that households may indeed treat transfer income differently from earned income. In what follows, we examine how exactly transfer income affects households' consumption of temptation goods in particular.

Methodology
Classification of Papers
In this section we describe the criteria used to define the universe of literature relevant to this systematic review, in terms of the types of interventions, studies, and outcome variables of primary interest, as well as the search strategy employed to find papers conforming to these criteria.

We restrict our analysis to conditional cash transfers (CCTs) and unconditional cash transfers (UCTs) implemented in low- and middle-income countries (as defined by the World Bank), with no other explicit population exclusion criteria. Since both CCTs and UCTs generally target poor and vulnerable households (often including school-age children or pregnant women), the entire set of eligible interventions is largely targeted at disadvantaged populations.

The review focuses on studies from 1997 to early 2014, which corresponds to the period after the onset of PROGRESA/Oportunidades, allowing for a relatively comparable group of cash transfer interventions, as in Baird et al. (2014). Eligible studies include both experimental and quasi-experimental designs. We limit the review to papers that compare cash transfer recipients to a group that receives no transfers. Specifically, in the systematic review we consider the effects on consumption of all those goods that studies themselves identify as "temptation," "demerit," or "antisocial" goods or those that reflect "misuse" or "waste." In conducting the review we focus on the effects on alcohol and tobacco consumption, for comparability purposes.

Consumption of temptation goods is measured in a number of ways across studies: notably, expenditure, share of expenditure, and share of individuals consuming the temptation good in the reference period. We include studies using all of these measures, although we focus on expenditure as our primary outcome of interest.

We classify this universe of eligible studies into the following three categories:

1. Impact Estimates: Randomized controlled trials or quasi-experimental studies that estimate the impact of cash transfers on the consumption of temptation goods, whether via the total household consumption of these goods, the proportion of total household expenditures spent on these goods, or other similar outcomes;
2. Level Estimates: Studies that use surveys or focus groups to characterize the number of beneficiaries or amount of transfers specifically used to purchase temptation goods; and
3. Qualitative Reports: Studies that discuss reports of the use of transfers to purchase temptation goods, not necessarily by the interviewed household.

Literature Search

In order to identify eligible studies in these three categories, we relied primarily on a Google Scholar search—for papers published since 1997, which included both the term "cash transfers" and any one of the terms "alcohol," "to-

bacco," "cigarettes," "temptation goods," or "demerit goods"—combined with expert recommendations. The various phases of the search process are summarized in chronological order in figure 1, together with the number of results they yielded. The search process is also described in detail in appendix A (apps. A–C available online). The search yielded a total of 42 papers: 19 containing impact estimates, 11 with level estimates, and 12 qualitative reports. The full list of papers is available in appendix B.

Analytical Strategy

We use the existing reported impact estimates to conduct two quantitative exercises. First, we conduct a meta-analysis to calculate the mean effect of cash transfers on total expenditures on temptation goods. Meta-analysis has the advantage of allowing us to combine estimates across studies, while explicitly taking the sample size and precision of estimates into account, to calculate a weighted-average overall effect size with its associated statistical significance.
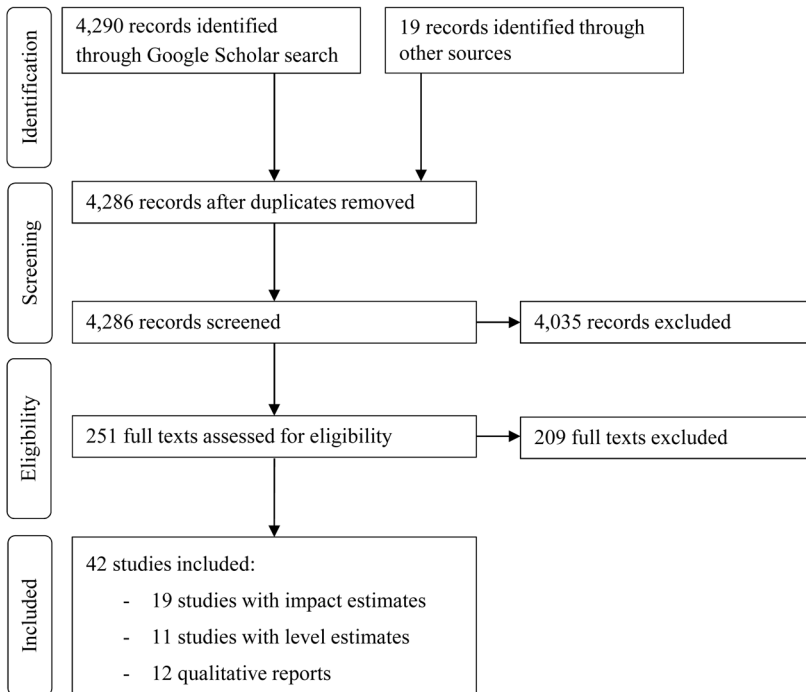

Figure 1. Flow diagram of literature review process using the PRISMA (2015) model, standard for many systematic reviews.

The disadvantages are that heavy data requirements mean that studies with in-complete reporting must be excluded and that only studies with the same out-come can be included in a single meta-analysis. For example, a study that ex-amines the impact of cash transfers on total spending on temptation goods could not be included in the same meta-analysis with a study that examines the impact on the probability of spending any money on temptation goods. As a result, we conduct a meta-analysis for a subset of impact estimates—all estimates of impacts on total temptation good expenditure for which suf-ficient data (i.e., treatment effect, standard error or standard deviation, and sample sizes for treatment and control groups) are reported in the original stud-ies. All of this analysis uses the aggregate data reported in the studies rather than the underlying individual participant data.

While our chosen sample of estimates has a common outcome—impact on total temptation good expenditure—this is reported on different scales across estimates since each study reports impacts in different currencies, from differ-ent years, and based on different sample sizes. For the meta-analysis, we stan-dardize these effects and the associated standard errors in order to be able to compare them directly and so as to be able to calculate an overall average effect.

Our unit of analysis for effect size is an experimental or quasi-experimental pair, where a group of cash transfer recipients is compared to a control group that did not receive the transfer. All of the studies included in our meta-analysis report the effect size as a raw mean difference, D. Almost all the studies use difference-in-differences methods to estimate the effect of cash transfer receipt on total temptation good expenditure. In these cases, D is the raw mean differ-ence between treatment and control groups, before and after a given program.

We calculate the standardized effect size or mean difference, d, for each es-timate, by dividing the raw mean difference, D, by the pooled standard devia-tion, S_{pooled}, as follows:

$$d = \frac{D}{S_{pooled}}, \qquad (1)$$

where S_{pooled} is the within-estimate standard deviation for the treatment and control groups combined. Where this is not directly reported in the studies, we calculate it using the following equation derived from Borenstein et al. (2009):

$$S_{pooled} = \sqrt{\frac{n_1 n_2}{n_1 + n_2}} SE_D, \qquad (2)$$

where n_1 is the sample size for the treatment group, n_2 is the sample size for the control group,[4] and SE_D is the standard error of the raw mean difference. For a complete derivation of equation (2), please see the mathematical appendix C.

Similarly, we calculate the standard error of the standardized mean difference, SE_d, for each estimate using the following equation derived from Borenstein et al. (2009), as described in appendix C:

$$SE_d = \sqrt{\frac{n_1 + n_2}{n_1 n_2} + \frac{d^2}{2(n_1 + n_2)}}. \tag{3}$$

We then incorporate these standardized effect sizes and standard errors into a DerSimonian and Laird (1986) random-effects model to construct a forest plot of the individual effects and calculate a pooled average effect, both with 95% confidence intervals. Random-effects models assume that the true effect can be different for each study (or in our case, for each estimate, as we use multiple estimates from certain studies). This is applicable in our case as the estimates we use are drawn from heterogeneous populations across different countries. Under random-effects models there are thus two components to the overall study error variance: within-study variance, V_i, and between-study variance, T^2. The weight assigned to each study in calculating the pooled average effect is then

$$W_i = \frac{1}{V_i + T^2}. \tag{4}$$

In calculating study weights, the DerSimonian-Laird model uses a noniterative method to estimate the between-study variance, without making any assumptions about the form of the distribution of within- or between-study effects. We generate these estimates using the metaan command in Stata 13 (Kontopantelis and Reeves 2010).[5]

Some studies contain multiple impact estimates because they either include multiple outcomes (e.g., effect on total alcohol expenditure and effect on total

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[4] A number of studies do not report information on the size of treatment and control group samples used in the temptation good regressions separately. In these cases, either we multiply the total number of observations in the relevant regression by the ratio of treatment to control villages reported (for Schluter and Wahba 2004; Attanasio et al. 2005; Gitter 2006; Bazzi, Sumarto, and Suryahadi 2012; Dasso and Fernandez 2013) or, where no ratio is reported or nearest-neighbor propensity score matching is used, we assume that observations are equally distributed across treatment and control groups (for Perova 2011; Cunha 2012).

[5] The data and do-files for the meta-analysis are available in a zip file online at *Economic Development and Cultural Change*.

tobacco expenditure) or use multiple estimation methods (e.g., propensity score matching and instrumental variables). Because meta-analysis uses a weighted aggregate of individual estimates to calculate a pooled effect, we only include multiple estimates from the same study if there is reason to treat each as a separate comparison, so as to avoid double counting the same participants. In cases in which multiple estimation techniques are used, we select the estimate that seems least likely to be biased (the preferred estimate). Later, we test the robustness of the meta-analysis results by including the less preferred estimate. We also test the robustness of the result to the exclusion of the largest individual estimates, as well as to only including estimates from randomized controlled trials (RCTs).

We then conduct subgroup meta-analysis to test for heterogeneous effects across a number of dimensions. Namely, we explore how the average effect of cash transfers on temptation good expenditure varies by (1) geographical region, (2) whether the transfer is conditional or unconditional, (3) whether labeling makes a difference among unconditional programs, and (4) the agency in charge of data collection.

Second, to account for the fact that not all estimates can be included in the meta-analysis, we implement a complementary "vote-counting" exercise, which includes all estimates and involves simply categorizing estimates into four groups: (1) negative and significant, (2) negative or zero and insignificant, (3) positive and insignificant, and (4) positive and significant. This method has the advantage of inclusiveness: all studies reporting estimates alongside their statistical significance can be incorporated in a single analysis even if outcome variables are not identical or insufficient data are reported to permit a meta-analysis. It has the disadvantage that it weighs all estimates equally: effect sizes are ignored, and their statistical precision is accounted for only in binary terms (i.e., significant or insignificant). The vote counting (which can include all estimates) and the meta-analysis (which treats a more limited set of estimates more rigorously) are thus complementary.

Beyond the vote counting and the meta-analysis, we include a discussion of level estimates—efforts to measure how much of transfer income specifically is spent on temptation goods—as well as qualitative evidence from developing countries worldwide.

Results

Nineteen studies from 10 countries around the world (in Latin America, Africa, and Asia) report impacts of cash transfers on the level or proportion of expenditures on alcohol or tobacco or the probability of consumption or abuse of these goods. These studies and the reported impacts are listed in tables 2 and 3. The 19 studies include 50 impact estimates, 19 of which are used to calculate an

TABLE 2

STUDIES WITH ESTIMATED IMPACT OF TRANSFER ON ALCOHOL OR TOBACCO EXPENDITURES

Reference	Country	Meta-analysis Estimate Code	Program Name	Temptation Good	Impact	Detail on Impact	Sample Size	Methodology	CCT/ UCT	Data Collection Agency
Braido, Olinto, and Perrone (2012)	Brazil	1. Brazil	Bolsa Alimentaria and Bolsa Escola	Alcohol, tobacco, and gambling	−1.961 (1.86)		1,006	DID	CCT	Separate agency
Attanasio et al. (2005)	Colombia	2. Colombia	Familias en Acción	Alcohol and tobacco	2.822 (4.64)	Urban	11,500	DID	CCT	...
		3. Colombia			−1.536 (3.31)	Rural	11,500	Before after		...
Bhowmik, Gartenberg, and Sarker (2013)	India		Unconditional Cash Transfer Pilot	Alcohol	−.001		200		UCT[L]	...
				Tobacco	−.001[a]					
Gangopadhyay, Lensink, and Yadav (2013)	India	4. India	Unconditional Cash Transfer Pilot	Alcohol	.080 (.53)	Transfer	191	DID	UCT[L]	...
		5. India			−.455 (.53)	Transfer and bank account	185			
Bazzi, Sumarto, and Suryahadi (2012)	Indonesia	6. Indonesia	Unconditional cash transfer	Alcohol and tobacco	−.0001*** (.00)	First disbursement	4,267	DID	UCT	Separate agency
		7. Indonesia			.0001* (.00)	Second disbursement	4,954			
		8. Indonesia			.0000 (.00)	Average across both	5,330			
Haushofer and Shapiro (2013)	Kenya	9. Kenya	GiveDirectly Unconditional Cash Transfer Program	Alcohol	−.017 (.02)		903	RCT	UCT[NL]	...
		10. Kenya		Tobacco	−.003 (.00)		903			
Cunha (2012)	Mexico	11. Mexico	Programa de Apoyo Alimentario	Alcohol	.336 (.40)		4,777	RCT	UCT	Separate agency
		12. Mexico		Tobacco	−.218 (.14)		4,777			

	Country		Program	Temptation good	Estimate (SE)	Specification	Sample size	Method	Type	Agency
Schluter and Wahba (2004)	Mexico	13. Mexico	PROGRESA	Tobacco	−.029 (.26)	Benefit (dummy)	5,579	RCT	CCT	Transferring agency
		14. Mexico			−.001 (.00)	Benefit (level)	5,579			
Gitter (2006)	Nicaragua	15. Nicaragua	Red de Protección Social	Alcohol and tobacco	−.010* (.01)	Year 1, excl. controls	2,550	DID	CCT	Transferring agency
		16. Nicaragua			−.001 (.01)	Year 2, excl. controls	2,550			
		17. Nicaragua			−.013* (.01)	Year 1, incl. controls	2,550			
		18. Nicaragua			−.003 (.01)	Year 2, incl. controls	2,550			
Maluccio and Flores (2005)	Nicaragua	Nicaragua	Red de Protección Social	Alcohol and tobacco	4.251		1,359	RCT	CCT	Transferring agency
Perova (2011)	Peru	19. Peru	Juntos	Alcohol	−.113** (.05)		1,525	PSM	CCT	Separate agency
		20. Peru			.210* (.12)		10,671	IV		
Dasso and Fernandez (2013)	Peru	21. Peru	Juntos	Alcohol: beer, whisky, rum, pisco	−.002 (.00)	2009	3,772	Recently vs. less recently paid	CCT	Separate agency
		22. Peru			.005 (.00)	2010	2,716			
Evans et al. (2014)	Tanzania	23. Tanzania	TASAF CCT Pilot Program	Cigarettes, tobacco, and snuff	−3.322 (3.16)	ETT midline	3,436	RCT	CCT	Separate agency
		24. Tanzania			−3.098 (4.24)	ITT midline	3,436			
		25. Tanzania			−2.470 (3.16)	ETT endline	3,131			
		26. Tanzania			−2.312 (4.31)	ITT endline	3,131			

Note. CCT is conditional cash transfer; UCT is unconditional cash transfer with no mention of the presence or absence of labeling. UCT^L (UCT^{NL}) denotes unconditional cash transfer programs in which beneficiaries have explicitly (not) been exposed to some labeling. ITT is the intent-to-treat estimator. RCT is randomized controlled trial. DID is differences in differences. PSM is propensity score matching. TASAF is the Tanzania Social Action Fund. Where studies do not report sample sizes explicitly for questions concerning the consumption of temptation goods, we report the overall sample size of the study. The reported impacts on total expenditures (and corresponding SE in parentheses) presented in 2012 PPP (purchasing power parity) are calculated by inflating the impact in local currency in the various base years (the year the data were collected, or as close to that as could be inferred) to their 2012 values using the inflation GDP deflator (annual %) before dividing by the 2012 PPP conversion factors for private consumption (local currency unit per international $). Both indicators used in the PPP conversion come from the World Development Indicators database (http://data.worldbank.org/data-catalog/world-development -indicators).

[a] Statistical significance is reported but not a standard error.
* Significant at the 10% level.
** Significant at the 5% level.
*** Significant at the 1% level.

TABLE 3
STUDIES WITH ALTERNATIVE ESTIMATED IMPACT OF TRANSFER ON ALCOHOL OR TOBACCO CONSUMPTION

Reference	Country	Program Name	Temptation Good	Impact	Detail on Impact	Sample Size	Methodology	CCT/ UCT	Data Collection Agency
Report proportional change in temptation good expenditures:									
Benedetti, Ibarrarán, and McEwan (2016)	Honduras	Bono 10,000	Alcohol and tobacco	−.170 (.167)	Excl. controls	270	RCT	CCT	Separate agency
				−.174 (.164)	Incl. some controls	270			
				−.008 (.170)	Incl. all controls	270			
Report proportion of total household expenditures:									
Braido, Olinto, and Perrone (2012)	Brazil	Bolsa Alimentaria and Bolsa Escola	Alcohol, tobacco, and gambling	−.003 (.002)		1,006	DID	CCT	Separate agency
Rubalcava, Teruel, and Thomas (2002)	Mexico	PROGRESA	Alcohol and tobacco	−.0025 (.0018)		14,437	RCT	CCT	Transferring agency
Schluter and Wahba (2004)	Mexico	PROGRESA	Tobacco	−.02 (.09)	Benefit dummy	5,579	RCT	CCT	Transferring agency
				−.51 (.398)	Benefit level	5,579			
Maluccio and Flores (2005)	Nicaragua	Red de Protección Social	Alcohol and tobacco	.1		1,359	RCT	CCT	Transferring agency
Report probability of alcohol abuse in household:									
Angelucci (2008)	Mexico	Oportunidades	Alcohol abuse	−.042*** (.016)		12,700	RCT	CCT	Separate agency
Report probability of consumption:									
Benedetti, Ibarrarán, and McEwan (2016)	Honduras	Bono 10,000	Alcohol and tobacco	−.013 (.009)	Excl. controls	3,839	RCT	CCT	Separate agency
				−.014 (.009)	Incl. some controls	3,839			
				−.013 (.010)	Incl. all controls	3,839			

Study	Country	Program	Good	Estimate (SE)	Subgroup	N	Method	Transfer type	Transferring agency
Gutiérrez et al. (2004)	Mexico	Oportunidades	Alcohol	−11%*** (.026)	Rural, 15–21-year-olds incorporated in 1998	2,635	PSM	CCT	Transferring agency
				−13%*** (.029)	Rural, 15–21-year-olds incorporated in 2000	2,375			
				−4%*** (.015)	Urban, 15–21-year-olds	3,878			
			Tobacco	−15%*** (.029)	Rural, 15–21-year-olds incorporated in 1998	816			
				−13%***[f] (.024)	Rural, 15–21-year-olds incorporated in 2000	782			
				−2%** (.007)	Urban, 15–21-year-olds	3,878			
Galárraga and Gertler (2009)	Mexico	Oportunidades	Alcohol	−40%[a]	Females	1,964	IV	CCT	Separate and transferring agencies
			Tobacco	−46%[a]	Males	1,779			
Report number of days consumed in past week (for children 1–7 only):									
Gilligan and Roy (2013)	Uganda	WFP Cash Transfers to ECD centers	Beer	−.198 (.198)	ITT	2,703	RCT	UCT[L]	. . .

Note. CCT is conditional cash transfer; UCT is unconditional cash transfer with no mention of the presence or absence of labeling. UCT[L] denotes unconditional cash transfer programs in which beneficiaries have explicitly been exposed to some labeling. ITT is the intent-to-treat estimator. RCT is randomized controlled trial. DID is differences in differences. PSM is propensity score matching. IV is instrumental variables. WFP is World Food Programme. ECD is Early Child Development. Where studies do not report sample sizes explicitly for questions concerning the consumption of temptation goods, we report the overall sample size of the study. SE in parentheses.

[a] Statistical significance is not reported.

** Significant at the 5% level.

*** Significant at the 1% level.

average pooled effect through meta-analysis, and all of which are included in the vote-counting exercise. In this section, we discuss the results of the meta-analysis and vote-counting exercises in turn, before presenting a discussion of level estimates and finally qualitative results.

Meta-analysis

Of the 50 impact estimates we identify, 29 report the impact of cash transfers on total temptation good expenditure, and of these, 26 estimates do so with detailed enough reporting (i.e., a standard error or standard deviation and sample size) to be included in our meta-analysis. A number of these, however, are multiple estimates from a single study. Selecting only a preferred estimate to use in our primary meta-analysis specification brings us to a final sample of 19 estimates.

Across these 19 estimates from three continents, we find cash transfers to have an average effect of -0.176 standard deviations $[-0.350, -0.002]$, significant at the 5% level, on total temptation good expenditure. Figure 2 shows a forest plot of the standardized individual effect sizes, as well as the pooled effect, with 95% confidence intervals. Squares and brackets in the figure indicate the effect sizes and 95% confidence intervals, respectively, with the relative size of squares proportional to their weight in the mean. In appendix C we also provide a table of standardized effect sizes, standard errors, and confidence intervals for each individual estimate (table C1).

We perform three robustness checks on this result: first, we remove the largest estimates and recalculate the pooled effect; second, we reestimate the model, substituting the less preferred of the multiple estimates for the preferred; and third, we estimate the model using only estimates from RCTs. In the first of these robustness checks we remove two estimates for Nicaragua (estimate numbers 17 and 18), which from the forest plot are distinguishably more negative than the general trend among the remaining estimates. Reestimating our random-effects model with these two largest values removed still yields a negative but insignificant impact on temptation good expenditure of -0.016 standard deviations $[-0.039, 0.006]$. Similarly, using the less preferred duplicate estimates yields an insignificant negative impact equal to -0.133 standard deviations $[-0.282, 0.016]$. Finally, using only estimates from RCTs also yields an insignificant negative impact of -0.019 standard deviations $[-0.045, 0.008]$. Thus, the result that cash transfers do not, on average, increase spending on alcohol and tobacco is robust to changes in the sample.

Nonetheless, there may be heterogeneity in the impact of cash transfers on temptation good consumption: namely, geographic variation and differences in program design could feasibly lead to different impacts. We perform
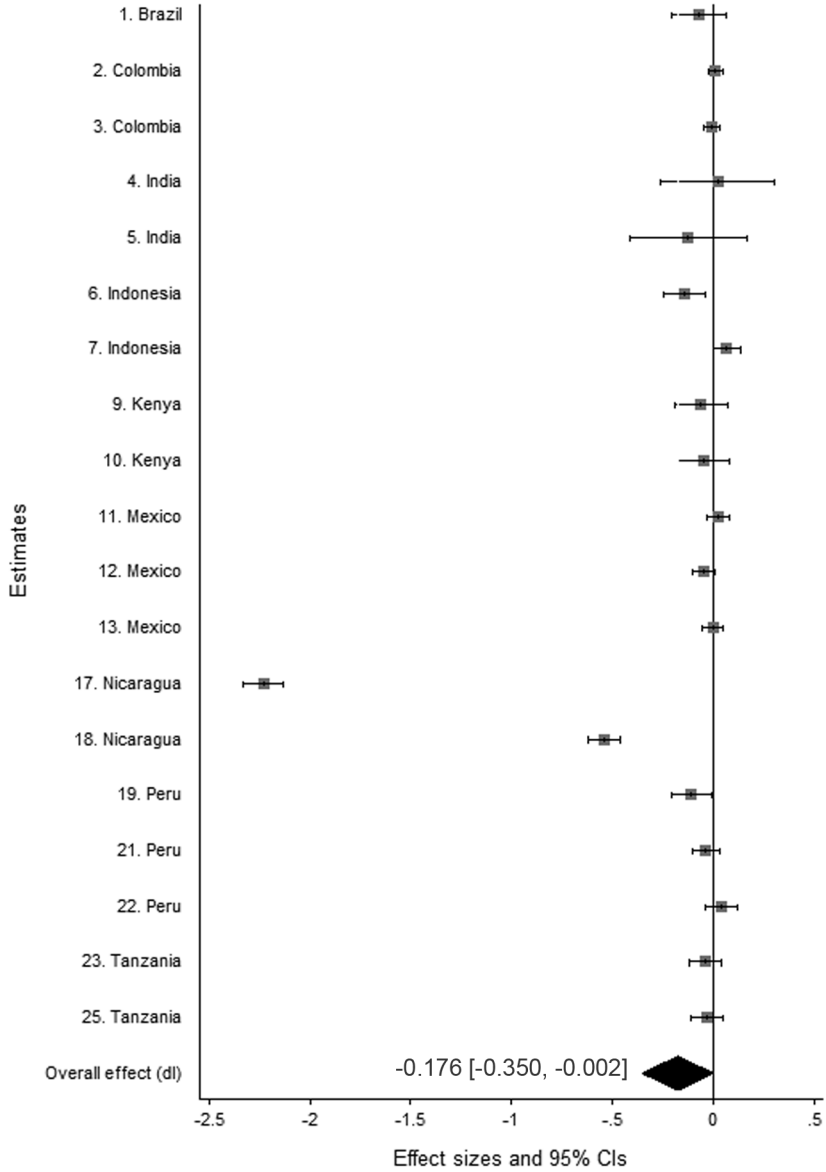
Figure 2. Effect sizes of cash transfers on temptation good expenditures. Labels indicate the estimate code listed in table 2 and the country to which the estimate belongs. Squares and brackets indicate effect sizes and 95% confidence intervals, respectively. Relative size of squares is proportional to their weight in the mean. Mean effect size is estimated using a DerSimonian-Laird random-effects model, as described in the text.

subgroup analysis using our preferred estimates in order to investigate this possibility. The results are presented in table 4. Looking first at regional variation, if we separate the estimates in Latin America from those in Africa and in Asia, we find that cash transfers have a negative effect on temptation good expenditure across all regions but that the result is only significant (and largest) in Latin America, at −0.268 standard deviations [−0.525, −0.012]. This may be driven in part by the fact that Latin America accounts for many more estimates (11 as compared to just four in each of Africa and Asia), and thus the average effect from this region is calculated across a much larger pooled sample size.

Turning next to program design, both conditional cash transfer programs and unconditional programs have negative effects on temptation good expenditure. However, this negative effect is only significant, and much larger, in the case of CCTs. The pooled effect for CCTs is −0.273 standard deviations [−0.544, −0.003] compared to −0.025 standard deviations [−0.078, 0.028] for UCTs. This difference could be explained by the fact that, through their imposition of school enrollment and attendance conditions, CCTs divert money toward schooling and by the social messaging that accompanies them. This is consistent with our finding that UCTs where labeling is explicitly mentioned in the documentation have, on average, a larger negative impact on temptation good spending (−0.049 standard deviations [−0.252, 0.153]) than those that have no explicit mention of labeling (−0.025 standard deviations [−0.083,

TABLE 4

AVERAGE EFFECT SIZES OF CASH TRANSFERS ON TEMPTATION GOOD EXPENDITURES FOR VARIOUS SUBGROUPS

Subgroup	Overall Effect	Lower Bound (95%)	Upper Bound (95%)	Number of Estimates
Region:				
Latin America	−.268	−.525	−.012	11
Africa	−.042	−.089	.006	4
Asia	−.039	−.183	.106	4
Condition:				
CCT	−.273	−.544	−.003	11
UCT	−.025	−.078	.028	8
Labeling:				
Labeled UCTs	−.049	−.252	.153	2
Unlabeled UCTs	−.025	−.083	.033	6
Data collection entity:				
Transfer agency	−.923	−2.118	.271	3
Separate agency	−.026	−.063	.012	10
Not reported	−.003	−.028	.021	6
Program duration:				
One year or less	−.460	−.982	.063	5
Greater than 1 year	−.074	−.159	.011	14

Note. Mean effect size is estimated using a DerSimonian-Laird random-effects model, as described in the text. CCT is conditional cash transfer; UCT is unconditional cash transfer.

0.033]). However, the main result is that neither CCTs nor UCTs, with or without labeling, increase expenditure on temptation goods.

Finally, there is a negative relationship between transfers and the consumption of alcohol and tobacco, regardless of the length of time that beneficiaries have been receiving transfers. The time during which beneficiaries have been receiving transfers is—on average for our preferred sample—1.6 years and ranges from 6 months to 3 years. Both cash transfer programs with a duration of 1 year or less and those lasting longer than 1 year have an insignificant negative effect on total expenditure on temptation goods, with effect sizes of −0.46 standard deviations [−0.982, 0.063] and −0.074 standard deviations [−0.159, 0.011], respectively.

Vote Counting

The meta-analysis results show that cash transfers do not increase total expenditure on temptation goods, irrespective of geography, program design, or program duration. While this is a strong result and one that holds across several robustness checks, it does not incorporate evidence from studies that use alternative measures of temptation goods. We find 17 estimates from nine studies that fall into this category. We conduct a vote-counting exercise to be able to analyze the overall distribution of effects, including those that cannot be included in the meta-analysis. While the meta-analysis draws on 19 estimates (in our preferred specification), the vote counting includes all 50 impact estimates.

Across all 50 impact estimates (from the 19 studies), there are 12 estimates that are negative and significant, 30 that are negative and insignificant, six that are positive and insignificant, and two that are positive and significant (table 5 panel A). In other words, 84% of estimates are negative, and just 4% of estimates are significant and positive (table 6 and fig. 3A). One of those two positive significant results is an unconditional cash transfer program in Indonesia: in the first disbursement, the impact was slightly negative and highly significant, whereas in the second disbursement, the impact was slightly positive and mildly significant. The size of the coefficient is almost identical to that for expenditures on prepared food. The other positive result, from Peru's Juntos program, is from a paper that uses two different methods, matching and instrumental variables, and finds opposite results from the two estimates on alcohol consumption: a moderately significant negative impact from the matching estimate and a weakly significant positive impact from the instrumental variables estimate. Estimates on other outcomes are mostly consistent across the two estimation methods. Thus, in both cases of positive significant results, the impacts are weakly significant and are not consistent across estimates within

TABLE 5

DISTRIBUTION OF ESTIMATES OF THE IMPACT OF CASH TRANSFERS ON TEMPTATION GOODS

	Negative and Significant	Negative (or 0) and Insignificant	Positive and Insignificant	Positive and Significant	All
	A. All estimates				
Estimates	12	30	6	2	50
From X studies	6	15	5	2	19
From X interventions	5	13	5	2	14
	B. All estimates—RCTs only				
Estimates	1	19	3	0	23
From X studies	1	6	2	0	9
From X interventions	1	6	2	0	8
	C. Only total expenditure estimates				
Estimates	5	17	5	2	29
From X studies	4	11	5	2	13
From X interventions	4	10	5	2	10
	D. Only total expenditure estimates—RCTs only				
Estimates	0	9	2	0	11
From X studies	0	4	2	0	5
From X interventions	0	4	2	0	5

Note. RCT stands for randomized controlled trial. X denotes the number of studies or interventions that the estimates come from.

the same study. Furthermore, the effect sizes are very small: one is less than a penny, whereas the other is 21 cents. Even if those estimates accurately reflect changes in expenditures, the changes are trivial.

If we instead consider only the 23 estimates from nine RCTs, we find one estimate that is negative and significant, 19 that are negative and insignificant, three that are positive and insignificant, and zero that are positive and significant (table 5 panel B). In other words, 87% are negative, and none are positive and significant (table 6 and fig. 3B). If we limit the vote count analysis to estimates on total expenditures—consistent with the meta-analysis—the results are similar (table 5 panels C and D; figs. 3C and 3D). Thus, the vote-counting results are consistent with the meta-analysis.

Level Estimates

Other studies, while not estimating the impact of transfers on consumption of or expenditures on temptation goods, have sought to quantify how many beneficiaries use transfers for temptation goods or how much of the transfers has been spent on temptation goods; these studies rely on either surveys or focus groups. We identified 11 such studies, representing programs in eight countries: six in Africa, one in Asia, and one in the Middle East (table 7). Four of the studies identified a proportion of beneficiaries or households that spent

<div align="center">

TABLE 6

DISTRIBUTION OF ESTIMATES OF THE IMPACT OF CASH TRANSFERS ON TEMPTATION GOODS (%)

</div>

	Negative and Significant	Negative (or 0) and Insignificant	Positive and Insignificant	Positive and Significant	Total
All estimates	24	60	12	4	100
Only total expenditure estimates	17	59	17	7	100
All estimates—RCTs only	4	83	13	0	100
Only total expenditure estimates— RCTs only	0	82	18	0	100

Note. RCT stands for randomized controlled trial.

some or all of the transfer on temptation goods. The median proportion was 1.2%, a tiny fraction of households. Even in the one outlier, Lesotho's Cash and Food Transfers Pilot Project, where about 6% of beneficiaries admitted to spending some of their transfer on alcohol and cigarettes, the study quotes a recipient as saying that it happens "only in rare and discreet cases" (Devereux and Mhlanga 2008, 33). Two more studies, from Malawi and Zimbabwe, identify the proportion of transfers spent on temptation goods: in both cases,
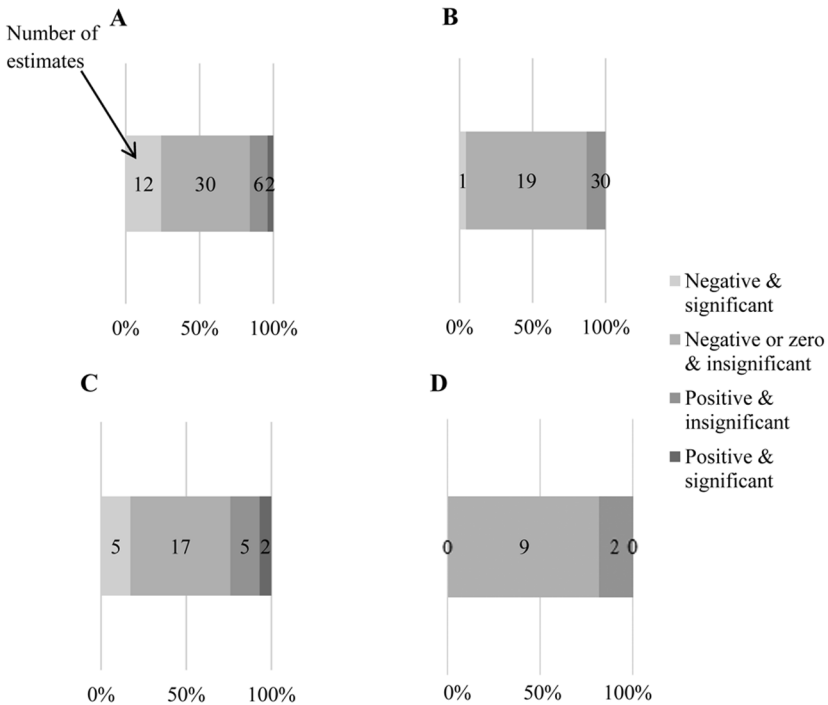

Figure 3. Distribution of estimates of the impact of cash transfers on temptation goods. *A*, All estimates; *B*, all estimates—RCTs (randomized controlled trials) only; *C*, total expenditure estimates only; *D*, total expenditure estimates—RCTs only.

TABLE 7

STUDIES WITH ESTIMATED SURVEY OR FOCUS GROUP LEVELS OF TRANSFER EXPENDITURE ON ALCOHOL OR TOBACCO

Reference	Country	Program Name	Temptation Good	Impact
Aker (2015)	Democratic Republic of Congo	Income Support to Internally Displaced Persons	Doughnuts and beer	<1% of households
Biron (2012)	Jordan	UNHCR cash grants	Alcohol, tobacco, and medicines	1.3% of households
Brewin (2008)	Kenya	Kerio Valley Cash Transfer Pilot	General	No reports of use on temptation goods
Slater and Mphale (2008)	Lesotho	World Vision Cash and Food Transfers Pilot Project	Alcohol and tobacco	No significant increase
Devereux and Mhlanga (2008)	Lesotho	Cash and Food Transfers Pilot Project	Alcohol and cigarettes	6.4% of recipients
Miller, Tsoka, and Reichert (2008)	Malawi	Mchinji Social Cash Transfer Pilot Scheme	Alcohol	1.1% of recipients
Devereux, Mvula, and Solomon (2006)	Malawi	Food and Cash Transfers	Alcohol, cigarettes, entertainment	0.1% of transfer
Harvey and Savage (2006)	Malawi and Zambia	Oxfam's cash transfers	Alcohol	No reports of use on temptation goods
Humphreys (2008)	Vietnam	Nonemergency cash grants in An Loc commune	Alcohol and gambling	No reports of use on temptation goods
Phiri (2012)	Zimbabwe	Government of Zimbabwe Harmonised Social Cash Transfer	Alcohol	Marginal increase in consumption
Román (2010)	Zimbabwe	Zimbabwe Emergency Cash Transfer (ZECT) Pilot Program	Alcohol and tobacco	<0.5% of transfer used on temptation goods

the proportion is under 0.5%. The remaining studies simply report that they found no evidence that households were purchasing temptation goods, except one case that reports a "marginal increase" (Phiri 2012).

This evidence is significantly less convincing than the impact estimates, which look at total expenditures rather than transfer expenditures alone, as transfer and other income are fungible. A household could, for example, use the transfer income entirely for education investments but at the same time decrease spending on education from regular income by 10%. Then they could use that 10% of regular income for temptation goods. In the respondent's view, none of the transfer income would have been used for temptation goods, although clearly the transfer is what enabled the increased expenditures. Despite this caveat, these level estimates are consistent with the finding of insignificant quantities being spent on alcohol and tobacco that was already observed in the more reliable estimates on overall expenditures.

Qualitative Results

While the impact estimates suggest either a negative or zero average effect, and the level estimates suggest only tiny fractions of beneficiaries using transfer resources to purchase temptation goods, qualitative reports sometimes tell a different story. Consider the following examples:

i) In Malawi, researchers reported from focus groups that "in our village, there were certain men who wasted their money even though they had families and children," and "We heard of four men who received their rations on a Thursday. They all went to a nearby popular drinking bar" (Devereux, Mvula, and Solomon 2006, 48, 49).

ii) In Bolivia, "Of the 35 subjects interviewed, 20 admitted they knew people who misspent the cash transfers." However, "Many mentioned the media as their main source of information regarding any misspending" (Vaughan 2010, 19, 23).

iii) In Kenya, "Cases of misuse of funds were reported in the two sites: according to key informants, in some cases, male recipients have used some of the cash to buy alcohol, although this is relatively rare (only three cases reported, with the majority of the cash being used for consumption and investment)" (Onyango-Ouma and Samuels 2012, 10).

iv) In Swaziland, a focus group participant reported that "men don't return home on pay-days; some have found other women to spend the money with" (Devereux and Jere 2008, 34).

v) In Uganda, participants and informants observed that "some beneficiaries—especially men—have used the cash transfer in over-drinking alco-

hol," and "Some older men especially drink all the money" (Bukuluki and Watson 2012, 72).

How do we reconcile these anecdotes with the extremely insignificant or even negative effects we observed earlier? First, the results previously discussed do not indicate that no single beneficiary uses his or her transfer on alcohol. For example, the Malawi anecdote above comes from a study that measured the proportion of transfers that were spent on alcohol; the proportion was 0.1%. So although interviewees had "heard of four men" or knew "certain men," these numbers seem very small. What the quantitative results earlier claim is that, on average, there is no positive impact of transfers on alcohol expenditures.

Second, most of these reports are not with reference to one's own household but rather to other individuals who respondents may know who spend the money on alcohol and tobacco. However, multiple respondents may well know the same person in the community who has a reputation for high levels of alcohol or tobacco consumption. These anecdotes can be subject to "saliency bias," in which individuals pay attention to highly noticeable factors and dramatic events: a village drunkard stands out and is likely to come up disproportionately in discussions.

An alternative possibility is that the respondents in household surveys are unaware of how their household resources are spent. For example, if a husband takes household resources and spends them on alcohol without the wife's knowledge and the wife is the survey respondent, then such spending might show up in qualitative reports from other households but be missing in the impact estimates. However, it seems unlikely both that (1) the surveys consistently interview the nondrinking member of the household and that (2) this member is consistently ignorant of these expenditures, particularly in low-income households with limited cash income.

These results underline the importance of complementing qualitative reports with quantitative data and are reconcilable with the earlier quantitative finding that, on average, there is no increase in the consumption of temptation goods.

Discussion

In this section we discuss some of the implications and challenges related to this analysis. One principal concern when studying the consumption of goods such as alcohol and tobacco, especially in the context of a program in which beneficiaries are encouraged not to use the resources on those goods, is that beneficiaries will report low expenditures on those goods because they want to minimize the risk of expulsion from the program or other potential negative

consequences. This is known as "social desirability bias." There is some evidence from undergraduate students in the United States that self-reports of alcohol consumption can be biased downward (Davis, Thake, and Vilhena 2010). In developing contexts, this is much less explored for alcohol and tobacco consumption. (For sexual behaviors, it has been explored extensively.)[6] However, we do not expect this to be a major problem here, for the following reasons.

First, the impact estimates presented here are usually based on detailed expenditure surveys that ask a household respondent how much the household spends on each of a long list of items. Alcohol and tobacco are not singled out. For the estimates of what proportion of households spent any resources on temptation goods, alcohol and tobacco may be singled out, which could explain why several studies found zero reports of any spending on temptation goods. However, those estimates merely provide supportive evidence to the more robust impact estimates.

Second, transfer income is not asked about separately, so households would have to recall the amount of their overall income spent on temptation goods before the program and report a similar amount later. The simplest solution for households seeking to appease an interviewer would be to report zero or extremely low expenditures on alcohol and tobacco. This is especially true since household surveys are administered infrequently, and so recalling previous reports may be difficult. In that case, we would expect to see a much starker pattern of significant negative impacts. On the contrary, we observe just 24% of all impacts on expenditures to be negative and significant, and 9% for RCTs. The far more common result is an insignificant difference: the outcome in all 11 randomized trials (fig. 3D). This does not look like systematic social desirability bias.

Third, for the studies with estimates of impact on total temptation good expenditure, we examined which entity was responsible for data collection. Social desirability bias could manifest in the form of more negative effects on the consumption of temptation goods for programs where data collection was carried out by the agency in charge of providing the cash transfers than for

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[6] This issue has been studied more extensively for sexual behavior in developing countries, and the evidence has been inconsistent: in Malawi and Kenya, e.g., young women were more likely to report ever having had sex in a face-to-face interview, whereas they were likely to report more total partners in an audio computer-assisted self-interview (Mensch et al. 2008). In Zimbabwe, respondents also reported fewer partners in face-to-face interviews (Gregson et al. 2002). A study in Tanzania found female adolescents were more honest about sexual infection in face-to-face interviews, whereas males were less honest (Plummer et al. 2004). A fuller list of relevant references is available in Handa et al. (2014).

those where separate agencies were in charge of data collection. We find that cash transfers have an insignificant negative effect on temptation good expenditure irrespective of the type of organization that gathered consumption data, although the effect is larger in cases in which data were gathered by the agency providing the cash transfers (see table 4).

Another potential concern is that the result that cash transfers do not increase the consumption of temptation goods (on average) could be biased by incomplete reporting of results. Indeed, most studies reviewed are far from adhering to the Consolidated Standards of Reporting Trials' (CONSORT) guidelines (Schulz, Altman, and Moher 2010) for reporting the results of randomized trials recommended by Cochrane (2013), one of the main registries of systematic reviews. Some of the studies reviewed fail to report critical information (e.g., standard errors) without which estimates cannot be included in a meta-analysis, while many others report information with insufficient detail, such that we had to make assumptions in order to include the estimates in our meta-analysis. For example, many studies do not report the number of observations for treatment and control for each outcome measured separately, so we assume that the overall ratio of treatment to control villages applies to the distribution of treatment and control observations for every regression. In this case, however, the vote-counting exercise proves useful. The vote-counting exercise includes all estimates, even those with insufficient reporting to be included in the meta-analysis, and yields the same result that cash transfers overwhelmingly have either no effect or a significant negative effect on the consumption of temptation goods.

An additional concern could be that these studies were not sufficiently statistically powered to capture consumption impacts at all, whether on temptation goods or other categories of consumables. For this, we focus on the six positive and insignificant estimates in more detail. These six estimates come from five studies (each from different countries around the world), most of which report the estimated impact of cash transfers for total expenditure on temptation goods; Maluccio and Flores (2005) also present an estimate of the impact on the proportion of expenditures. For each of these studies, we examine whether they had sufficient statistical power to identify significant impacts on overall consumption using the same estimation methodology (table 8). We observe that in every case, the studies finding positive and insignificant estimates for temptation goods at the same time produce significant (positive) estimates for the impact on overall consumption. Because identifying impacts on individual consumption items or categories requires greater statistical power than identifying effects on total consumption, we also look at whether these studies find significant impacts on individual consumption items other than

TABLE 8
OVERALL CONSUMPTION IMPACTS FOR STUDIES WITH POSITIVE AND INSIGNIFICANT
ESTIMATES ON TEMPTATION GOODS

Reference	Country	Program Name	Significant Impact on Total Consumption	Disaggregated Consumption Estimates		
				Total	Number Significant	Percentage Significant
Report total expenditures:						
Attanasio et al. (2005)	Colombia	Familias en Acción	Yes	34	17	50
Gangopadhyay, Lensink, and Yadav (2013)	India	Unconditional Cash Transfer Pilot	Yes	6	4	67
Cunha (2012)	Mexico	Programa de Apoyo Alimentario	Yes	32	7	22
Maluccio and Flores (2005)	Nicaragua	Red de Protección Social	Yes	16	9	56
Dasso and Fernandez (2013)	Peru	Juntos	Yes	14	4	29
Report proportion of expenditures:						
Maluccio and Flores (2005)	Nicaragua	Red de Protección Social	Yes	16	9	56

Note. Analysis of the statistical power of evaluations to identify significant impacts on consumption, for those studies that find positive insignificant impact estimates on the consumption of temptation goods. We present both whether these studies find significant impacts on total consumption as well as the number and percentage of significant estimates they find for disaggregated consumption items. We are conservative in our calculations of the latter, counting only the most disaggregated estimates in a given study (e.g., we exclude the estimates for grains in studies that further disaggregate these into estimates for rice, pasta, and cereal). When considering disaggregated consumption estimates, we exclude estimates on alcohol and tobacco in these calculations so as to compare the statistical power of the evaluations to identify nontemptation good consumption estimates with that for identifying temptation good estimates.

temptation goods (also in table 8). We find that every study finding positive and insignificant estimates for temptation goods produces significant estimates for at least 20% of the disaggregated consumption items. This suggests that the insignificance of these temptation good estimates does not derive from a lack of statistical power. As a more conservative approach, one could examine the upper bound of the 95% confidence interval for the 19 estimates included in the meta-analysis. Four of those upper bounds are less than zero (i.e., the point estimates are statistically significantly negative), and roughly 80% of the upper bounds are less than 0.1 standard deviations (fig. 2 and table C1). In other words, while most of the individual studies cannot rule out some positive change, any possible change would be small. Taken together, the evidence

strongly suggests that, on average, beneficiaries do not use their transfers on alcohol and tobacco.

Conclusion

We have investigated evidence from around the developing world, including Latin America, Africa, and Asia. There is clear evidence that transfers are not consistently used for alcohol or tobacco in any of these environments. On average, across all regions and program modalities and durations, we find that cash transfers actually decrease total expenditure on temptation goods. If we rely on randomized trials only, we find a negative, statistically insignificant average effect. This negative direction could be a combination of the substitution effect, the labeling effect, and—potentially—the effect of women controlling more resources (the household bargaining effect), together outweighing the income effect. The fact that, while no programs produce a significant positive impact, we observe a larger and more significant negative impact of conditional cash transfer programs than unconditional programs and, among unconditional programs, a greater impact for those programs that explicitly use labeling provides suggestive evidence for the substitution and labeling effects. We observe no significant positive impacts in any geographical region, and programs in Latin America have the largest negative effect on temptation good consumption.

These results provide strong evidence that concerns that transfers will be used on alcohol and tobacco are unfounded. We do have estimates from Peru that beneficiaries are more likely to purchase a roasted chicken at a restaurant or some chocolates soon after receiving their transfer (Dasso and Fernandez 2013), but hopefully even the most puritanical policy maker would not begrudge the poor a piece of chocolate.

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