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Health Risk Information, Social Stigma and Demand for Condoms: Experimental Evidence from Ghana

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Abstract

I investigate two potential barriers that may limit demand for condoms: inadequate information about health risks and fear of social stigma associated with condom purchases. Using a randomized experiment, I test whether providing information about (i) health risks and (ii) peers' views regarding the social appropriateness of condom purchase can increase condom demand among young adults in Ghana. I find that providing health risk information led to a 32% increase in demand. In contrast, providing additional information about peers' views regarding the social appropriateness of condom purchase had no meaningful effect on condom demand. Moreover, the effect of health risk information on condom demand is persistent. Interestingly, even though I document persistent effects of health risk information on condom demand, I find that information has temporary effects on perceptions about the appropriateness of using condoms. These results suggest that targeted information can durably shift health behavior even when underlying perceptions remain slow to change.

JEL codes: D83, D91, I12, J13
Keywords: beliefs, misperceptions, information, health risk, social stigma, learning

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1 Introduction

More than two-thirds of new HIV infections worldwide occur in Sub-Saharan Africa, where the epidemic remains a severe public health challenge (UNAIDS, 2020). Condoms are a highly effective and inexpensive technology to prevent HIV and other sexually transmitted infections, and are widely available through both public and private distribution channels. Nevertheless, uptake across the region remains low (Boadu, 2022; Broderick et al., 2023). This gap between availability and adoption suggests that the constraints to condom demand may not be only financial or logistical, but also behavioral and social in nature.

One salient example of such a barrier is social stigma. The 2013 Pew Global Attitudes Survey found that in countries such as Pakistan, Nigeria, and Ghana, more than half of adults viewed contraceptive use as morally unacceptable, with similar attitudes reported by sizable minorities in Malaysia and Tunisia (Pew Research Institute, 2013). Such attitudes suggest that opposition to condom use is influenced by normative beliefs about sexual behavior and morality, rather than problems of access.

If opposition to condom use is rooted in moral or normative beliefs, then the type of information provided becomes critical. Standard health messages emphasizing HIV risk and condom effectiveness may not be sufficient if individuals fear that using condoms signals promiscuity. In such settings, information that corrects misperceptions about peer views can be more effective, because young adults may have inaccurate beliefs about the extent of opposition to condoms among their peers, given that the younger generation tend to hold more progressive views on sexual behavior than members of older generations.

On the other hand, if low rates of condom use reflect a lack of accurate knowledge about health risks, then clear information about the protective benefits of condoms is more likely to influence behavior. Providing evidence on how condoms reduce the risk of HIV and other sexually transmitted infections may increase demand by lowering uncertainty and correcting false beliefs.

This paper provides experimental evidence on how health risk information and peer views about the social acceptability of condom purchase interact to shape demand for condoms. In particular, I test whether access to health risk information alone can increase condom demand. I also examine whether complementing health risk information with accurate information on peer views about condom purchases has any additional effects. This extension is motivated by the literature on social norms and peer influence, which shows that peers shape behavior through norms and expectations (Sacerdote, 2001; Bursztyn et al., 2020).

I study these issues in Ghana, where condom demand remains low despite widespread awareness of modern methods of contraceptives and broad availability through both public and private channels. Anecdotal evidence suggests that premarital sex is frowned upon, which may in turn discourage contraceptive purchase, as obtaining condoms can serve as a credible signal of sexual activity. This makes Ghana a relevant setting to examine how both health risk information and peer view information can influence condom demand.

The experimental design is set up to address two main questions: (i) does the provision of health risk information affect demand for condoms, and if so, how persistent are the effects? (ii) do young adults have inaccurate beliefs about their peers' views toward

condom use, and can correcting these misperceptions affect their own perceptions? I also investigate whether these effects depend on the social status of the peers whose views are shared.

The experimental design combines a supplementary survey with a main experiment. The supplementary survey was implemented to document and quantify types of misperceptions that may exist, while the main experiment, comprised of a baseline and an endline survey, was used to deliver the information treatments and to test the role of risk information and the role of correcting misperceptions about peers' views on perceptions and condom demand.

The supplementary survey was conducted in person among 381 undergraduate students at Kwame Nkrumah University of Science and Technology (KNUST) in Kumasi, Ghana. The average respondent was 21 years old, and the sample was predominantly male (82 percent). All students were drawn from the same campus and thus shared a common social network. In this survey, respondents were asked to state their own views on a set of statements about condom use and purchase and were incentivized to guess the share of their peers in the room who agreed with each statement. For example, while 42 percent of respondents agreed that "people who buy condoms have multiple sexual partners," they believed that 53 percent of their peers agreed with this statement. Similar gaps appeared across other statements reflecting moral and cultural views on contraceptive use. I interpret these systematic gaps as evidence that students overestimate the negativity of their peers' views toward condoms.

The main experiment was conducted with 2,151 unmarried male students aged 18–39 at the same university. In the baseline survey, one quarter of respondents were randomized into the $Risk$ treatment, where they received accurate information about the national burden of HIV/AIDS, learning that over 340,000 people were currently living with the disease, with new infections concentrated among young people, and the high daily number of unwanted pregnancies ending in abortions, particularly among women aged 20–24. This information emphasized that condom use provides effective protection against both sexually transmitted infections and unintended pregnancies. The remaining treatment arms are incrementally built on this specific treatment, allowing for identification of the marginal effects of adding peer view information and prestige cues to a common baseline of health risk messaging.

To examine whether correcting misperceptions about peers' views influences condom demand, another quarter of respondents were randomized into the $Risk\_Peers$ treatment, where they received the same risk information plus data on the true share of students who agreed with the statement "people who buy condoms have multiple sexual partners" based on the results of the supplementary survey. Because young adults may respond differently depending on the type of peers referenced, a third treatment arm ($Risk\_Peers(status)$) emphasized the prestige of the peers whose views were presented, identifying them as "elite" students including medical students, top academic performers, student leaders, and campus influencers. The final quarter of respondents were randomized into a control group that received no information.

At the endline survey, conducted two months later, I test the existence of stigma around condom purchase by measuring social image considerations. One concern is that the act

of publicly obtaining condoms may reflect embarrassment from being seen with condoms by peers, rather than low underlying demand. To disentangle privacy concerns from low demand due to moral disapproval concerns, respondents were randomly assigned to see condoms in either transparent or non-transparent packaging and informed that delivery would be made by a student representative. They were then asked in the survey to indicate whether they wished to receive the condoms.

The primary outcome is condom demand, proxied by voucher redemption. Following both the baseline and endline surveys, respondents received a voucher redeemable for a pack of condoms. I complement this measure with self-reported interest in condom vouchers and sexual behavior data collected during the endline survey. The secondary outcome is reported perceptions toward condoms, measured using survey questions on attitudes and knowledge, at both baseline and endline surveys. Because both voucher redemption and perceptions are measured repeatedly, I can assess the persistence of treatment effects over time.

I find that, after the information intervention, respondents who received any type of health risk information were more likely to indicate positive condom take-up intentions and then to proceed to actually acquire condoms than control respondents. For condom take-up intentions, 58% of the control group who received no information indicated interest, while take-up intentions for respondents who received any type of treatment is 64% (p=0.051). Likewise, for actual condom take-up, 13% of control respondents exchanged their vouchers for condoms, while the share goes up by 4 percentage points (p=0.05, a 32 percent increase) for treated respondents.

Disaggregating the results by specific treatment assignment, I find no evidence that information about peers' views on the social appropriateness of condom purchase and use or signaling the prestige/status of those peers had any meaningful effect on actual take-up of condoms. The main behavioral response is driven by the provision of health risk information.

At the endline, two months after the intervention, the behavioral effects persisted: treated respondents remained substantially more likely to request another package of condoms, with take-up rates 57 percent higher than those in the control group (where the actual baseline take-up rate was 13 percent). Regarding whether packaging conditions altered this effect, there is some indication of mild stigma, as respondents were somewhat more likely to redeem condom vouchers when the condoms were offered in non-transparent packaging rather than transparent packaging. Specifically, actual condom take-up rose from 11 to 16 percent in the control group and from 19 to 23 percent in the treatment group under non-transparent packaging. However, these within-group differences are not statistically significant. The main effect is driven by the information intervention, which increases condom take-up by 8 percentage points under transparent packaging and by 7 percentage points under non-transparent packaging, indicating that the impact of the information intervention is robust across packaging types.

This increase in actual condom take-up is concentrated among respondents who are likely to be sexually active, and the magnitudes are substantial. Among those who report having had a boy/girlfriend for at least six months (proxy for past sexual status), the actual take-up rate rises by 64 percent (from a baseline of 13 percent), and two months later, the

effect is 89 percent((from a baseline of 14 percent). In contrast, actual take-up rates do not differ significantly among respondents who are unlikely to be sexually active across treatment arms. This suggests that the health risk information primarily resonates with those for whom it is most personally relevant. The absence of any meaningful increase in redemption among the non-sexually active group also provides indirect reassurance that the condoms were not being redeemed for resale or unrelated purposes. Instead, the observed behavioral response appears to be concentrated among individuals with a genuine potential need for protection.

Perceptions toward condoms improved substantially after the information intervention but diminished over time. Treated respondents were initially 20 percentage points more likely to report positive attitudes and 44 percentage points more likely to recognize that condoms are safe and have no negative health effects after receiving information. However, these perceptual gains dissipated by the endline survey two months later.

Taken together, the results reveal a novel pattern: behavioral effects that persist even as perceptions converge. I argue that this persistence arises because temporal shifts in perceptions can set behavioral changes that reinforce themselves in motion. In particular, condoms are an experiential good: as a result, using them provides individuals with direct experience that may alter their prior beliefs about convenience, comfort, and/or social costs. Once respondents experiment with condoms in response to an initial information shock, they update their behavior in ways that persist even after the salience of the information diminishes. This mechanism is consistent with the literature on learning and habit formation, which shows that initial adoption can generate lasting behavioral change even after the original stimulus fades (Dupas, 2014; Fischer, 2019; DellaVigna, 2009).

This paper is related to the growing literature on information provision experiments, which show that providing accurate information can shape both beliefs and behavior across a wide range of economic domains, including schooling (Jensen, 2010), voting and charitable giving (DellaVigna and Kaplan, 2007), and vaccine uptake (Bartoš et al., 2022). In the health domain, a number of studies have investigated how information affects sexual behavior in developing countries. Dupas (2011) demonstrates that teenage girls in rural Kenya respond to relative risk information on HIV infection by reducing inter-generational sex, which leads to fewer cases of teenage pregnancies. Other studies, such as Chong et al. (2013), Jamison et al. (2013), and Ashraf et al. (2014) test different forms of sexual health interventions and report mixed effects on knowledge and behavior. I contribute to this body of work by showing that young men in Ghana respond to sexual health risk information by increasing their demand for condoms.

This paper also contributes to the literature on the demand for health products and learning in low- and middle-income countries. These studies emphasize how individuals learn about preventive health technologies through experimentation. Research on temporary price subsidies shows that even short-run adoption opportunities can generate persistent usage once households update their priors through direct experience (Dupas, 2009; Fischer, 2019). This work highlights the role of learning-by-doing and habit formation in sustaining health behaviors. I add to this literature by showing that condoms also function as experiential goods. Information prompts short-term changes in demand,

but once individuals try condoms, the behavior persists even after changes in perceptions fade. Thus, information has temporary effects on perceptions but durable effects on actual behavior.

A further related literature studies how information about social connections shapes individual decisions. Coffman et al. (2017) and Duflo and Saez (2003) show that social interactions influence beliefs and behavior in multiple domains, and contraceptive choices in particular have been found to be strongly affected by social networks (Behrman et al., 2002; Ashraf et al., 2014). Evidence on whether peer information directly changes behavior, however, is mixed. (Banerjee et al., 2019) show that edutainment-based interventions can shift knowledge and reduce stigma but generate only modest changes in sexual behavior. In contrast, (Chong et al., 2013) report that online sex education in Colombian schools increased knowledge and condom demand, with additional spillover effects on untreated peers. My paper contributes to this debate by testing whether correcting misperceptions about peers' views affects condom demand. I find that while young adults update their perceptions when told that peers hold more favorable views, this belief updating does not increase actual condom take-up.

Finally, this paper relates to the literature on social norms in economics, which emphasizes how cultural values and stigma shape economic outcomes (Bénabou and Tirole, 2011; Bursztyn and Yang, 2021; Yang et al., 2021). Bursztyn et al. (2020) show that stigma associated with conformity can constrain labor supply decisions. I extend this literature by documenting the existence of stigma in condom acquisition in Ghana. The results indicate that discretion in packaging can reduce social image concerns and modestly increase condom demand.

From a policy perspective, these findings imply that providing accurate information about sexual health risks can generate a lasting increase in condom demand among young adults. Because the intervention is light-touch and inexpensive, it suggests a potentially scalable strategy for public health authorities seeking to reduce risky sexual behavior in high-prevalence settings.

The remainder of this paper proceeds as follows. In section 2, I present some background on contraceptive use and evidence regarding the social appropriateness of condom use and purchase in Ghana. In section 3, I discuss the experimental design of the main and endline surveys. Next, in section 4, I present and discuss the results from the experiment. Section 5 concludes the paper.

2 Background: Contraception and Views About Usage in Ghana

2.1 Contraception Knowledge and Use in Ghana

Knowledge of contraceptive methods in Ghana is nearly universal among women and men of reproductive age, as has been consistently documented in recent rounds of the Ghana Demographic and Health Survey (GDHS). Use of modern contraceptive measures by

married women has risen over time but remains modest (about 28% in 2022), while uptake is substantially higher among sexually active unmarried women (overall contraceptive prevalence 63%, with modern methods 46%) (Ghana Statistical Service (GSS) and ICF, 2024).

Yet, this broad awareness has not translated into consistent use, particularly for unmarried people. Barriers to use are not purely economic. Qualitative and quantitative studies in Ghana show that stigma around premarital sex and contraceptive use, including fears of being seen acquiring condoms or contraceptive pills shapes adolescent and young women's choices (Hall et al., 2018; Takyi et al., 2006). Religious and cultural norms have also been shown to influence contraceptive behavior among young adults. (Gyimah et al., 2012) uses GDHS data to identify systematic differences in adoption and method mix by religious affiliation, pointing to social opposition and perceived side effects as salient constraints alongside access.

Information environments further reinforce these demand-side constraints. Debates over comprehensive sexuality education (CSE) have contributed to uneven provision of accurate, youth-appropriate information. Ghana's 2019 CSE guidelines faced public push-back and were not implemented nationwide, and recent assessments highlight the continued prominence of abstinence-only framings among influential actors (UNESCO Global Education Monitoring Report (GEM Report), 2023). Together, these dynamics can weaken demand for modern contraception, especially among the sexually active unmarried youth.

On the supply side, Ghana's public health system remains the backbone for clinical family planning, complemented by a sizable private retail sector (pharmacies and drug shops), especially for condoms, pills, and emergency contraception. Earlier GDHS-based analyses reports roughly one-third of modern method users obtaining their contraceptives from private sources, with condoms and pills overwhelmingly sourced privately (Ghana Statistical Service (GSS) and ICF, 2024).

Policy initiatives over the last decade have sought to expand youth-friendly services and reduce financial barriers. Notably, family planning services were added to the National Health Insurance Scheme (NHIS) benefits package starting in 2022, with ongoing efforts to operationalize and scale coverage. Implementation is progressing but is uneven across districts, and disparities by residence, age, and socioeconomic status persist.

Ghana's combination of almost universal knowledge, persistent demand-side frictions, and a robust retail market for short-acting methods of contraceptives makes it an ideal setting for testing whether the salience of stigma and information about health risks affects condom acquisition. Moreover, an active public family-planning system and ongoing youth-friendly initiatives provide credible partners and realistic pathways to scale if such interventions prove effective.

2.2 Pre-experimental Evidence Regarding the Social Appropriateness of Condoms

To document prevailing beliefs and misperceptions, I conducted an in-person supplementary survey with 381 undergraduate students at the Kwame Nkrumah University of Science

and Technology (KNUST) in Kumasi, Ghana. The students were on average 21 years old, predominantly male, and mostly had no children. 6 out of every 10 respondents at the time of the survey currently had or had had a boyfriend/girlfriend before.

Respondents were presented a set of statements about condom use and purchase and asked both to report their own views and to estimate the degree of agreement of their peers who were present during the survey. The statements included: whether unmarried people who buy condoms generally have multiple sexual partners; whether condom use is immoral; whether condom use is emasculating; whether contraceptive use is against cultural and/or religious values; whether women who buy condoms are likely to be unfaithful; whether contraceptives are only for women of child-bearing age; whether women are solely responsible for contraceptive decisions; whether contraceptive use increases births of deformed babies; whether contraceptive use causes infertility; and whether contraceptives are appropriate only for married couples.

Table 1 shows the gap between respondent's own beliefs and what they perceive that their peers think. Across most statements, respondents overestimated the extent to which their peers held negative views. For example, while 42% percent of respondents agreed that unmarried people who buy condoms are likely to have multiple sexual partners, they estimated that 53% percent of their peers agreed with this statement. The statement "Condom use is emasculating" produces the largest perception gap of 13% (p-value$<$ 0.00), followed by the statement "Contraceptives are only for women of child-bearing age". Interestingly, respondents inaccurately think that contraceptive use can cause infertility (64% of respondents agree to this statement), while the second order belief about this statement is underestimated (51%).

These differences, though not always large, suggest that respondents systematically overestimate the negativity of peers' attitudes toward condom purchase and use. This overestimation may reflect a fear of social rejection, which can lead to exaggerated perceptions of stigma if individuals believe that expressing positive or neutral views about condoms will attract negative social consequences.

For the purpose of the main survey, I present the responses to the statement "Unmarried people who buy condoms have multiple sexual partners" as the peer view information. This statement is of policy relevance as it associates condom purchase with risky sexual behavior. Moreover, condom purchases are observable and readily measurable, making it more feasible to estimate in an experimental setting.

3 Experimental Design

This section describes the structure of the main experiment. I begin by outlining the study population and sampling strategy, followed by a discussion of the randomization procedures used to assign participants to treatment arms. I then present the details of the information intervention itself, including the timing, delivery, and content of the treatments. Finally, I describe the primary and secondary outcomes of interest and how they are measured.

Table 1: Degree of Misperceptions Measured in the Supplementary Survey

Statement	Perceived Mean	Actual Mean	Misperception (p-value)	N
Unmarried people who buy condoms have multiple partners	53.00	41.73	11.270*** (0.00)	381
People who buy condoms are immoral	45.62	37.53	8.090*** (0.00)	381
Condom use is emasculating	35.22	22.05	13.170*** (0.00)	381
Contraceptive use is against religious/cultural values	50.93	57.48	-6.550*** (0.00)	381
Women who buy condoms are likely to be unfaithful	49.15	39.63	9.520*** (0.00)	381
Contraceptives are only for women of child-bearing age	30.54	18.11	12.430*** (0.00)	381
Women are soley responsible for decisions about contraceptives	37.49	31.76	5.730*** (0.00)	381
Contraceptive use increases births of deformed babies	40.64	36.75	3.890** (0.01)	381
Contraceptive use can cause infertility	50.66	64.04	-13.380*** (0.00)	381
Contraceptives are suitable for married couples only	33.76	29.13	4.630*** (0.00)	381

Note: Supplementary Survey data. Sample restricted to attentive respondents. Column (2) reports respondents' guesses about the percentage of their peers who agree with each statement, and column (3) reports the true mean percentage of respondents who agree with the statement. Column (4) shows the difference between the stated perceptions and the actual means. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

3.1 Sample and Randomization

The experiment was conducted among undergraduate students at the Kwame Nkrumah University of Science and Technology (KNUST) in Ghana. The survey was administered online via personalized Qualtrics links and was restricted to students who had not participated in the earlier supplementary survey. In anticipation that lower-year students may have more inaccurate beliefs about their peers' views, and consequently be at higher risk, I deliberately over-sampled first and second year students. This choice was also motivated by the expectation of higher attrition rates among upper-year students in potential follow-up rounds. To ensure relevance of the study questions, participation was limited to unmarried students, who are more likely to face the any barriers associated with condom use.

Respondents were randomly assigned at the individual level to one of three treatment groups or to a control group. A total of 2,151 students completed the baseline survey. To ensure high quality responses were provided in the main survey, an attention check was embedded in the questionnaire. Respondents were asked to indicate "Extremely important" in response to the question "How important is shopping to you?", a simple instruction that did not require any substantive knowledge. Failure to comply is interpreted as an indicator of inattention. Of the total baseline sample, 1,618 respondents (75%) passed this attention check.

Consistent with this classification, there are clear differences in survey completion time between the attentive and inattentive respondents. While inattentive respondents have a slightly higher average completion time (about 54 minutes compared to 48 minutes

among attentive respondents), this difference is driven by a few outliers who likely left the survey open for unusually long periods. Appendix Figure A1 shows that the distribution of completion times for inattentive respondents is in fact shifted toward shorter durations, with a higher concentration of very fast completions under roughly 15 minutes. This pattern supports the interpretation that these respondents were less engaged with the survey tasks. As a result, for the main analysis, I focus on the attentive sample, but I also replicate the results for the full sample in Appendix B. The treatment effects are qualitatively similar, though attenuated in the full sample, consistent with the view that inattentive respondents did not carefully process the survey questions. Reassuringly, randomization remains valid, with no systematic differences in observable characteristics across treatment arms.

Table 2 reports balance checks for the attentive sample. On average, respondents are 22 years old and have no children. They also tend to strongly identify with a religion. One in two of all participants currently have (or have ever had) a boy/girlfriend of more than one year. The majority of participants are unemployed and depend on their families for their living expenses. Treated and control respondents are similar across observable characteristics, with no significant differences except for employment status and region of residence. A joint orthogonality test confirms overall balance, failing to reject the null hypothesis of equality across groups ($p = 0.59$).

3.2 Details of the Experimental Design

The main experiment consists of a baseline survey, a midline voucher distribution, and an endline survey.

At baseline, respondent's basic demographic information was collected, followed by a series of belief elicitation tasks. Afterward, they were randomly assigned to a treatment or control group, and the corresponding interventions were delivered. To reduce social desirability bias, the survey was framed alongside unrelated statements so as not to reveal its central focus. Each respondent was assigned a unique identification number to link survey responses across rounds. At the conclusion of the baseline survey, participants received a show-up fee of 20 cedis (approximately USD 1.49) plus any bonus earnings from belief elicitation tasks.

Approximately three weeks later, a second condom voucher was distributed to all respondents, redeemable at a different campus pharmacy. This midline stage was intended to capture short-run treatment effects on behavior.

At eight weeks, respondents were re-interviewed in an endline survey. In addition to the survey, participants were given the option of receiving condoms delivered to their residence by a student representative. The packaging of the delivery was randomized between transparent and non-transparent bags (see Appendix C).

Across all rounds, condom vouchers and deliveries were framed as part of survey participation logistics rather than as central study outcomes, in order to limit any potential bias among respondents.

Table 2: Summary Statistics for Attentive Sample Only

	(1) All	(2) Risk_Peers(status)	(3) Risk_Peers	(4) Risk	(5) Control	(6) (2) vs. (3)	(7) (3) vs. (4)	(8) (4) vs. (5)
N	1618	412	401	419	386	824	802	838
Age	21.55	21.56	21.64	21.36	21.65	0.72	0.21	0.19
	(3.12)	(2.92)	(3.33)	(2.97)	(3.25)			
Number of Children	0.01	0.00	0.01	0.00	0.01	0.02	0.23	0.36
	(0.08)	(0.00)	(0.11)	(0.07)	(0.10)			
Prior Belief About Peers' View (%)	47.52	46.10	47.46	49.02	47.47	0.47	0.40	0.40
	(26.74)	(27.72)	(26.58)	(26.57)	(26.03)			
Identifies with a Particular Religion (%)	94.44	93.45	94.76	95.47	94.04	0.43	0.64	0.36
Encountered Ethnic Biases Related to Family Planning (%)	20.64	21.60	19.45	21.72	19.69	0.45	0.42	0.48
Undergraduate Student(%)	96.54	97.33	96.76	95.94	96.11	0.63	0.53	0.90
Unemployed(%)	89.43	88.35	87.28	93.56	88.34	0.64	0.00	0.01
Ever had a Boy/Girlfriend(%)	64.52	63.35	65.09	63.72	66.06	0.61	0.68	0.49
Identify Prestige Characteristics(%)	83.50	81.07	84.04	85.20	83.68	0.27	0.64	0.55
Received Sexual/Reproductive Health Edu. in the Last 90 Days (%)	37.21	37.86	41.90	35.80	33.16	0.24	0.07	0.43
Household Income								
Up to 2,000 Cedis	64.22	67.72	64.09	61.58	63.47	0.28	0.46	0.58
2,000 - 4,999 Cedis	21.94	19.17	22.69	22.43	23.58	0.22	0.93	0.70
5,000 - 7,999 Cedis	7.66	7.52	6.98	8.83	7.25	0.77	0.33	0.41
8,000 - 11,999 Cedis	3.03	3.16	3.49	2.86	2.59	0.79	0.61	0.81
Above 12,000 Cedis	3.15	2.43	2.74	4.30	3.11	0.78	0.23	0.37
Ethnicity								
Akan	64.96	61.65	68.08	67.54	62.44	0.06	0.87	0.13
Ewe	13.97	15.53	12.97	13.60	13.73	0.30	0.79	0.96
Ga-Adangbe	8.03	8.25	7.23	7.16	9.59	0.59	0.97	0.21
Mole-Dagbani	9.58	11.17	8.73	8.11	10.36	0.25	0.75	0.27
Guan	2.22	2.43	1.75	1.67	3.11	0.50	0.93	0.18
Gurma	1.24	0.97	1.25	1.91	0.78	0.71	0.45	0.17
Region of Residence								
Western	6.06	6.55	7.73	4.53	5.44	0.51	0.06	0.56
Central	6.86	7.77	5.99	5.73	8.03	0.32	0.88	0.20
Greater Accra	33.56	34.71	34.66	35.32	29.27	0.99	0.84	0.07
Volta Region	2.16	2.18	2.00	1.67	2.85	0.85	0.73	0.26
Eastern	7.48	7.28	6.98	6.92	8.81	0.87	0.97	0.32
Ashanti	32.51	29.37	33.42	33.65	33.68	0.21	0.94	0.99
Brong-Ahafo	3.58	3.16	3.74	4.30	3.11	0.65	0.69	0.37
Northern	1.85	2.18	0.75	1.91	2.59	0.09	0.15	0.51
Upper East	0.87	0.73	0.50	0.95	1.30	0.68	0.44	0.65
Upper West	0.87	1.21	0.50	0.72	1.04	0.27	0.69	0.63
Bono East	1.17	1.46	1.50	1.43	0.26	0.96	0.94	0.07
Ahafo	0.93	0.49	1.25	1.19	0.78	0.24	0.94	0.55
Savannah	0.68	0.97	0.25	0.48	1.04	0.19	0.59	0.36
North East	0.43	0.73	0.25	0.48	0.26	0.33	0.59	0.61
Oti	0.31	0.49	0.00	0.00	0.78	0.16	.	0.07
Western North	0.68	0.73	0.50	0.72	0.78	0.68	0.69	0.92
Test of Joint Significance of all Variables								
P-value								0.59

Note: Baseline data. Sample restricted to attentive respondents. Treatment is a dummy equal to 1 if the respondent received the intervention at baseline. The F-test p-value is the joint significance of the differences between the treatments and control group for all of the variables reported in the table.

3.3 Experimental Treatment Arms

Treatment 1: Risk Treatment Group (*Risk*). A total of 419 students were randomly assigned to receive targeted health information outlining the consequences of unprotected sex. The intervention was motivated by the hypothesis that relevant health risk information can shift perceptions of risk and influence behavior. The information, adapted from official statistics compiled by the Ghana AIDS Commission, emphasized three key points: (i) more than 340,000 people in Ghana are currently living with HIV/AIDS, with the highest prevalence among individuals aged 15–24; (ii) 11,705 new HIV infections were projected for 2023; and (iii) in 2022, unprotected sex resulted in an estimated 21 abortions per day, most frequently among women aged 20–24. The information concluded by highlighting the protective role of condoms in mitigating these risks. Comparing outcomes for participants in this group to those in the control group identifies the causal effect of receiving the health risk information.

Treatment 2: Risk and Peer View Treatment Group (*Risk_Peers*). 401 students were

assigned to this group. The treatment tested whether health risk information is more effective when combined with information about peers' views toward condom purchases and use. Participants in this group first received the identical health risk information used in the $Risk$ arm, followed immediately by the results of the supplementary survey of 201 KNUST students. This peer view information reported that 65% of students surveyed earlier did not agree with the statement that people who buy condoms have multiple sexual partners, while 35% agreed.

The peer view information was intended to correct inaccurate perceptions of prevailing norms, which may otherwise reinforce stigma and discourage condom purchase. Comparing the $Risk\_Peers$ group to the *Risk* group isolates the marginal effect of receiving the additional peer view information alongside health risk information.

Treatment 3: Risk and Peer View With Status Treatment Group ($Risk\_Peers(status)$). This treatment arm, comprised of 412 students, extended the $Risk\_Peers$ design by adding an explicit signal about the prestige of the peers whose views were reported. As in the $Risk\_Peers$ group, participants were presented with both the health risk information and the peer view information. However, in this arm, the message specified that the peers surveyed possessed prestigious characteristics. Specifically, the information stated that these peers were "elite" KNUST students, medical students, social media or campus influencers, top academic performers in their departments, student leaders, or confident public speakers.[1] The emphasis on prestige was designed to test whether social status cues amplify the influence of peer view information.

In principle, high-status peers may be perceived as more credible or as opinion leaders whose behaviors and views are more likely to be emulated. By comparing outcomes between the $Risk\_Peers(status)$ and $Risk\_Peers$ groups, the analysis isolates the incremental effect of associating peer views with prestige signals while holding the content of the peer views constant.

Control Group. A total of 386 students were randomly assigned to receive no informational intervention at the baseline survey. This group provides a benchmark for assessing the impact of the treatments, as any differences in outcomes between the control group and the treated groups can be attributed to the informational content of the interventions, given random assignment.

Appendix C reproduces the full text and presentation of the information treatments.

3.4 Outcomes

I focus on two outcomes: demand for condoms and perceptions of condom. The main measure of condom demand is voucher redemption, which serves as a behavioral proxy. To complement this measure, I use self-reported interest in condom vouchers and self-reported sexual behavior data collected at the endline survey. The secondary outcome is

[1]To avoid deceiving participants, only the responses from students who actually identified with these characteristics were presented. This reduced the supplementary sample from 381 to 201.

reported perceptions toward condoms, measured using survey questions on attitudes and knowledge about contraceptives in general. For all outcomes, I use data from both survey rounds.

Condom Demand. I measure condom demand primarily by using data on voucher redemption. After the baseline survey and the midline, each respondent was offered a date-stamped condom voucher, redeemable only by the original recipient, for a pack of three condoms at a designated campus pharmacy. Retail prices per pack of male condoms in Ghana vary from one brand to another, ranging between GHS 3.00 and GHS 25.00 (roughly 15–30 percent of the daily wage rate). Each voucher was valid for an average of 21 days. The pharmacy maintained a database linking voucher serial numbers to redemption status, allowing us to determine whether each voucher was redeemed.

The pharmacy originally engaged for condom redemption after the baseline survey was replaced at midline due to logistical constraints. The second pharmacy, however, operated shorter opening hours, which substantially reduced accessibility for all respondents. Because this limitation applied equally across treatment arms, it is unlikely to bias the estimated treatment effects, though it did lower overall redemption rates and introduce additional noise that complicates interpretation. For this reason, the main analysis focuses on baseline and endline redemption outcomes, with midline results only reported for completeness.

At the endline, condoms were delivered directly to respondents by student representatives, who recorded redemption data on spreadsheets. Because the condom packaging was randomized at this stage, this variation provides a direct test of stigma-related demand attached to the visible act of obtaining condoms. If stigma is a binding constraint, respondents offered condoms in a non-transparent package should be more likely to accept delivery than those offered the transparent package.

Moreover, because vouchers were distributed repeatedly, I can compare redemption outcomes across survey rounds to estimate the persistence of treatment effects over time. In addition to voucher redemption data, I also collected self-reported interest in condom vouchers from the survey rounds. In each round, after respondents were informed of their eligibility to receive condoms, they were asked whether they would be willing to accept the voucher. This measure complements the behavioral redemption data by capturing stated condom take-up intentions, which may differ due to stigma, privacy concerns, or other unobserved constraints.

Self-Reported Sexual Behavior. Because condom voucher redemption is an imperfect proxy for sexual health behavior, I complement it with self-reported sexual behavior data collected in the endline survey. These measures capture four specific behaviors: (i) whether the respondent reports having been sexually active in the past 90 days; (ii) whether the respondent had multiple sexual partners in the past 90 days; (iii) whether a condom was used during the most recent sexual encounter; and (iv) whether a contraceptive was used in the past 90 days. In addition, I construct a composite index of condom use over the past 90 days by averaging the responses to measures (i) and (iii) above.

While these data offer valuable behavioral indicators, they are dependent on the mag-

nitude of actual take-up and subject to limitations of self-reports, including recall error and social desirability bias. In this context, such biases could lead to under-reporting of risky behaviors or over-reporting of protective behaviors. Nevertheless, the self-reported measures serve as an important complement to the behavioral voucher redemption data, helping to validate and contextualize the objective outcome measure.

Perceptions of Condoms. The secondary outcome captures respondents' perceptions of condoms, measured through survey questions on attitudes and knowledge. Attitude measures assess the perceived appropriateness, morality, and social acceptability of condom use. Respondents were asked to indicate their agreement based on a scale from 0 (highly disagree) to 10 (highly agree), with the following statements:

- Placing condom dispensers in each residence hall will not increase the number of sexual partners students have;
- Willingness to sign a petition for condom dispensers to be placed in each residence hall;
- It is moral to use condoms;
- It is moral to use contraceptive pills;
- It is not shameful or embarrassing to buy condoms; and
- contraceptive use is not a sign of lack of self-control.

Knowledge measures assess factual understanding of contraceptive safety and effectiveness. Respondents rated their agreement (0–10 scale) with the statement:

- condoms are safe and have no negative effects.

This knowledge item provides an objective check on whether the treatments improved accurate beliefs about condoms. All perception measures were collected twice, immediately after the delivery of the treatment and again at the endline survey, allowing me to estimate how persistent the effects of the interventions are on attitudes and knowledge.

Figure 1 provides an overview of the experimental timeline, indicating the sequence of baseline data collection, intervention delivery, and the endline survey.

3.5 Preregistration

This study was approved by the CERGE-EI Ethics Committee. The experiment and analysis were preregistered on the AEA RCT registry. All experimental procedures, including detailed instructions, can be found at https://doi.org/10.1257/rct.11378-2.0.

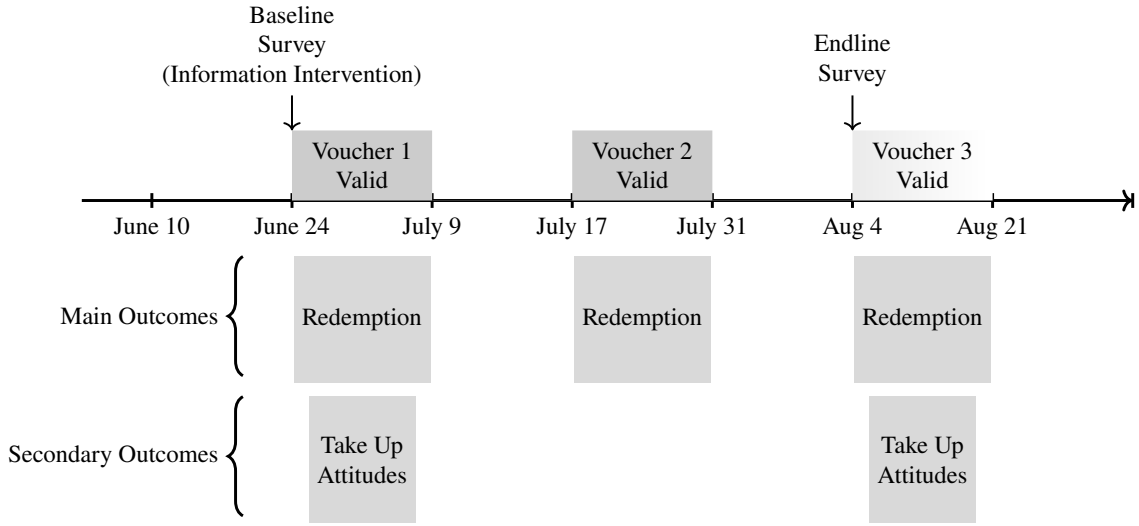

Figure 1: Experimental Timeline

4 Results

4.1 Effect of the Information Interventions on Condom Demand

In the following sections, I present results from the main experiment using both the linear probability model with the pre-registered set of controls and the double selection LASSO linear regression for each empirical specification. I first compare respondents assigned to any treatment to those in the control group, before turning to estimates disaggregated by treatment arm.

4.1.1 Voucher Redemption : Actual Condom Take-up and Condom Take-up Intentions

Receiving any form of information significantly increases actual condom take-up, with effects that persist over time. Figure 2 compares information effects immediately after the baseline survey and two months after the information intervention. Treated respondents are 4 percentage points more likely than controls to redeem their vouchers for condoms at baseline ($p = 0.043$), and this effect grows to 7 percentage points two months later ($p = 0.012$), indicating a sustained increase in demand.

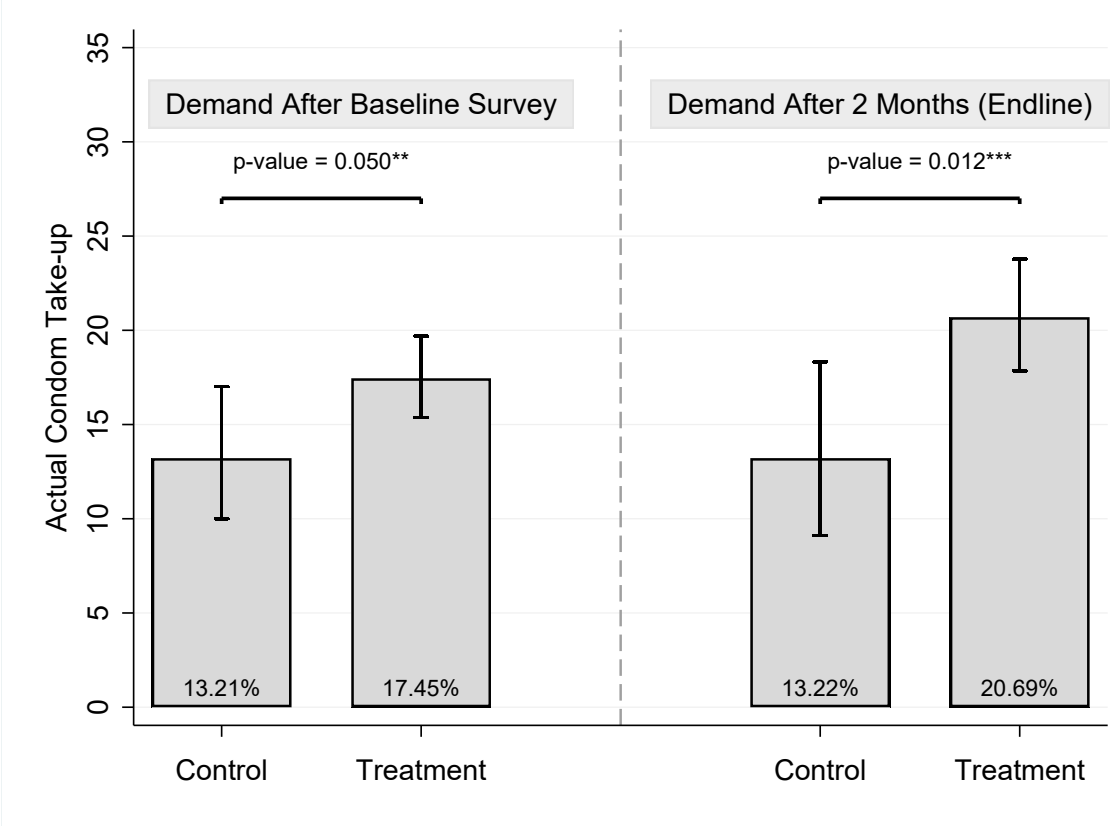

Figure 2: Effect of receiving information on condom demand. The left panel illustrates the estimated effect of receiving either type of treatment on actual condom take-up after expiry of the first voucher, while the right panel shows the effect of receiving either type of treatment on actual condom take-up after expiry of the third voucher.

Disaggregating the results by treatment arm reveals that differences across information types are small and statistically insignificant. In Figure 3, immediately after the baseline survey, the *Risk* treatment, which only provided health risk information increased actual condom take-up by 4.2 percentage points, though the effect is not statistically significant. The *Risk_Peers* treatment, which combined risk information with information on peer views, produced the largest effect, a 5.0 percentage point increase ($p < 0.1$), while the *Risk_Peers*(*status*) treatment, which further emphasized the prestige of peers, led to a 3.5 percentage point increase that is not statistically significant. Pairwise comparisons confirm that neither peer information nor peer status significantly amplifies the effect of risk information ($p = 0.77$ and $p = 0.58$, respectively).

Two months later, the disaggregated endline effects show a similar pattern. Condom demand remains higher among all treatment groups relative to the control. The *Risk* treatment yields a 6.3 percentage point increase (not statistically significant), while the *Risk_Peers* treatment generates the largest and most precisely estimated effect, an increase of 10.1 percentage points (p < 0.01). The *Risk_Peers*(*status*) treatment also produces a positive and statistically significant increase of 6.1 percentage points (p < 0.1). However, pairwise comparison tests again detect no incremental effects between treatments (p = 0.230 for *Risk_Peers* vs. *Risk*; p = 0.369 for *Risk_Peers*(*status*) vs. *Risk_Peers*).

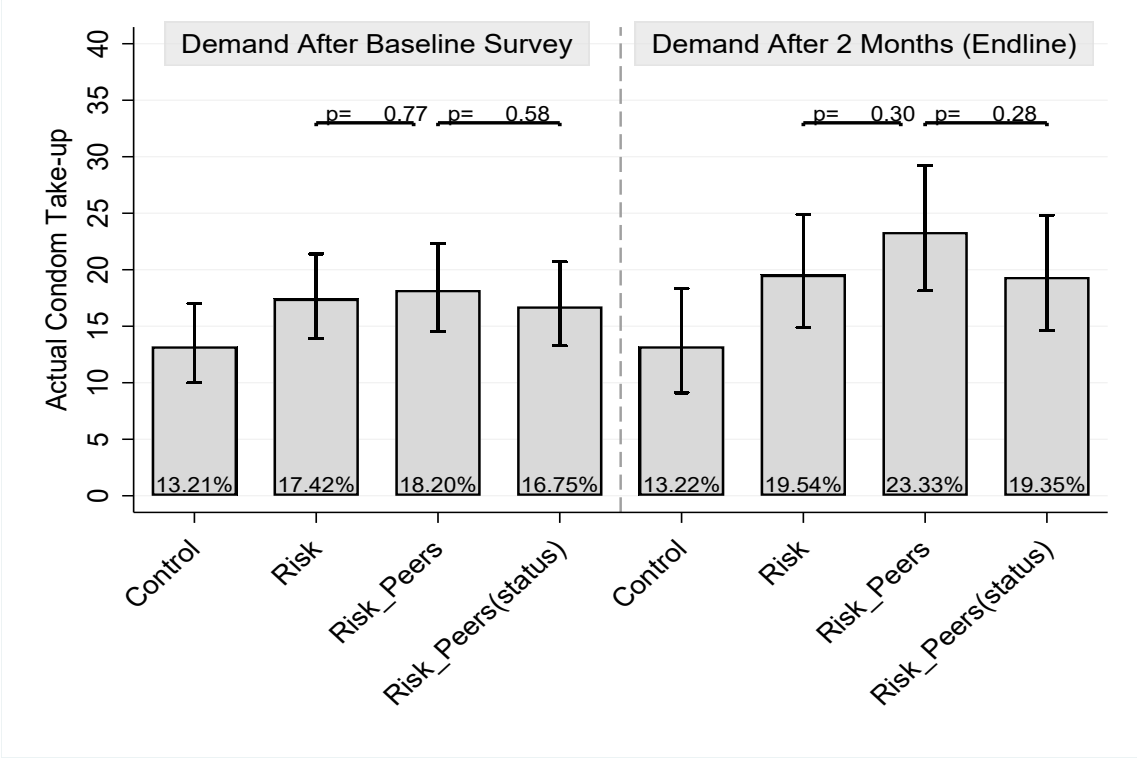

Figure 3: Effect of receiving information on condom demand. The left figure illustrates the estimated effects of receiving a specific type of treatment on actual condom take-up after expiry of the first voucher, and the right figure shows the effect of receiving a specific type of treatment on actual condom take-up after expiry of the third voucher.

Patterns in condom take-up intentions mirror these results. Immediately after the intervention, treated respondents are a significant 5.5 percentage points more likely than control respondents to express interest in redeeming a voucher (see Appendix Figure A2) and the effects are also persistent. Across treatment arms, the $Risk$, $Risk\_Peers$, and $Risk\_Peers(status)$ groups each show intention effects of similar magnitude, between 3 and 8 percentage points, with marginal to no statistically significant differences among them (see Appendix Figure A3). These consistent patterns between stated intentions and actual behavior reinforce the conclusion that information treatments meaningfully increased both willingness and follow through in condom acquisition. Table 3 reports the corresponding regression estimates.

Table 3: Effects of Receiving Information on Behavior

	Actual Take-up			Take-up Intentions		
	(1) Main Survey	(2) Follow Up	(3) Endline Survey	(4) Main Survey	(5) Follow Up	(6) Endline Survey
Panel A: Pooled Treatment						
Linear probability model with pre-registered set of controls						
Any Treatment	0.0411**	-0.0242*	0.0713***	0.0535*	0.0342	0.0655*
	(0.0203)	(0.0146)	(0.0268)	(0.0275)	(0.0332)	(0.0371)
R^2	0.054	0.039	0.065	0.101	0.086	0.092
Double-selection LASSO linear regression						
Any Treatment	0.0419**	-0.0241	0.0759***	0.0546**	0.0544	0.0767**
	(0.0202)	(0.0152)	(0.0266)	(0.0272)	(0.0331)	(0.0370)
PANEL B: Specific Treatment						
Linear probability model with pre-registered set of controls						
Risk_Peers(status)	0.0382	-0.0244	0.0646*	0.0559*	0.0455	0.0792*
	(0.0250)	(0.0168)	(0.0340)	(0.0332)	(0.0395)	(0.0444)
Risk_Peers	0.0501*	-0.0237	0.0979***	0.0267	0.0132	0.0538
	(0.0259)	(0.0167)	(0.0351)	(0.0336)	(0.0403)	(0.0447)
Risk	0.0354	-0.0243	0.0534	0.0768**	0.0422	0.0633
	(0.0254)	(0.0171)	(0.0332)	(0.0331)	(0.0400)	(0.0439)
R^2	0.054	0.039	0.066	0.102	0.086	0.093
Double-selection LASSO linear regression						
Risk_Peers(status)	0.0384	-0.0228	0.0688**	0.0566*	0.0570	0.0893**
	(0.0249)	(0.0170)	(0.0335)	(0.0331)	(0.0393)	(0.0437)
Risk_Peers	0.0529**	-0.0243	0.105***	0.0285	0.0249	0.0602
	(0.0257)	(0.0175)	(0.0349)	(0.0333)	(0.0403)	(0.0442)
Risk	0.0351	-0.0222	0.0516	0.0795**	0.0644	0.0675
	(0.0251)	(0.0172)	(0.0334)	(0.0329)	(0.0402)	(0.0436)
Control mean	0.580	0.580	0.577	0.132	0.057	0.132
N	1615	1197	975	1615	1197	975
Test:						
Any Treament= Control	0.041	-0.024	0.071	0.053	0.034	0.066
P-value	(0.043)	(0.097)	(0.008)	(0.052)	(0.304)	(0.078)
Risk_Peers=Risk	0.015	0.001	0.045	-0.050	-0.029	-0.009
P-value	(0.581)	(0.967)	(0.230)	(0.124)	(0.451)	(0.822)
Risk_Peers(status)=Risk_Peers	-0.012	-0.001	-0.033	0.029	0.032	0.025
P-value	(0.652)	(0.961)	(0.369)	(0.377)	(0.397)	(0.547)

Data from main, follow-up, and endline surveys. Sample restricted to attentive respondents. Estimates from OLS regression. Standard errors are in brackets. Panel A compares receiving either type of treatment to control, while Panel B compares specific treatments to control. Columns 1–3 are the effect of treatment on take-up intentions, and Columns 4–6 are the effect on actual condom take-up. All regressions include the pre-registered set of controls. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

4.1.2 The Role of Packaging

To further probe the causal impact of information on condom demand, I exploit the random assignment of packaging types in the endline survey. This design allows me to examine whether the observed informational treatment effects are robust to differences in the way condoms were delivered to respondents.

Figure 4 shows that within each packaging condition, the information intervention generates sizable and statistically significant increases in actual condom take-up. Among respondents assigned to transparent packaging, actual condom take-up rises from 10.81 percent in the control group to 18.52 percent in the treatment group, a difference of 7.71 percentage points ($p = 0.03$). A nearly identical pattern emerges in the non-transparent condition, where actual condom take-up increases from 15.52 percent among the control group to 22.91 percent among the treated respondents, corresponding to a 7.39 percentage point increase ($p = 0.04$). These results confirm that the information intervention had a

causal impact on demand across both packaging types.

At the same time, the packaging condition itself appears to influence overall actual condom take-up levels, though the differences conditional on initial information treatment assignment are not statistically significant. For all control respondents, actual condom take-up is 4.71 percentage points higher when condoms are delivered in a non-transparent package (15.52 vs. 10.81 percent, $p = 0.30$). A similar statistically insignificant pattern is observed among the treated group: actual condom take-up rates are 22.91 percent under non-transparent packaging versus 18.52 percent under transparent packaging, a difference of 4.39 percentage points ($p = 0.14$)

These results point to two main observations. First, the informational intervention appears to have a robust effect on condom demand. The magnitude of the health risk information treatment effect is comparable across both transparent and non-transparent packaging conditions, and in each case, the rise in actual condom take-up is statistically significant. This consistency across packaging types suggests that the observed increase in demand is primarily driven by the health risk information intervention rather than by packaging differences.

Second, although condom take-up rates are generally higher under non-transparent packaging, the within-group differences (transparent vs. non-transparent among the treated and then transparent vs. non-transparent among control) are not statistically significant, indicating that the estimates are imprecise. However, when pooling across packaging assignment without considering baseline information treatment assignment, individuals exposed to the non-transparent packaging are about 4 percentage points more likely to redeem condom vouchers than those assigned to transparent packaging (p = 0.08; see Appendix Figure A4). This marginal difference points to a potential role of packaging in influencing demand.

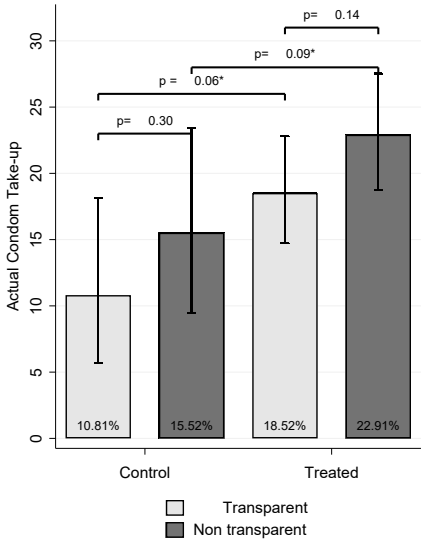

Figure 4: Effect of receiving information on condom demand by packaging assignment. The figure illustrates the effect on actual condom take-up at the endline by packaging type assignments.

Heterogeneity: I observe a consistent and systematic heterogeneity in estimated treatment effects by past sexual activity (defined as having had a boy/girlfriend for at least six months).

Reassuringly, I find that the positive effect on condom demand is concentrated among those who are likely sexually active at baseline, while no systematic effect is observed among those who are likely not sexually active (see Figure 5). After the baseline survey, among respondents categorized as not sexually active (N=894), there is no statistical difference in actual condom take-up among treated and non-treated respondents. In contrast, among the sexually active participants (N=721), the information intervention leads to an 64 percent increase ($p = 0.02$) in actual condom take-up. After the endline survey, the corresponding effects increase even further, by 89 percent for sexually active participants ($p < 0.01$, control mean=14.2, N=452).

Among non–sexually active participants, actual take-up after the endline increases by 3.4 percentage points for the treated, and is not statistically different at conventional levels (p=0.37, control mean=12.3, N=523). This pattern is consistent with the interpretation that the intervention primarily affected the behavior of those currently in a position to use condoms.

Panel B of Table 4 shows the disaggregated results by specific treatment assignment that reinforce this interpretation. After the baseline survey, among non–sexually active participants, the estimated effects are small (−1.7, 3.6, and 2.8 percentage points), and insignificant in all treatment arms. After the endline survey, the $Risk\_Peers(status)$ leads to a statistically significant marginal effect size of 8.1 percentage points. However, the significance disappears with the LASSO regression. In contrast, among the sexually active participants, the results are stronger. After the baseline survey, both the $Risk\_Peers(status)$ and $Risk\_Peers$ increase actual condom take-up rates by 11.5 and 7.3 percentage points, respectively. After the endline survey, actual condom take-up rates rises by 7.8 percentage points in the $Risk\_Peers(status)$ arm, 19.4 percentage points in the $Risk\_Peers$ arm ($p < 0.01$), and 10.4 points in the Risk arm ($p < 0.10$).

Taken together, these findings suggest that treatment effects are concentrated among individuals who are the most likely to be sexually active. Nevertheless, the analysis of heterogeneous effects should be treated as tentative, as the differences in coefficients are not always statistically significant, and no adjustments are made for multiple hypothesis testing.

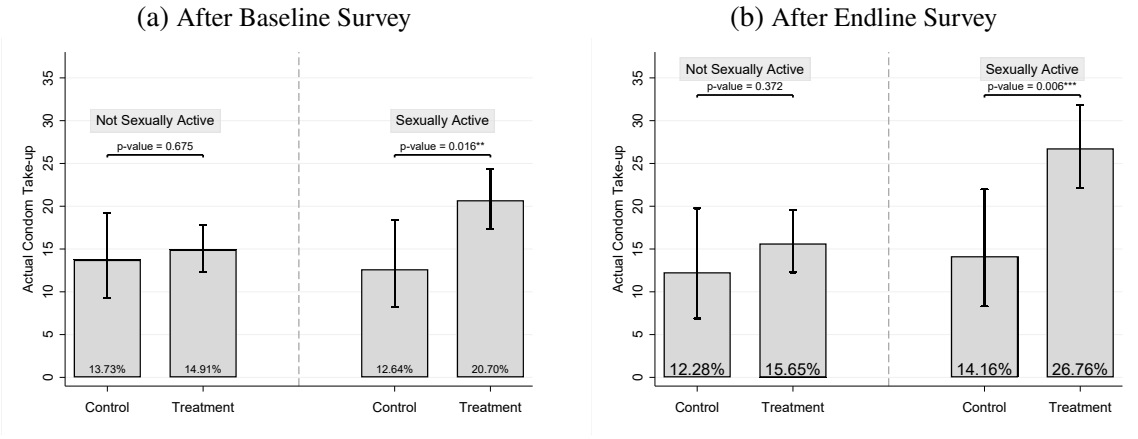

Figure 5: Effect of receiving information on actual condom take-up: heterogeneity by past sexual activity status

Table 4: Effect of Receiving Information on Actual Condom Take-up: Heterogeneity by Past Sexual Activity Status

	After Main Survey		After Endline Survey	
	Not Sexually Active (1)	Sexually Active (2)	Not Sexually Active (3)	Sexually Active (4)
PANEL A: Pooled Treatment				
LPM with pre-registered controls				
Any Kind of Treatment	0.0148	0.0741**	0.0422	0.125***
	(0.0278)	(0.0301)	(0.0371)	(0.0422)
R^2	0.049	0.080	0.063	0.122
Comparison chi-sq (p-value)	2.181(0.140)		2.332(0.127)	
Double-select. LASSO regression				
Any Kind of Treatment	0.0124	0.0780***	0.0344	0.120***
	(0.0275)	(0.0302)	(0.0357)	(0.0402)
PANEL B: Specific Treatment				
LPM with pre-registered controls				
Risk_Peers(status)	-0.0174	0.115***	0.0808*	0.0776
	(0.0317)	(0.0410)	(0.0481)	(0.0527)
Risk_Peers	0.0360	0.0726*	0.0211	0.194***
	(0.0353)	(0.0393)	(0.0438)	(0.0566)
Risk	0.0284	0.0397	0.0248	0.104*
	(0.0350)	(0.0379)	(0.0445)	(0.0546)
R^2	0.053	0.084	0.067	0.131
Double-select. LASSO regression				
Risk_Peers(status)	-0.0183	0.114***	0.0641	0.0677
	(0.0315)	(0.0406)	(0.0452)	(0.0512)
Risk_Peers	0.0356	0.0716*	0.0272	0.192***
	(0.0349)	(0.0382)	(0.0443)	(0.0551)
Risk	0.0229	0.0434	0.0138	0.0928*
	(0.0344)	(0.0372)	(0.0427)	(0.0525)
Control Mean	0.137	0.126	0.123	0.142
N	894	721	523	452

Data from main, follow-up, and endline surveys. Sample restricted to attentive respondents. Estimates from OLS regression. Standard errors are in brackets. Panel A compares receiving either type of treatment to control, while Panel B compares specific treatments to control. All regressions include the pre-registered set of controls. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

4.1.3 Self-Reported Sexual Behavior

Table 5 reports the effects of the interventions on self-reported sexual behavior at endline. Columns (1)–(4) consider individual outcomes, including whether the respondent reports having been sexually active in the past 90 days, had multiple partners, used a condom on the most recent occasion, or used any contraceptive method in the past 90 days. Columns (5)–(7) aggregate these into composite indices capturing safer and riskier sex practices including condom use.

Across specifications, the pooled treatment effects are all small and statistically insignificant. Respondents assigned to any treatment were 2 percentage points more likely to report sexual activity (column 1) and 3 percentage points more likely to report condom use at last sex (column 3), relative to the control means of 23 percent and 39 percent, respectively. The effects on multiple partnerships and contraceptive use are near zero, and the indices in Columns (5)–(7) show modest increases of 1-3 percentage points that fall short of conventional significance thresholds.

Panel B disaggregates the pooled estimates by specific treatment arms. Point estimates are consistently positive for condom use at last sex and the safe-sex index, with magnitudes of 2-5 percentage points across the three treatments. The $Risk$ arm generates the largest increase in reported condom use (5 percentage points), while $Risk\_Peers$ produces the largest increase in the risky-sex index (1.5 percentage points). However, none of these differences are statistically distinguishable from zero given the wide standard errors.

The imprecision of these estimates likely reflects several factors. First, these outcomes capture behaviors that are conceptually distinct from condom take-up, measuring actual condom use and sexual activity rather than willingness to obtain condoms. Because overall condom take-up was modest, the analysis may be underpowered to detect meaningful treatment effects, particularly for outcomes that are relatively infrequent. Second, the survey questions are inherently noisy, as they ask about behaviors over the past 90 days, a period that partly predates the information intervention and thus dilutes potential treatment effects. Finally, self-reported sexual behavior is subject to substantial measurement error, as subjects may tend to under-report risky practices and over-report socially desirable ones, biasing coefficients toward zero and inflating standard errors.

All together, these results provide no definitive evidence of behavioral change, but the consistent direction of the estimates across condom use and the safe-sex index is suggestive. When viewed alongside the clear increases in voucher redemption documented earlier, the evidence points toward some behavioral response to the interventions, albeit one that is difficult to measure precisely given the data and outcomes available.

Table 5: Effect of Receiving Information on Self-Reported Sexual Behavior at Endline

	Self Reported Sexual Behavior				Composite Indices of		
	(1) Sexually active last 90 days	(2) Multi. partners last 90 days	(3) Used condom at last sex	(4) Used contracep last 90 days	(5) Used condom last 90 days	(6) Practice safe sex	(7) Practice risky sex
Pooled Treatment							
Any Kind of Treatment	0.0223	0.00787	0.0334	-0.00429	0.0279	0.0171	0.00608
	(0.0245)	(0.0134)	(0.0287)	(0.0185)	(0.0210)	(0.0171)	(0.00967)
R^2	0.048	0.028	0.029	0.038	0.024	0.028	0.033
Double-select. LASSO Regression							
Any Kind of Treatment	0.0218	0.00933	0.0320	-0.00871	0.0264	0.0145	0.00960
	(0.0241)	(0.0132)	(0.0284)	(0.0185)	(0.0207)	(0.0169)	(0.00975)
PANEL B: Specific Treatment							
Risk_Peers(status)	0.0151	0.0128	0.0190	0.000514	0.0170	0.0115	0.00613
	(0.0299)	(0.0170)	(0.0350)	(0.0226)	(0.0257)	(0.0209)	(0.0116)
Risk_Peers	0.0339	0.00911	0.0293	-0.0201	0.0316	0.0144	0.0146
	(0.0304)	(0.0167)	(0.0353)	(0.0217)	(0.0260)	(0.0208)	(0.0117)
Risk	0.0186	0.00176	0.0516	0.00591	0.0351	0.0254	-0.00207
	(0.0297)	(0.0161)	(0.0349)	(0.0226)	(0.0256)	(0.0206)	(0.0121)
R^2	0.048	0.028	0.030	0.039	0.024	0.028	0.034
Double-select. LASSO Regression							
Risk_Peers(status)	0.0149	0.0127	0.0247	-0.00480	0.0182	0.0104	0.00908
	(0.0294)	(0.0167)	(0.0348)	(0.0225)	(0.0254)	(0.0206)	(0.0115)
Risk_Peers	0.0358	0.0124	0.0284	-0.0246	0.0320	0.0132	0.0188
	(0.0300)	(0.0167)	(0.0349)	(0.0217)	(0.0257)	(0.0206)	(0.0116)
Risk	0.0172	0.00256	0.0420	0.00547	0.0278	0.0200	-0.000459
	(0.0295)	(0.0160)	(0.0347)	(0.0227)	(0.0255)	(0.0206)	(0.0122)
Control mean	0.228	0.052	0.394	0.117	0.311	0.246	0.468
N	1615	1615	1615	1615	1615	1615	1615

Data from endline survey. Sample restricted to attentive respondents. Estimates from OLS regression. Standard errors are in brackets. Columns 1–4 show the effect of treatment on self-reported sexual behavior. Column 5 is an index averaging responses to columns 1 and 3, column 6 averages responses to columns 1, 2, and 3, and column 7 averages responses to columns 1 and 2 minus the response to column 4. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Robustness: The persistent effects of health risk information on condom demand are robust across a range of specifications. Appendix Table A1 reports the effect of receiving either type of treatment on actual condom take-up at endline under different sets of control variables, starting with the specification with no controls and with standard errors clustered at the individual level. Across these variations, the estimated treatment effect lies between 7.1 and 7.6 percentage points, and is statistically significant at conventional levels.

Differential attrition is unlikely to account for the results. Sixty percent of the attentive sample from the baseline survey also participated in the endline survey, and there is no systematic evidence of differential attrition across treatment groups by baseline characteristics (see Appendix Table A2). While omnibus tests suggest some subgroup-specific variation, sensitivity analysis using the (2009) Lee bounds confirms the robustness of the findings. This approach assumes monotonic attrition, meaning that treatment assignment influences sample retention in only one direction: either all individuals who would have remained in the control group also remain in the treatment group, or vice versa. Under this assumption, the estimated treatment effects remain within a conservative range of 4.8 to 8.2 percentage points, with both bounds statistically significant at the 5 percent level.

These results are also unlikely to reflect experimenter demand effects. The positive

impacts appear not only in stated take-up intentions but also in actual condom redemption behavior, which involves a tangible and potentially costly action, being publicly observed obtaining condoms. Such behavior is less likely to be driven by a desire to please the experimenter. Moreover, the persistence of these effects over a two-month period is inconsistent with typical demand effects, which tend to be short-lived and fade quickly once the experimenter's influence is removed (Haaland et al., 2023; Bartoš et al., 2022).

Further evidence against an experimenter demand interpretation comes from the observed heterogeneity in treatment effects. The increase in condom demand is concentrated among individuals for whom condoms are most relevant; those who are sexually active and thus stand to benefit directly from safer sex practices. If responses were primarily driven by social desirability or experimenter expectations, effects would likely be more uniform across the sample rather than concentrated among those with genuine need.

Overall, the pooled estimates provide strong evidence that the health risk information, which was common to all the interventions, successfully increased both intentions and actual condom take-up. The treatment specific results show similar magnitudes across the three treatment arms, with no robust evidence that one particular variant outperforms the others.

4.2 Effect of the Information Interventions on Perceptions about Contraceptives

To better understand why the risk information treatment affected behavior, I next explore whether the information led to changes in attitudes and knowledge about condoms and contraceptives in general. I also explore whether the effects on these perceptions were lasting or temporary.

Table 6 reports the effects of the information interventions on perceptions about contraceptives, distinguishing between short-term effects measured immediately after the intervention (columns 1–8) and long-term effects measured two months later (columns 9–14). Columns 2 and 10 capture the knowledge outcome, whether respondents correctly report that condoms are safe, while the remaining columns reflect attitudinal outcomes, including beliefs about condom use, morality, and the social acceptability of contraception use.

Short-Term Effects. In the short run, assignment to any treatment significantly improves perceptions about contraceptives. Treated respondents score 0.21 standard deviations higher on the overall contraceptive attitudes index (column 1, $p < 0.05$). This effect is primarily driven by gains in knowledge and more positive perceptions of condom dispensers. Specifically, treated respondents are 0.44 standard deviations (s.d.) more likely to correctly report that condoms are safe (column 2, $p < 0.05$) and equally more likely to agree that installing condom dispensers on campus will not increase promiscuity (column 3, $p < 0.05$). Other attitudinal outcomes such as moral acceptability, shame, and self-control move in a positive direction but with smaller, magnitudes (ranging from 0.01 to 0.22 standard deviations) and are not precisely estimated (columns 4–8).

Panel B disaggregates effects by specific treatment arms. Both the $Risk\_Peers(status)$

and $Risk\_Peers$ treatments generate large and significant knowledge gains of 0.47 and 0.43 s.d., respectively, as well as positive, though noisier improvements in attitudes toward condom dispensers. The $Risk$ only treatment also produces significant knowledge gains of 0.41 s.d. ($p < 0.05$), though its effects on attitudinal outcomes are smaller and less precisely estimated.

The marginal treatment comparisons in Panel C provide further insights. Providing peer view information ($Risk\_Peers$) substantially improves the perception that condom dispensers do not increase promiscuity by 54 pp ($p = 0.02$) and increases the perception that purchasing condoms is not shameful by 6.3 percentage points ($p = 0.041$). Adding explicit status cues to the peer view treatment ($Risk\_Peers(status)$ vs. $Risk\_Peers$) reverses this pattern, reducing the perception that condom purchase is not shameful by 7.3 pp ($p = 0.02$). Because the status cues combine social and academic dimensions (e.g., influencers and high-GPA students), it is difficult to disentangle which aspect drives these differences.

Long-Term Effects. Two months after the intervention, the effects weaken considerably. The overall contraceptive attitudes index falls to 0.19 s.d. for treated respondents and becomes statistically insignificant. Respondents remain 0.21 s.d. more likely to report that condoms are safe, but the estimate is imprecise. Attitudinal outcomes related to morality, shame, and self-control (columns 11–14) are close to zero and statistically indistinguishable from control means.

Across specific treatment arms, the $Risk$ only treatment maintains a sizable and statistically significant 0.42 s.d. improvement in condom safety knowledge ($p < 0.05$), whereas the knowledge gains for the $Risk\_Peers(status)$ and $Risk\_Peers$ treatments decline and become insignificant. Attitudinal improvements across all three treatments vanish, with coefficients on morality, shame, and self-control near zero and statistically indistinguishable from controls.

Examining marginal effects at the endline highlights the limitation of the peer treatment. Peer view information significantly reduces perceptions across several outcomes: it lowers the overall perception index by 25 s.d. ($p = 0.04$), reduces the probability of reporting that condoms are safe by 33 s.d. ($p = 0.09$), decreases the belief that condoms are morally acceptable by 53 s.d. ($p = 0.057$), and reduces perceptions that contraception in general is morally acceptable by 45 s.d. ($p = 0.098$).

Treatment Effects Measured with Delay. All interaction coefficients are negative and small, suggesting that changes in attitudes are not sustained. For example, the coefficient on $Risk_Peers(status)$x$Time$ for condom safety is -0.36 ($p > 0.1$), and $Risk_Peers$x$Time$ for moral acceptability is -0.64 ($p < 0.10$), indicating partial reversion toward baseline beliefs. No treatment displays a positive or growing effect over time. These findings confirm that the knowledge and attitudinal changes observed immediately after treatment largely dissipate within two months.

In summary, the results show that the interventions generated meaningful short-run improvements in both knowledge and attitudes, with the strongest and most robust effects on knowledge about condom safety. These knowledge effects persist over time, especially

for the *Risk* treatment. In contrast, attitudinal gains appear to be transitory, dissipating by the endline survey. The evidence from the marginal treatment comparisons suggests that the intervention's effectiveness stems primarily from heightened risk salience, rather than from peer influence. Finally, the attenuation of treatment effects over time may partly reflect positive spillovers onto the control group, as control means increase substantially between the baseline and endline surveys.

Table 6: Effect of Receiving Information on Perceptions about Contraceptives

	Short term Effects								Long term Effects					
	(1) Overall Att Contracept	(2) Cond. Safe	(3) Cond. ATMs promiscuity	(4) Petition for Cond. ATMs	(5) Cond. Moral	(6) Contracept. Moral	(7) Cond. purchase not shameful	(8) Contracept. not self contrl	(9) Overall Att Contracept	(10) Cond. Safe	(11) Cond. Moral	(12) Contracept. Moral	(13) Cond. purchase not shameful	(14) Contracept. not self contrl
PANEL A: Pooled Treatment														
LPM with Pre-registered Controls														
Any Kind of Treatment	0.206**	0.439**	0.439**	0.185	0.170	0.223	-0.00659	-0.00771	0.0190	0.207	-0.148	-0.00142	0.00552	0.0319
	(0.0855)	(0.171)	(0.190)	(0.204)	(0.199)	(0.191)	(0.0262)	(0.0264)	(0.104)	(0.181)	(0.229)	(0.229)	(0.0321)	(0.0359)
R^2	0.086	0.034	0.077	0.060	0.050	0.048	0.021	0.045	0.070	0.057	0.067	0.049	0.034	0.047
Double-select. LASSO Regression														
Any Kind of Treatment	0.192**	0.399**	0.407**	0.155	0.155	0.194	-0.00595	-0.00997	0.0372	0.233	-0.131	0.0605	0.00518	0.0308
	(0.0856)	(0.170)	(0.189)	(0.204)	(0.198)	(0.190)	(0.0262)	(0.0265)	(0.103)	(0.178)	(0.227)	(0.225)	(0.0320)	(0.0352)
PANEL B: Specific Treatment														
LPM with Pre-registered Controls														
Risk_Peers(status)	0.254**	0.473**	0.575**	0.136	0.264	0.372	-0.0337	-0.00924	-0.0181	0.0940	-0.120	-0.0868	-0.00467	0.0272
	(0.103)	(0.201)	(0.233)	(0.251)	(0.240)	(0.232)	(0.0325)	(0.0324)	(0.130)	(0.214)	(0.290)	(0.285)	(0.0394)	(0.0431)
Risk_Peers	0.245**	0.432**	0.645***	0.127	0.209	0.248	0.0397	0.0166	-0.0960	0.0867	-0.441	-0.194	0.0376	0.0306
	(0.104)	(0.206)	(0.233)	(0.251)	(0.241)	(0.236)	(0.0312)	(0.0321)	(0.130)	(0.219)	(0.291)	(0.284)	(0.0384)	(0.0435)
Risk	0.119	0.410**	0.104	0.289	0.0369	0.0480	-0.0238	-0.0296	0.157	0.420**	0.0915	0.252	-0.0139	0.0376
	(0.107)	(0.203)	(0.233)	(0.248)	(0.244)	(0.234)	(0.0322)	(0.0321)	(0.121)	(0.210)	(0.265)	(0.268)	(0.0391)	(0.0422)
R^2	0.088	0.034	0.081	0.061	0.050	0.050	0.025	0.046	0.075	0.061	0.070	0.052	0.036	0.047
Double-select. LASSO Regression														
Risk_Peers(status)	0.231**	0.445**	0.540**	0.110	0.241	0.326	-0.0323	-0.0166	-0.00466	0.120	-0.113	-0.0462	-0.000410	0.0280
	(0.103)	(0.201)	(0.231)	(0.249)	(0.242)	(0.233)	(0.0323)	(0.0325)	(0.130)	(0.211)	(0.287)	(0.282)	(0.0392)	(0.0425)
Risk_Peers	0.229**	0.404**	0.618***	0.0920	0.184	0.200	0.0397	0.0129	-0.0848	0.140	-0.461	-0.149	0.0403	0.0339
	(0.105)	(0.205)	(0.230)	(0.251)	(0.239)	(0.235)	(0.0312)	(0.0321)	(0.129)	(0.217)	(0.289)	(0.279)	(0.0382)	(0.0425)
Risk	0.127	0.377*	0.0818	0.253	0.0276	0.0646	-0.0222	-0.0254	0.157	0.419**	0.0902	0.264	-0.0105	0.0309
	(0.106)	(0.201)	(0.231)	(0.248)	(0.242)	(0.232)	(0.0320)	(0.0324)	(0.119)	(0.203)	(0.263)	(0.264)	(0.0391)	(0.0420)
Control mean	4.780	7.554	5.497	6.570	6.870	5.536	0.723	0.710	4.552	8.198	7.304	5.802	0.767	0.687
N	1615	1615	1615	1615	1615	1615	1615	1615	975	975	975	975	975	975
PANEL C: Pairwise Difference Tests														
Any Treatment=Control	0.206	0.439	0.439	0.185	0.170	0.223	-0.007	-0.008	0.019	0.207	-0.148	-0.001	0.006	0.032
P-value	(0.016)	(0.010)	(0.021)	(0.185)	(0.170)	(0.223)	(0.801)	(0.770)	(0.855)	(0.255)	(0.520)	(0.995)	(0.864)	(0.374)
Risk_Peers=Risk	0.126	0.022	0.540	-0.162	0.173	0.200	0.063	0.046	-0.253	-0.333	-0.532	-0.446	0.051	-0.007
P-value	(0.236)	(0.911)	(0.020)	(0.518)	(0.467)	(0.391)	(0.041)	(0.147)	(0.040)	(0.094)	(0.057)	(0.098)	(0.173)	(0.863)
Risk_Peers(status)=Risk_Peers	0.009	0.041	-0.069	0.009	0.054	0.124	-0.073	-0.026	0.078	0.007	0.320	0.107	-0.042	-0.003
P-value	(0.934)	(0.829)	(0.766)	(0.972)	(0.818)	(0.597)	(0.020)	(0.418)	(0.552)	(0.971)	(0.285)	(0.705)	(0.265)	(0.933)

Data from main and endline surveys. Sample restricted to attentive respondents. Standard errors are in brackets. Panel A compares receiving either type of treatment to the control group, while Panel B compares the effect of receiving a specific treatment to the control group. Columns 1–8 present the short-term effects of treatment on perceptions, while columns 9–14 show the long-term effects. Column 1 is an index averaging responses to columns 2–8, and column 9 is an index averaging responses to columns 10–14. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

4.3 Discussion: Why Temporal Shifts in Attitudes Can Have Lasting Impacts on Behavior

These empirical results show that, while the information intervention has positive temporal effects on perceptions, it has lasting positive effects on behavior- actual condom take-up. I interpret these findings as consistent with two distinct mechanisms: learning from information and habit formation, either of which could operate independently or reinforce one another.

One potential mechanism is learning. Health risk information provides durable knowledge about the consequences of risky sex and the protective benefits of condom use, which translates into more favorable attitudes toward condoms. The effects are strongest for attitudes directly related to the intervention, such as beliefs about condom safety and the social acceptability of their use. Although these attitudinal shifts weaken over time, the

initial learning lowers the perceived social cost of acquisition and sustains higher demand months after the intervention.

This interpretation aligns with evidence from other health contexts. Dupas (2014) shows that short-run subsidies for preventive health products in Kenya increased long-run adoption even after the subsidy expired, and Fischer (2019) finds a similar pattern for water treatment in Uganda. In a comparable way, the results of this study suggest that access to information, like short-term subsidies, can have persistent effects on condom demand by facilitating learning and lowering initial barriers, even if attitudinal changes themselves are temporal.

A second and related mechanism is habit formation. For individuals who redeemed vouchers soon after treatment, the act of visiting a pharmacy may have helped overcome initial hesitation or stigma associated with condom purchase. Once this initial barrier is crossed, repetition lowers both psychological and practical costs, turning one-time actions into more automatic behaviors. Over time, this process can create self-reinforcing patterns in which early learning triggers a behavioral response, and repeated engagement consolidates that response into a lasting habit. In this sense, learning and habit formation may operate together, information initiates behavioral change, and habit formation sustains it.

5 Conclusion

This paper uses a randomized field experiment to examine how access to health risk information and peer view information influence demand for condoms and perceptions toward contraceptives in general. Using condom voucher redemption as an objective measure of demand, I find that, among 2,151 unmarried male university students in Ghana, exposure to accurate information about the risks of unprotected sex increased actual condom take-up by 32 percent relative to the control group, with effects persisting at roughly 57 percent eight weeks later. These behavioral responses are concentrated among respondents who are more likely to be sexually active. In contrast, peer view information, which highlighted the social acceptability of condom purchases and use, had no additional effect on actual demand.

Interestingly, the provision of health risk information also temporarily improved perceptions of condoms, but these attitudinal gains faded over time, while the behavioral effects remained. This divergence between beliefs and behavior suggests that initial information shocks can set durable behavioral changes in motion even when shifts in perceptions do not persist.

A limitation of the available data is that it is not possible to test whether the treatment effect varied with prior beliefs about health risks, as these were not measured at the baseline. As a result, one could wonder whether the treatment effect truly closed this gap. Similarly, while stigma clearly affects redemption behavior (respondents more often request condoms when they are offered in non-transparent packaging) this experiment cannot identify which form of stigma drives demand. What is clear is that peer view information does not mitigate this concern.

Nevertheless, these findings highlight that even in environments in which informational

campaigns are common, well-designed and credible messages about health risks can meaningfully shift preventive behavior. Clear evidence on the consequences of unprotected sex induces a persistent increase in condom uptake, while messages aimed at correcting perceived social norms appear to be less effective.

More broadly, the results highlight that temporary changes in beliefs can generate lasting behavioral effects, particularly among individuals for whom the information is most relevant. For experiential goods such as condoms, even short-lived shifts in perceptions can prompt experimentation that reinforces itself through experience. Once individuals act on new information and update their beliefs through firsthand use, behavior can remain altered long after the initial informational effect has faded.

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A Appendix A

A.1 Figures

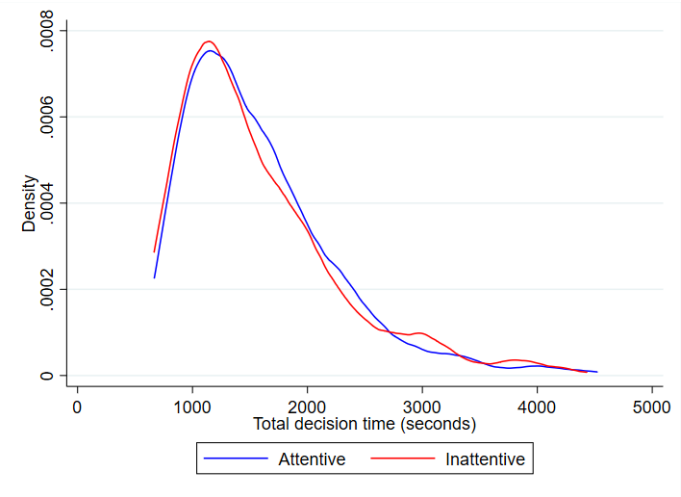


Figure A1: Survey completion time.

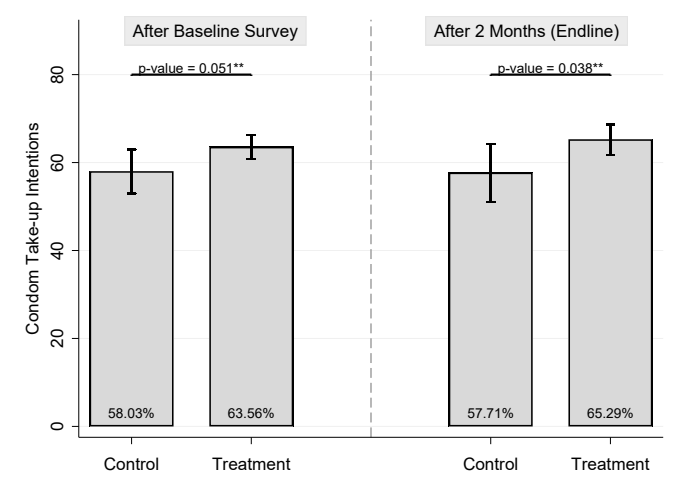


Figure A2: Effect of receiving information on condom take-up intentions. The left panel illustrates the estimated effects of receiving either type of treatment information on condom take-up intentions recorded in the baseline survey, and the right panel shows the effect of receiving either type of treatment information on condom take-up intentions recorded in the endline survey.

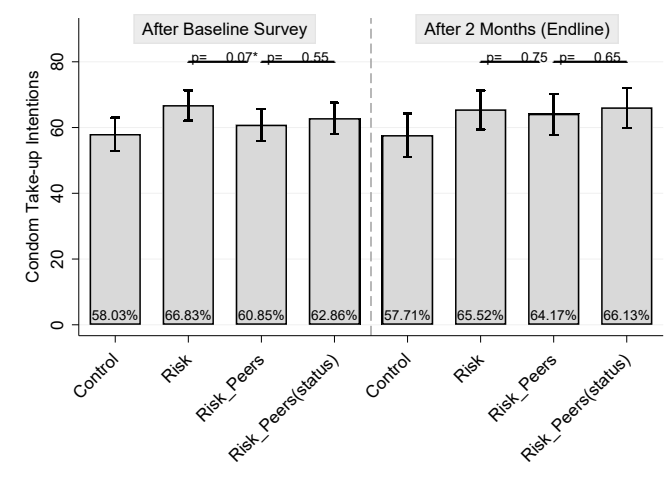


Figure A3: Effect of receiving information on condom take-up intentions. The left panel illustrates the estimated effects of receiving a specific type of treatment information on condom take-up intentions recorded in the baseline survey, and the right panel shows the effect of receiving a specific type of treatment information on condom take-up intentions recorded in the endline survey.

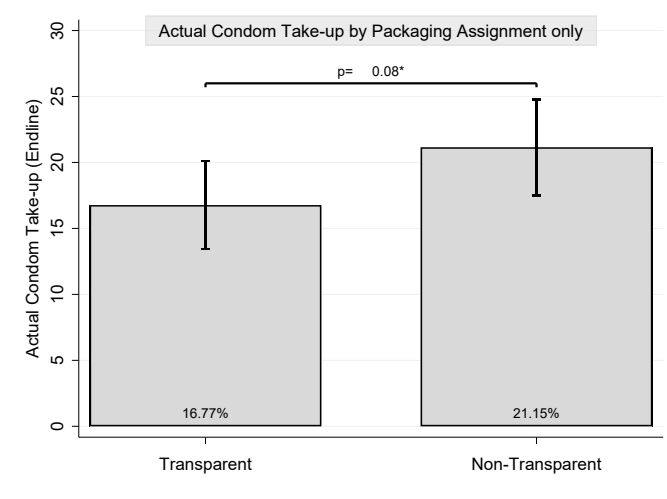


Figure A4: Differences in redemption rates by packaging assignment, unconditional on information treatment.

A.2 Tables

Table A1: Effects of Receiving Information on Behavior: Robustness

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
PANEL A: Pooled Treatment											
Any Kind of Treatment	0.0748***	0.0748***	0.0745***	0.0731***	0.0727***	0.0714***	0.0710***	0.0713***	0.0713***	0.0735***	0.0759***
	(0.0269)	(0.0271)	(0.0271)	(0.0269)	(0.0269)	(0.0268)	(0.0267)	(0.0268)	(0.0268)	(0.0269)	(0.0266)
Observations	976	976	975	975	975	975	975	975	975	975	975
R^2	0.006	0.006	0.013	0.040	0.043	0.058	0.065	0.065	0.065	0.066	
Control mean	0.132	0.132	0.132	0.132	0.132	0.132	0.132	0.132	0.132	0.132	0.132
Specification											
Pre-registered set of controls	No	No	No	No	No	No	No	Yes	No	No	No
Double- selection LASSO Linear regression	No	No	No	No	No	No	No	No	No	No	Yes
Baseline Controls											LASSO selected:
None	Yes	No	No	No	No	No	No	No	No	No	No
Children	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No
Age	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No
Ethnicity	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No
Income	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No
Region of Residence	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No
Encounted bias about contraception	No	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes	No
Education	No	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes	No
Religious	No	No	No	No	No	Yes	Yes	Yes	Yes	Yes	No
Has/Had boyfriend/girlfriend	No	No	No	No	No	Yes	Yes	Yes	Yes	Yes	No
Unemployed	No	No	No	No	No	No	Yes	Yes	Yes	Yes	Yes
Prior beliefs about peers	No	No	No	No	No	No	No	Yes	Yes	Yes	No
Identifies Prestige	No	No	No	No	No	No	No	No	Yes	Yes	No
Exposed to other health interventions	No	No	No	No	No	No	No	No	No	Yes	No

Data from Endline Survey. Sample restricted to attentive respondents. Estimates from OLS regression. Standard errors are clustered at the individual level and reported in brackets. The dependent variable is an indicator for voucher redemption at endline. Columns 1–11 report regression coefficients for different sets of control variables. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A2: Respondent Attrition at Endline Survey

	(1) Participated in EndLine Survey
PANEL A: Pooled Treatment	
Any Treatment	-0.0199
	(0.0287)
R^2	0.000
PANEL B: Specific Treatment	
Risk_Peers(status)	-0.0139
	(0.0348)
Risk_Peers	-0.0104
	(0.0351)
Risk	-0.0348
	(0.0345)
R^2	0.001
Control Mean	0.412
N	1618
Omnibus test for a joint effect of interaction terms of Treatment and pre-specified controls:	
P-value (any Treatment)	0.000
P-value (Risk_Peers(status), Risk_Peers, Risk)	0.000
Lee(2009) Bounds: T	
Lower	0.0480 (0.0481)
Upper	0.0818 (0.0291)

Data from endline survey. Sample restricted to attentive respondents. The dependent variable is an indicator for whether a respondent participated in the endline survey. The omnibus randomization test of joint significance presents a p-value from an F-test for joint significance of the coefficients for treatment assignment and for all interactions of pre-specified controls with treatment in an OLS regression, with participation as the dependent variable and treatment, pre-specified controls, and their interaction terms as independent variables. Standard errors are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A3: Effect of Receiving Information on Perceptions about Contraceptives: Interactions

	LPM with Pre-registered Controls					Double-select. LASSO Regression				
	(1) Cond. Safe	(2) Cond. Moral	(3) Contracept. Moral	(4) Cond. purchase not shameful	(5) Contracept. not self contrl	(6) Cond. Safe	(7) Cond. Moral	(8) Contracept. Moral	(9) Cond. purchase not shameful	(10) Contracept. not self contrl
PANEL A: Pooled Treatment										
Any Treatment	0.428**	0.174	0.215	-0.00628	-0.00652	0.413**	0.160	0.222	-0.00701	-0.00707
	(0.169)	(0.198)	(0.190)	(0.0261)	(0.0265)	(0.169)	(0.198)	(0.191)	(0.0262)	(0.0265)
Time	0.668***	0.483*	0.295	0.0435	-0.0200	0.661***	0.436*	0.269	0.0436	-0.0190
	(0.219)	(0.259)	(0.255)	(0.0360)	(0.0384)	(0.218)	(0.259)	(0.254)	(0.0362)	(0.0384)
T× Time	-0.197	-0.313	-0.177	0.0123	0.0370	-0.189	-0.271	-0.159	0.0134	0.0350
	(0.245)	(0.299)	(0.294)	(0.0412)	(0.0437)	(0.244)	(0.300)	(0.293)	(0.0413)	(0.0437)
R^2	0.046	0.049	0.042	0.018	0.038					
PANEL B: Specific Treatment										
Risk_Peers(status)	0.469**	0.281	0.364	-0.0342	-0.0104	0.445**	0.249	0.331	-0.0330	-0.0161
	(0.200)	(0.240)	(0.231)	(0.0324)	(0.0324)	(0.198)	(0.241)	(0.233)	(0.0323)	(0.0325)
Risk_Peers	0.419**	0.210	0.236	0.0408	0.0184	0.405**	0.142	0.190	0.0419	0.0147
	(0.204)	(0.240)	(0.235)	(0.0312)	(0.0321)	(0.204)	(0.239)	(0.235)	(0.0312)	(0.0321)
Risk	0.397**	0.0309	0.0464	-0.0235	-0.0265	0.391*	-0.00922	0.0467	-0.0211	-0.0249
	(0.201)	(0.242)	(0.232)	(0.0321)	(0.0321)	(0.200)	(0.240)	(0.232)	(0.0321)	(0.0324)
Time	0.669***	0.484*	0.296	0.0435	-0.0200	0.662***	0.449*	0.270	0.0422	-0.0201
	(0.219)	(0.259)	(0.256)	(0.0360)	(0.0384)	(0.218)	(0.260)	(0.255)	(0.0363)	(0.0386)
Risk_Peers(status) × Time	-0.364	-0.408	-0.423	0.0311	0.0379	-0.345	-0.385	-0.406	0.0333	0.0366
	(0.289)	(0.371)	(0.362)	(0.0506)	(0.0528)	(0.289)	(0.373)	(0.363)	(0.0507)	(0.0530)
Risk_Peers × Time	-0.300	-0.641*	-0.387	-0.00427	0.0128	-0.280	-0.604	-0.364	-0.000894	0.0121
	(0.298)	(0.373)	(0.363)	(0.0491)	(0.0530)	(0.297)	(0.374)	(0.363)	(0.0492)	(0.0530)
Risk × Time	0.0561	0.0821	0.252	0.0105	0.0590	0.0441	0.0975	0.242	0.0123	0.0555
	(0.285)	(0.353)	(0.349)	(0.0501)	(0.0522)	(0.284)	(0.354)	(0.348)	(0.0503)	(0.0526)
R^2	0.047	0.051	0.043	0.022	0.039					
Control mean	7.793	7.031	5.635	0.739	0.701	7.793	7.031	5.635	0.739	0.701
N	2590	2590	2590	2590	2590	2590	2590	2590	2590	2590
Test:										
Risk_Peers=Risk	-0.356	-0.723	-0.639	-0.015	-0.046					
P-value	(0.191)	(0.044)	(0.068)	(0.759)	(0.365)					
Risk_Peers(status)=Risk_Peers	-0.064	0.233	-0.036	0.035	0.025					
P-value	(0.816)	(0.536)	(0.922)	(0.467)	(0.625)					

Data from main and endline surveys. Sample restricted to attentive respondents. Standard errors are in brackets. *Time* is an indicator for survey round. Panel A compares receiving either type of treatment to the control group, while Panel B compares the effect of receiving a specific treatment to the control group. Columns 1–5 use the linear probability model, and columns 6–10 use the double LASSO regression model. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

B Appendix B: Results for Full Sample

B.1 Figures

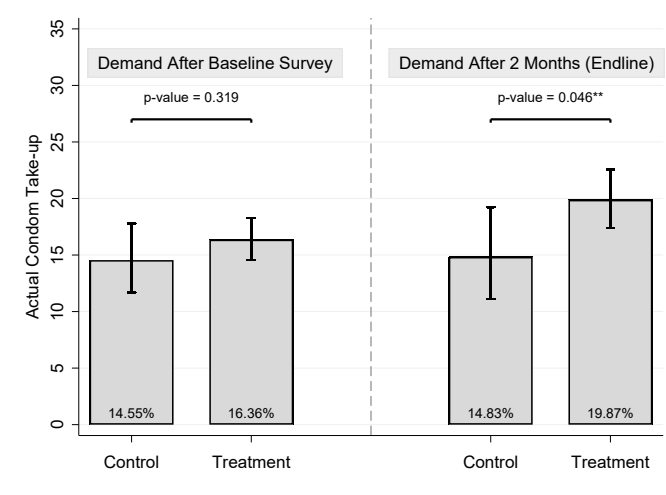


Figure B1: Effect of receiving information on condom demand. The left panel illustrates the estimated effect of receiving either type of treatment on actual condom take-up after the expiry of the first voucher, while the right panel shows the effect of receiving either type of treatment on actual condom take-up after the expiry of the third voucher.

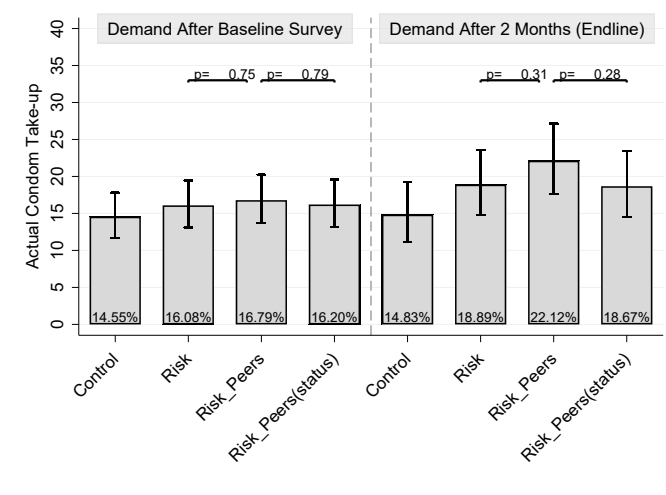


Figure B2: Effect of receiving information on condom demand. The left panel illustrates the estimated effects of receiving a specific type of treatment on actual condom take-up after the expiry of the first voucher, and the right panel shows the effect of receiving a specific type of treatment on actual condom take-up after the expiry of the third voucher.

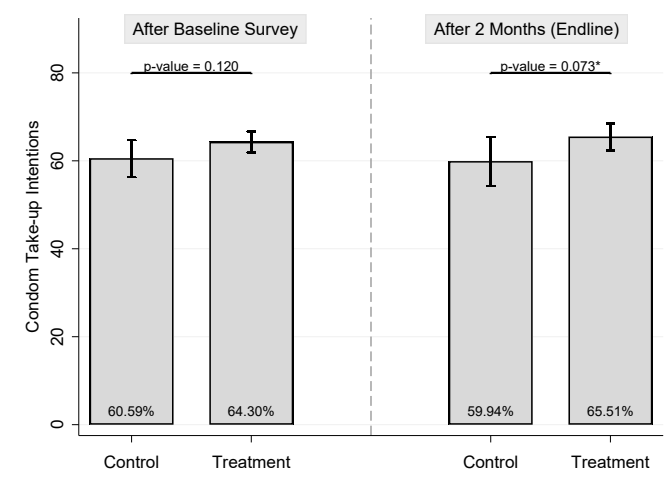


Figure B3: Effect of receiving information on condom take-up intentions. The left panel illustrates the estimated effects of receiving either type of treatment information on condom take-up intentions recorded in the baseline survey, and the right panel shows the effect of receiving either type of treatment information on condom take-up intentions recorded in the endline survey.

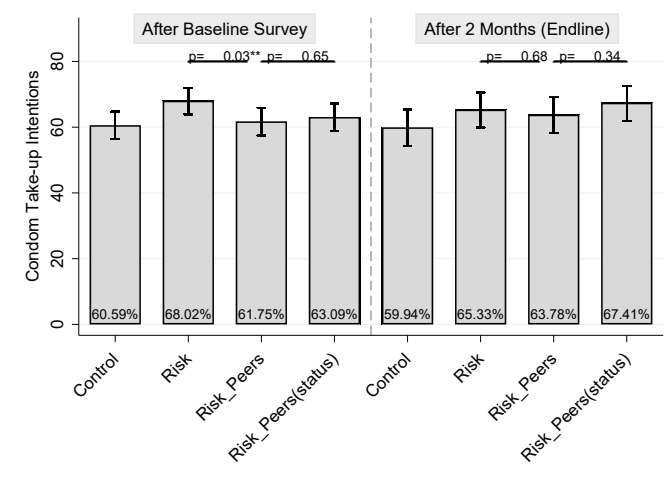


Figure B4: Effect of receiving information on condom take-up intentions. The left panel illustrates the estimated effects of receiving a specific type of treatment information on condom take-up intentions recorded in the baseline survey, and the right panel shows the effect of receiving a specific type of treatment information on condom take-up intentions recorded in the endline survey.

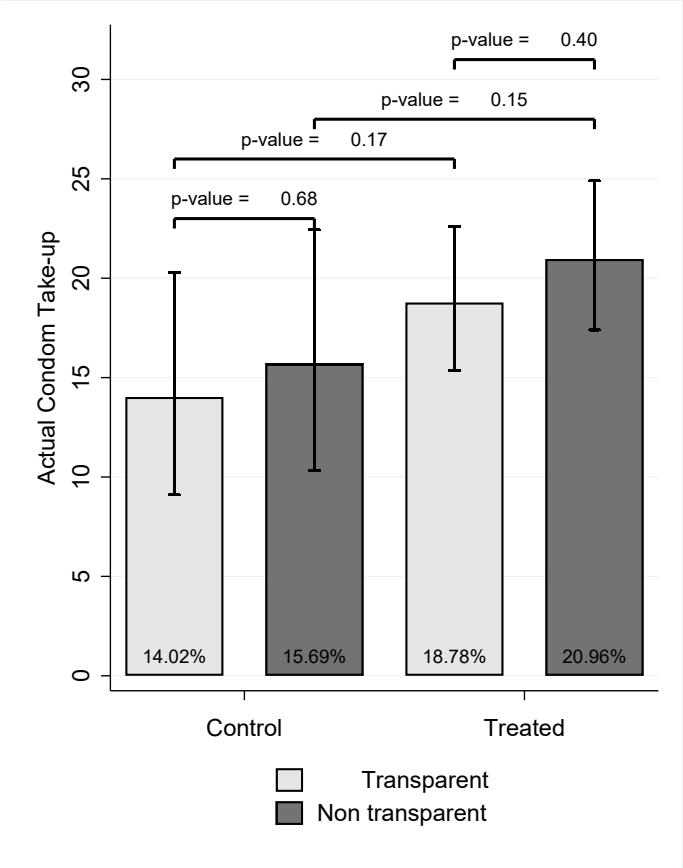


Figure B5: Effect of receiving information on condom demand by packaging assignment. The figure illustrates the effect on actual condom take-up at endline by packaging type assignments.

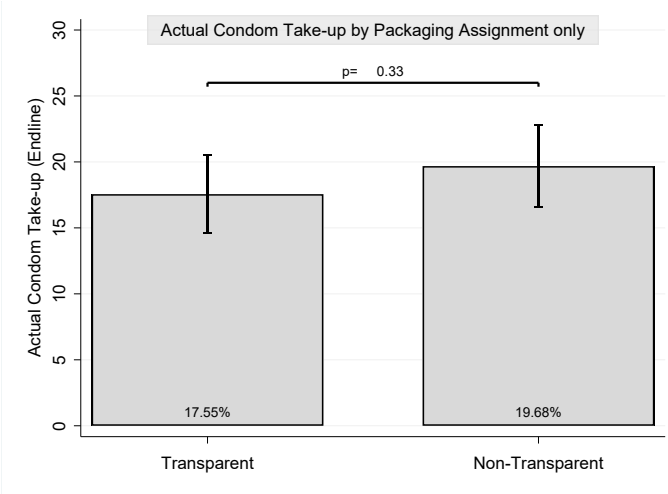
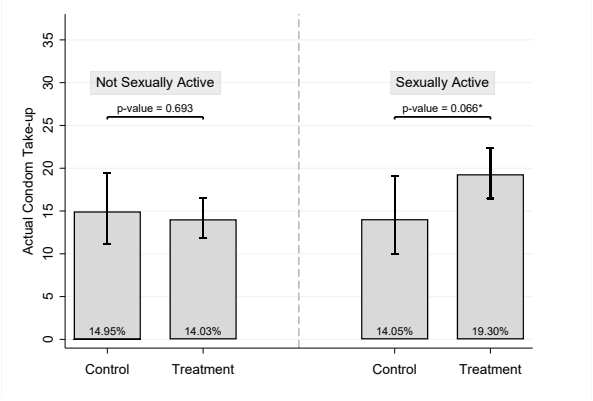


Figure B6: Differences in redemption rates by packaging assignment, unconditional on information treatment.

(a) After Baseline Survey



(b) After Endline Survey

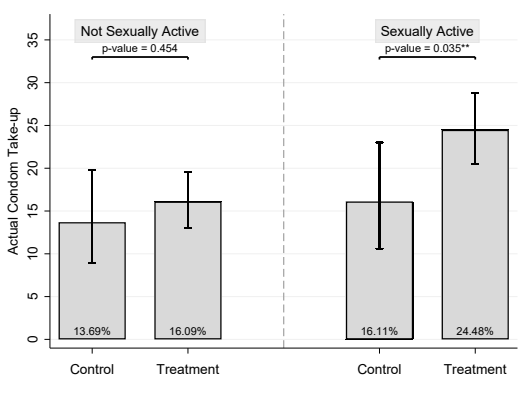


Figure B7: Effect of receiving information on actual condom take-up: heterogeneity by past sexual activity status

B.2 Tables

Table B1: Summary Statistics (Full Sample)

	(1) All	(2) Risk_Peers(status)	(3) Risk_Peers	(4) Risk	(5) Control	(6) (2) vs. (3)	(7) (3) vs. (4)	(8) (4) vs. (5)
N	2151	531	536	541	543	1062	1072	1082
Age	21.67	21.59	21.90	21.51	21.68	0.12	0.06	0.37
	(3.25)	(3.00)	(3.58)	(3.11)	(3.28)			
Number of Children	0.01	0.00	0.01	0.01	0.01	0.06	0.78	0.36
	(0.10)	(0.06)	(0.12)	(0.11)	(0.09)			
Prior Belief About Peers' View (%)	47.69	46.22	47.72	49.37	47.43	0.37	0.31	0.22
	(26.54)	(27.33)	(26.96)	(25.96)	(25.90)			
Identifies with a Particular Religion (%)	93.17	92.84	94.22	93.72	91.90	0.36	0.73	0.25
Encountered Ethnic Biases Related to Family Planning (%)	20.87	21.28	20.90	22.37	18.97	0.88	0.56	0.17
Undergraduate Student(%)	95.72	96.61	95.15	95.19	95.95	0.23	0.97	0.55
Unemployed(%)	88.10	88.32	85.07	91.13	87.85	0.12	0.00	0.08
Ever had a Boy/Girlfriend(%)	65.13	64.41	67.16	64.33	64.64	0.34	0.33	0.91
Identify prestige characteristics(%)	83.73	81.54	84.89	84.47	83.98	0.14	0.85	0.82
Received Sexual/Reproductive Health Edu. in the Last 90 Days (%)	37.94	38.61	41.79	38.08	33.33	0.29	0.21	0.10
Household Income								
Up to 2,000 Cedis	65.09	67.61	65.49	62.29	65.01	0.46	0.28	0.35
2,000 - 4,999 Cedis	21.76	19.21	22.76	22.55	22.47	0.15	0.93	0.97
5,000 - 7,999 Cedis	7.30	7.53	6.16	8.32	7.18	0.37	0.17	0.48
8,000 - 11,999 Cedis	2.79	3.20	2.99	2.96	2.03	0.84	0.98	0.33
Above 12,000 Cedis	3.07	2.45	2.61	3.88	3.31	0.86	0.24	0.62
Ethnicity								
Akan	65.92	62.90	68.28	66.91	65.56	0.06	0.63	0.64
Ewe	13.25	13.94	12.50	14.23	12.34	0.49	0.40	0.36
Ga-Adangbe	7.72	8.10	7.28	6.65	8.84	0.61	0.69	0.18
Mole-Dagbani	9.39	10.92	8.40	8.50	9.76	0.16	0.95	0.47
Guan	2.42	2.64	2.24	2.22	2.58	0.67	0.98	0.70
Gurma	1.30	1.51	1.31	1.48	0.92	0.78	0.81	0.40
Region of Residence								
Western	6.23	6.03	7.09	5.73	6.08	0.48	0.36	0.81
Central	6.51	7.16	5.78	5.91	7.18	0.36	0.93	0.40
Greater Accra	32.45	32.77	34.51	34.57	27.99	0.55	0.99	0.02
Volta Region	2.32	2.26	2.05	1.66	3.31	0.82	0.64	0.08
Eastern	7.39	6.97	7.09	7.21	8.29	0.94	0.94	0.51
Ashanti	32.82	30.89	32.84	32.53	34.99	0.49	0.92	0.39
Brong-Ahafo	4.04	3.95	4.48	4.44	3.31	0.67	0.97	0.34
Northern	1.91	2.45	0.75	2.22	2.21	0.03	0.05	0.99
Upper East	0.98	0.94	0.75	0.74	1.47	0.73	0.99	0.25
Upper West	0.88	1.51	0.75	0.55	0.74	0.24	0.70	0.71
Bono East	1.21	1.69	1.49	1.29	0.37	0.79	0.78	0.09
Ahafo	1.02	0.56	1.12	1.29	1.10	0.32	0.79	0.78
Savannah	0.65	1.13	0.19	0.37	0.92	0.06	0.57	0.26
North East	0.46	0.75	0.19	0.55	0.37	0.18	0.32	0.65
Oti	0.23	0.38	0.00	0.00	0.55	0.16	.	0.08
Western North	0.88	0.56	0.93	0.92	1.10	0.49	0.99	0.77
Test of Joint Significance of all Variables								
P-value								0.25

Note: Baseline data. Treatment is a dummy equal to 1 if the respondent received the intervention at baseline. The F-test p-value is the joint significance of the differences between the treatment and control groups for all variables reported in the table. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table B2: Effects of Receiving Information on Behavior (Full Sample)

	Actual Take-up			Take-up Intentions		
	(1) Main Survey	(2) Follow Up	(3) Endline Survey	(4) Main Survey	(5) Follow Up	(6) Endline Survey
Panel A: Pooled Treatment						
Linear probability model with pre-registered set of controls						
Any Treatment	0.0150	-0.0293**	0.0497**	0.0330	0.0339	0.0467
	(0.0178)	(0.0129)	(0.0234)	(0.0234)	(0.0282)	(0.0315)
R^2	0.040	0.032	0.054	0.080	0.065	0.074
Double-selection LASSO linear regression						
Any Treatment	0.0171	-0.0290**	0.0510**	0.0326	0.0419	0.0520*
	(0.0177)	(0.0131)	(0.0236)	(0.0232)	(0.0281)	(0.0315)
PANEL B: Specific Treatment						
Linear probability model with pre-registered set of controls						
Risk_Peers(status)	0.0144	-0.0343**	0.0397	0.0269	0.0344	0.0709*
	(0.0220)	(0.0146)	(0.0296)	(0.0286)	(0.0340)	(0.0379)
Risk_Peers	0.0215	-0.0239	0.0750**	0.00609	0.0203	0.0263
	(0.0222)	(0.0150)	(0.0304)	(0.0289)	(0.0344)	(0.0387)
Risk	0.00911	-0.0294**	0.0355	0.0657**	0.0476	0.0425
	(0.0221)	(0.0150)	(0.0293)	(0.0285)	(0.0345)	(0.0379)
R^2	0.040	0.033	0.056	0.082	0.065	0.076
Double-selection LASSO linear regression						
Risk_Peers(status)	0.0147	-0.0324**	0.0426	0.0250	0.0393	0.0708*
	(0.0219)	(0.0146)	(0.0294)	(0.0285)	(0.0341)	(0.0381)
Risk_Peers	0.0267	-0.0252	0.0773**	0.00756	0.0260	0.0310
	(0.0220)	(0.0153)	(0.0305)	(0.0287)	(0.0343)	(0.0385)
Risk	0.00995	-0.0294**	0.0325	0.0648**	0.0615*	0.0487
	(0.0218)	(0.0149)	(0.0295)	(0.0283)	(0.0345)	(0.0381)
Control mean	0.606	0.599	0.599	0.145	0.059	0.148
N	2143	1549	1264	2143	1549	1264
Test:						
Any Treament= Control	0.015	-0.029	0.050	0.033	0.034	0.047
P-value	(0.401)	(0.024)	(0.034)	(0.159)	(0.229)	(0.139)
Risk_Peers=Risk	0.012	0.005	0.040	-0.060	-0.027	-0.016
P-value	(0.583)	(0.676)	(0.219)	(0.037)	(0.424)	(0.667)
Risk_Peers(status)=Risk_Peers	-0.007	-0.010	-0.035	0.021	0.014	0.045
P-value	(0.754)	(0.407)	(0.270)	(0.472)	(0.676)	(0.231)

Data from Main, Follow-up, and Endline Surveys. Estimates from OLS regression. Standard errors are in brackets. Panel A compares receiving either type of treatment to the control group, while Panel B compares specific treatments to the control group. Columns 1–3 show the effect of treatment on take-up intentions, and Columns 4–6 show the effect of treatment on actual condom take-up. All regressions include the pre-registered set of controls. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table B3: Effect of Receiving Information on Actual Condom Take-up: Heterogeneity by Direction of Wedges and Sexual Activity Status (Full Sample)

	After Main Survey		After Endline Survey	
	Not Sexually Active (1)	Sexually Active (2)	Not Sexually Active (3)	Sexually Active (4)
PANEL A: Pooled Treatment				
LPM with pre-registered controls				
Any Kind of Treatment	-0.00570	0.0403	0.0378	0.0807**
	(0.0240)	(0.0270)	(0.0318)	(0.0369)
R^2	0.034	0.069	0.051	0.101
Comparison chi-sq (p-value)	1.676(0.195)		0.820(0.365)	
Double-select. LASSO regression				
Any Kind of Treatment	-0.00736	0.0464*	0.0285	0.0752**
	(0.0236)	(0.0267)	(0.0311)	(0.0361)
PANEL B: Specific Treatment				
LPM with pre-registered controls				
Risk_Peers(status)	-0.0272	0.0724**	0.0766*	0.0176
	(0.0277)	(0.0360)	(0.0413)	(0.0450)
Risk_Peers	0.00913	0.0362	0.0184	0.157***
	(0.0302)	(0.0338)	(0.0382)	(0.0492)
Risk	0.00220	0.0153	0.0188	0.0667
	(0.0298)	(0.0336)	(0.0388)	(0.0477)
R^2	0.036	0.071	0.055	0.114
Double-select. LASSO regression				
Risk_Peers(status)	-0.0284	0.0732**	0.0629	0.00934
	(0.0275)	(0.0357)	(0.0400)	(0.0443)
Risk_Peers	0.00939	0.0458	0.0128	0.156***
	(0.0296)	(0.0332)	(0.0382)	(0.0483)
Risk	-0.00197	0.0226	0.00947	0.0516
	(0.0292)	(0.0330)	(0.0375)	(0.0468)
Control Mean	0.150	0.140	0.137	0.161
N	1195	948	688	576

Data from Main, Follow-up, and Endline Surveys. Estimates from OLS regression. Standard errors are in brackets. Panel A compares receiving either type of treatment to the control group, while Panel B compares specific treatments to the control group. Columns 1–3 show the effect of treatment on take-up intentions, and Columns 4–6 show the effect of treatment on actual condom take-up. All regressions include the pre-registered set of controls. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table B4: Effect of Receiving Information on Self-Reported Sexual Behavior at Endline (Full Sample)

	Self Reported Sexual Behavior				Composite Indices of		
	(1) Sexually active last 90 days	(2) Multi. partners last 90 days	(3) Used condom at last sex	(4) Used contracep last 90 days	(5) Used condom last 90 days	(6) Practice safe sex	(7) Practice risky sex
Pooled Treatment							
Any Kind of Treatment	0.0205	0.00857	0.0263	-0.0143	0.0234	0.0109	0.0114
	(0.0208)	(0.0122)	(0.0240)	(0.0163)	(0.0176)	(0.0144)	(0.00854)
R^2	0.039	0.021	0.034	0.032	0.021	0.022	0.018
Double-select. LASSO Regression							
Any Kind of Treatment	0.0228	0.00963	0.0236	-0.0155	0.0238	0.0107	0.0127
	(0.0205)	(0.0120)	(0.0239)	(0.0164)	(0.0175)	(0.0143)	(0.00852)
PANEL B: Specific Treatment							
Risk_Peers(status)	0.0229	0.0144	0.0152	-0.00432	0.0191	0.0113	0.00936
	(0.0258)	(0.0155)	(0.0297)	(0.0203)	(0.0218)	(0.0180)	(0.0104)
Risk_Peers	0.00921	0.00419	0.0275	-0.0402**	0.0184	-0.00115	0.0222**
	(0.0255)	(0.0149)	(0.0296)	(0.0188)	(0.0218)	(0.0175)	(0.0101)
Risk	0.0292	0.00707	0.0363	0.00126	0.0327	0.0222	0.00291
	(0.0259)	(0.0151)	(0.0297)	(0.0201)	(0.0219)	(0.0178)	(0.0107)
R^2	0.040	0.021	0.034	0.034	0.021	0.022	0.020
Double-select. LASSO Regression							
Risk_Peers(status)	0.0238	0.0145	0.0185	-0.00702	0.0217	0.0121	0.0107
	(0.0254)	(0.0153)	(0.0296)	(0.0202)	(0.0217)	(0.0178)	(0.0103)
Risk_Peers	0.0138	0.00754	0.0233	-0.0398**	0.0184	-0.00101	0.0240**
	(0.0252)	(0.0149)	(0.0294)	(0.0189)	(0.0218)	(0.0174)	(0.0101)
Risk	0.0308	0.00691	0.0289	0.000237	0.0312	0.0209	0.00365
	(0.0256)	(0.0148)	(0.0295)	(0.0204)	(0.0218)	(0.0178)	(0.0107)
Control mean	0.223	0.059	0.376	0.127	0.299	0.242	0.466
N	2143	2143	2143	2143	2143	2143	2143

Data from Endline Survey. Estimates from OLS regression. Standard errors are in brackets. Columns 1–4 show the effect of treatment on self-reported sexual behavior. Column 5 is an index averaging responses to columns 1 and 3, column 6 averages responses to columns 1, 2, and 3, and column 7 averages responses to columns 1 and 2 minus the response to column 4. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table B5: Effect of Receiving Information on Perceptions about Contraceptives (Full Sample)

	Short term Effects								Long term Effects					
	(1) Overall Att Contracept	(2) Cond. Safe	(3) Cond. ATMs promiscuity	(4) Petition for Cond. ATMs	(5) Cond. Moral	(6) Contracept. Moral	(7) Cond. purchase not shameful	(8) Contracept. not self contrl	(9) Overall Att Contracept	(10) Cond. Safe	(11) Cond. Moral	(12) Contracept. Moral	(13) Cond. purchase not shameful	(14) Contracept. not self contrl
PANEL A: Pooled Treatment														
LPM with Pre-registered Controls														
Any Kind of Treatment	0.160**	0.341**	0.258	0.147	0.118	0.240	0.0222	-0.00948	0.0124	0.190	-0.138	-0.00947	0.0187	0.00157
	(0.0724)	(0.144)	(0.162)	(0.174)	(0.167)	(0.164)	(0.0230)	(0.0229)	(0.0872)	(0.151)	(0.192)	(0.192)	(0.0283)	(0.0303)
R^2	0.081	0.027	0.070	0.053	0.050	0.039	0.017	0.034	0.075	0.052	0.068	0.056	0.025	0.046
Double-select. LASSO Regression														
Any Kind of Treatment	0.144**	0.330**	0.240	0.132	0.0656	0.220	0.0220	-0.00869	0.0187	0.205	-0.145	0.0159	0.0195	0.000860
	(0.0723)	(0.142)	(0.162)	(0.172)	(0.167)	(0.163)	(0.0229)	(0.0228)	(0.0872)	(0.149)	(0.191)	(0.189)	(0.0282)	(0.0299)
PANEL B: Specific Treatment														
LPM with Pre-registered Controls														
Risk_Peers(status)	0.166*	0.299*	0.375*	0.0510	0.163	0.274	-0.00716	0.00617	-0.0768	0.0212	-0.203	-0.203	0.00361	-0.00245
	(0.0886)	(0.174)	(0.200)	(0.215)	(0.204)	(0.200)	(0.0284)	(0.0280)	(0.110)	(0.183)	(0.246)	(0.242)	(0.0349)	(0.0372)
Risk_Peers	0.159*	0.310*	0.392*	0.0772	0.0811	0.196	0.0626**	-0.00298	-0.0801	0.131	-0.363	-0.212	0.0504	-0.00738
	(0.0889)	(0.173)	(0.201)	(0.215)	(0.204)	(0.202)	(0.0274)	(0.0280)	(0.109)	(0.185)	(0.244)	(0.239)	(0.0341)	(0.0372)
Risk	0.154*	0.415**	0.00864	0.313	0.109	0.249	0.0116	-0.0316	0.187*	0.409**	0.137	0.370	0.00330	0.0140
	(0.0904)	(0.172)	(0.199)	(0.213)	(0.207)	(0.202)	(0.0283)	(0.0282)	(0.105)	(0.178)	(0.229)	(0.233)	(0.0348)	(0.0364)
R^2	0.081	0.027	0.072	0.053	0.050	0.039	0.020	0.035	0.081	0.056	0.071	0.062	0.027	0.047
Double-select. LASSO Regression														
Risk_Peers(status)	0.146*	0.283*	0.345*	0.0421	0.136	0.252	-0.00773	0.00388	-0.0680	0.0539	-0.212	-0.188	0.00750	0.001000
	(0.0886)	(0.172)	(0.200)	(0.213)	(0.205)	(0.199)	(0.0283)	(0.0280)	(0.111)	(0.182)	(0.246)	(0.241)	(0.0348)	(0.0366)
Risk_Peers	0.139	0.310*	0.364*	0.0436	0.0191	0.157	0.0625**	-0.00261	-0.0830	0.161	-0.395	-0.216	0.0500	-0.00987
	(0.0888)	(0.172)	(0.199)	(0.213)	(0.203)	(0.200)	(0.0273)	(0.0279)	(0.108)	(0.183)	(0.242)	(0.235)	(0.0339)	(0.0369)
Risk	0.147	0.395**	0.0125	0.308	0.0419	0.252	0.0110	-0.0271	0.183*	0.395**	0.117	0.384*	0.0102	0.00707
	(0.0899)	(0.170)	(0.199)	(0.211)	(0.205)	(0.200)	(0.0280)	(0.0282)	(0.104)	(0.175)	(0.226)	(0.228)	(0.0346)	(0.0363)
Control mean	4.795	7.540	5.632	6.541	6.941	5.517	0.689	0.702	4.572	8.227	7.369	5.823	0.741	0.700
N	2143	2143	2143	2143	2143	2143	2143	2143	1264	1264	1264	1264	1264	1264
Test:														
Any Treatment=Control	0.160	0.341	0.258	0.147	0.118	0.240	0.022	-0.009	0.012	0.190	-0.138	-0.009	0.019	0.002
P-value	(0.028)	(0.018)	(0.112)	(0.147)	(0.118)	(0.240)	(0.334)	(0.679)	(0.887)	(0.210)	(0.471)	(0.961)	(0.510)	(0.959)
Risk_Peers=Risk	0.006	-0.106	0.383	-0.236	-0.028	-0.053	0.051	0.029	-0.267	-0.278	-0.500	-0.582	0.047	-0.021
P-value	(0.950)	(0.524)	(0.059)	(0.274)	(0.891)	(0.792)	(0.062)	(0.311)	(0.015)	(0.115)	(0.040)	(0.016)	(0.165)	(0.559)
Risk_Peers(status) =Risk_Peers	0.006	-0.011	-0.016	-0.026	0.081	0.078	-0.070	0.009	0.003	-0.110	0.160	0.008	-0.047	0.005
P-value	(0.942)	(0.949)	(0.936)	(0.905)	(0.689)	(0.700)	(0.012)	(0.744)	(0.977)	(0.542)	(0.536)	(0.974)	(0.170)	(0.893)

Data from main and endline surveys. Standard errors are in brackets. Panel A compares receiving either type of treatment to the control group, while Panel B compares the effect of receiving a specific treatment to the control group. Columns 1–8 present the short-term effects of treatment on perceptions, while columns 9–14 show the long-term effects. Column 1 is an index averaging responses to columns 2–8, and column 9 is an index averaging responses to columns 10–14. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table B6: Effects of Receiving Information on Behavior: Robustness (Full Sample)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
PANEL A: Pooled Treatment											
Any Kind of Treatment	0.0505**	0.0503**	0.0504**	0.0526**	0.0530**	0.0503**	0.0491**	0.0497**	0.0498**	0.0519**	0.0510**
	(0.0238)	(0.0238)	(0.0238)	(0.0235)	(0.0235)	(0.0235)	(0.0234)	(0.0234)	(0.0234)	(0.0236)	(0.0236)
Observations	1268	1268	1264	1264	1264	1264	1264	1264	1264	1264	1264
R^2	0.003	0.004	0.013	0.035	0.038	0.047	0.054	0.054	0.054	0.055	
Control mean	0.148	0.148	0.148	0.148	0.148	0.148	0.148	0.148	0.148	0.148	0.148
Specification											
Pre-registered set of controls	No	No	No	No	No	No	No	Yes	No	No	No
Double- selection LASSO Linear regression	No	No	No	No	No	No	No	No	No	No	Yes
Baseline Controls											LASSO selected:
None	Yes	No	No	No	No	No	No	No	No	No	No
Children	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No
Age	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No
Ethnicity	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No
Income	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No
Region of Residence	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No
Encounted bias about contraception	No	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes	No
Education	No	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes	No
Religious	No	No	No	No	No	Yes	Yes	Yes	Yes	Yes	No
Has/Had boyfriend/girlfriend	No	No	No	No	No	Yes	Yes	Yes	Yes	Yes	No
Unemployed	No	No	No	No	No	No	Yes	Yes	Yes	Yes	Yes
Prior beliefs about peers	No	No	No	No	No	No	No	Yes	Yes	Yes	No
Identifies Prestige	No	No	No	No	No	No	No	No	Yes	Yes	No
Exposed to other health interventions	No	No	No	No	No	No	No	No	No	Yes	No

Data from Endline Survey. Estimates from OLS regression. Standard errors are clustered at the individual level and reported in brackets. The dependent variable is an indicator for voucher redemption at endline. Columns 1–11 report regression coefficients for different sets of control variables. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table B7: Respondent Attrition at Endline Survey (Full Sample)

	(1) Participated in EndLine Survey
PANEL A: Pooled Treatment	
Any Treatment	-0.00762
	(0.0245)
R^2	0.000
PANEL B: Specific Treatment	
Risk_Peers(status)	-0.0113
	(0.0300)
Risk_Peers	0.00170
	(0.0300)
Risk	-0.0132
	(0.0299)
R^2	0.000
Control Mean	0.416
N	2151
Omnibus test for a joint effect of interaction terms of Treatment and pre-specified controls:	
P-value (any Treatment)	0.077
P-value (Risk_Peers(status), Risk_Peers, Risk)	0.000
Lee(2009) Bounds: T	
Lower	0.0400 (0.0415)
Upper	0.0531 (0.0253)

Data from endline survey. The dependent variable is an indicator for whether a respondent participated in the endline survey. The omnibus randomization test of joint significance presents a p-value from an F-test for the joint significance of the coefficients for treatment assignment and all interactions of pre-specified controls with treatment in an OLS regression, with participation as the dependent variable and treatment, pre-specified controls, and their interaction terms as independent variables. Standard errors are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table B8: Effect of Receiving Information on Perceptions about Contraceptives: Interactions (Full Sample)

	LPM with Pre-registered Controls					Double-select. LASSO Regression				
	(1) Cond. Safe	(2) Cond. Moral	(3) Contracept. Moral	(4) Cond. purchase not shameful	(5) Contracept. not self contrl	(6) Cond. Safe	(7) Cond. Moral	(8) Contracept. Moral	(9) Cond. purchase not shameful	(10) Contracept. not self contrl
PANEL A: Pooled Treatment										
Any Treatment	0.339**	0.120	0.244	0.0225	-0.00805	0.331**	0.0897	0.245	0.0217	-0.00635
	(0.143)	(0.167)	(0.163)	(0.0229)	(0.0229)	(0.142)	(0.166)	(0.163)	(0.0229)	(0.0228)
Time	0.708***	0.460**	0.322	0.0535*	0.00209	0.681***	0.427**	0.286	0.0525*	0.00190
	(0.181)	(0.215)	(0.214)	(0.0315)	(0.0325)	(0.180)	(0.216)	(0.213)	(0.0316)	(0.0324)
T× Time	-0.140	-0.252	-0.245	-0.00287	0.00605	-0.118	-0.228	-0.216	-0.000712	0.00592
	(0.205)	(0.251)	(0.249)	(0.0362)	(0.0374)	(0.205)	(0.252)	(0.249)	(0.0363)	(0.0374)
R^2	0.042	0.050	0.038	0.017	0.030					
PANEL B: Specific Treatment										
Risk_Peers(status)	0.295*	0.177	0.280	-0.00725	0.00468	0.284*	0.141	0.262	-0.00785	0.00331
	(0.173)	(0.203)	(0.199)	(0.0283)	(0.0280)	(0.172)	(0.204)	(0.199)	(0.0283)	(0.0280)
Risk_Peers	0.309*	0.0826	0.198	0.0636**	-0.000170	0.304*	0.0118	0.159	0.0639**	-0.00100
	(0.172)	(0.203)	(0.201)	(0.0273)	(0.0281)	(0.172)	(0.202)	(0.200)	(0.0273)	(0.0280)
Risk	0.412**	0.101	0.254	0.0114	-0.0286	0.394**	0.0585	0.241	0.0114	-0.0283
	(0.171)	(0.206)	(0.201)	(0.0282)	(0.0281)	(0.170)	(0.205)	(0.201)	(0.0281)	(0.0282)
Time	0.708***	0.461**	0.323	0.0534*	0.00212	0.683***	0.437**	0.290	0.0519	0.00158
	(0.181)	(0.215)	(0.214)	(0.0316)	(0.0325)	(0.180)	(0.216)	(0.213)	(0.0316)	(0.0325)
Risk_Peers(status) × Time	-0.270	-0.389	-0.485	0.0119	-0.00598	-0.239	-0.381	-0.458	0.0149	-0.00656
	(0.250)	(0.316)	(0.310)	(0.0446)	(0.0458)	(0.251)	(0.318)	(0.311)	(0.0447)	(0.0459)
Risk_Peers × Time	-0.168	-0.441	-0.397	-0.0151	-0.0120	-0.135	-0.423	-0.372	-0.0131	-0.0119
	(0.250)	(0.314)	(0.308)	(0.0433)	(0.0461)	(0.251)	(0.315)	(0.308)	(0.0434)	(0.0462)
Risk × Time	0.0125	0.0641	0.134	-0.00464	0.0354	0.00957	0.0716	0.139	-0.000998	0.0341
	(0.244)	(0.305)	(0.304)	(0.0444)	(0.0456)	(0.244)	(0.305)	(0.304)	(0.0444)	(0.0459)
R^2	0.043	0.051	0.040	0.019	0.031					
Control mean	7.793	7.099	5.630	0.708	0.701	7.793	7.099	5.630	0.708	0.701
N	3407	3407	3407	3407	3407	3407	3407	3407	3407	3407
Test:										
Risk_Peers=Risk	-0.180	-0.506	-0.531	-0.010	-0.047					
P-value	(0.451)	(0.108)	(0.087)	(0.809)	(0.300)					
Risk_Peers(status)=Risk_Peers	-0.102	0.052	-0.088	0.027	0.006					
P-value	(0.677)	(0.872)	(0.781)	(0.533)	(0.896)					

Data from main and endline surveys. Standard errors are in brackets. *Time* is an indicator for survey round. Panel A compares receiving either type of treatment to the control group, while Panel B compares the effect of receiving a specific treatment to the control group. Columns 1–5 use the linear probability model, and columns 6–10 use the double LASSO regression model. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

C Appendix C: Survey Materials and Instructions

C.1 Wording of Information Treatments

There were three treatment variations and a control group. The treatment variations are the *Risk* Treatment, Risk with peer view Treatment (*Risk_Peers*), and Risk with peer view and salient status Treatment (*Risk_Peers*(*status*)). Below, I provide the information delivered in each treatment variation.

Risk Treatment. Respondents in this group received information about HIV/AIDS and abortions in Ghana. They saw this prompt;

HEALTH CONSEQUENCES OF UNPROTECTED SEX

Engaging in risky health behaviors, such as unprotected sex, can lead to severe consequences including sexually transmitted infections, unwanted pregnancies, and negative psychological effects. According to the Ghana AIDS Commission (n.d); **Over 340,000 people are** currently living with HIV/AIDS, primarily infecting young individuals aged 15-24. An estimated **11,705 new HIV infections are expected to occur** by the end of 2023.

In 2022, unprotected sex resulted in **approximately 21 abortions per day**, with the highest occurrence among women aged **20-24**.

These statistics highlight the crucial role of preventive measures, such as contraceptive use, in protecting one's health and reducing the risks of unwanted pregnancies and sexually transmitted infections.

Risk_Peers Treatment. Respondents in this group saw the same information presented in the Risk Treatment in addition to this prompt;

WHAT DO KNUST'S STUDENTS THINK ABOUT CONDOMS?

In an earlier study, we surveyed 201 **KNUST students** about their **views on the public purchase and use of condoms**.

The study found that: majority of these students, **(65%) do not agree** that **people who buy condoms have multiple sexual partners**, the rest of these students, **(35%) agree** that **people who buy condoms have multiple sexual partners**.

Risk_Peers(status) Treatment. Respondents in this group saw the same information presented in the Risk Treatment in addition to this prompt with emphasizes on the status of those peers surveyed.

WHAT DO KNUST'S ELITE STUDENTS THINK ABOUT CONDOMS?

In an earlier study, we surveyed 201 **KNUST students** about their **views on the public purchase and use of condoms**.

These students identify as medical students, social media/campus influencers, top performers in their departments, student leaders, confident and/or good public speakers, and are often perceived as such by the majority of their peers.

The study found that: majority of these students **(65%) do not agree** that **people who buy condoms have multiple sexual partners**, the rest of these students **(35%) agree** that **people who buy condoms have multiple sexual partners**.

[*All respondents who received the additional peer view information (Risk_Peers and Risk_Peers(status)) saw a graphical representation of the information with matching colours to enhance their understanding.*]

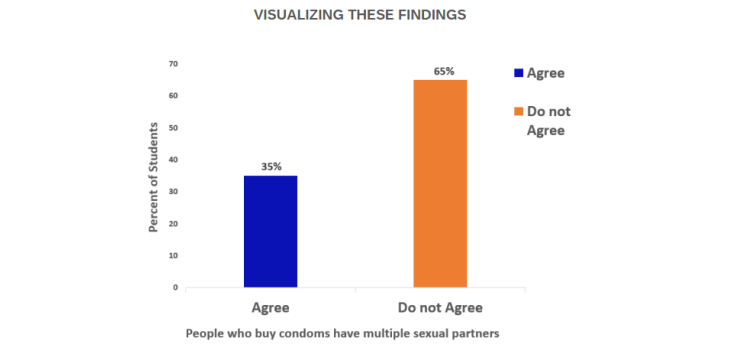


Figure C1: Graphical representation of peer view information, color-coded to facilitate understanding.

C.2 Packaging Assignment at the Endline Survey

Presentation of Condoms in Non-transparent Packaging

Figure C2: Condoms in non-transparent packaging.

Presentation of Condoms in Transparent Packaging

In addition to the possibility of winning **GH¢ 20.00**, you are eligible to receive **one pack of 3 condoms** from us. **The picture below shows how your package will look like.**

Figure C3: Condoms in transparent packaging.

Abstrakt

Zkoumám dvě potenciální překážky, které mohou omezovat poptávku po kondomech: nedostatečné informace o zdravotních rizicích a obavy ze sociální stigmatizace spojené s nákupem kondomů. Pomocí randomizovaného experimentu testuji, zda poskytování informací o (i) zdravotních rizicích a (ii) názorech vrstevníků na sociální přijatelnost nákupu kondomů může zvýšit poptávku po kondomech mezi mladými dospělými v Ghaně. Zjišťuji, že poskytování informací o zdravotních rizicích vedlo k 32% nárůstu poptávky. Naproti tomu poskytování dodatečných informací o názorech vrstevníků na sociální přijatelnost nákupu kondomů nemělo žádný významný vliv na poptávku po kondomech. Dále zjišťuji, že efekt informací o zdravotních rizicích na poptávku po kondomech je přetrvávající. Zajímavé je, že přestože dokumentuji přetrvávající účinky informací o zdravotních rizicích na poptávku po kondomech, informace mají pouze dočasné účinky na vnímání vhodnosti používání kondomů. Tyto výsledky naznačují, že cílené informace mohou dlouhodobě změnit zdravotní chování, i když se základní postoje mění jen pomalu.

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