Nudging Leads Consumers In Colorado To Shop But Not Switch ACA Marketplace Plans

Keith Marzilli Ericson, Jon Kingsdale, Timothy Layton, Adam Sacarny

Prepublication version. Published in Health Affairs, February 2017

Abstract

The Affordable Care Act dramatically expanded the use of regulated marketplaces in health insurance, but consumers often fail to shop for plans during open enrollment periods. These consumers are automatically re-enrolled in their old plans, potentially exposing them to unexpected increases in their insurance premiums and cost-sharing. We conducted a randomized intervention to encourage renewing Affordable Care Act Marketplace consumers to shop for plans. We tested the effect of letters and e-mails with personalized information about the savings on insurance premiums that they could realize from switching plans as well as generic messages that simply emphasized the possibility of saving. The personalized and generic messages both increased shopping on the website by 23 %, but neither message had a significant effect on plan switching. These findings show that simple "nudges" with even generic information can promote shopping in health insurance marketplaces, but whether they can lead to switching remains an open question.

Keith M. Marzilli Ericson is an assistant professor of markets, public policy, and law in the Questrom School of Business at Boston University, in Massachusetts, and a faculty research fellow at the National Bureau of Economic Research, in Cambridge, Massachusetts.

Jon Kingsdale is an associate professor of the practice in the Department of Health Law, Policy, and Management, School of Public Health, at Boston University, and director of the Wakely Consulting Group, in Boston.

Tim Layton is an assistant professor of health care policy at Harvard Medical School, in Boston.

Adam Sacarny (ajs2102@columbia.edu) is an assistant professor in the Department of Health Policy and Management at the Mailman School of Public Health, Columbia University, in New York City; a faculty research fellow at the National Bureau of Economic Research; and an affiliate at the Abdul Latif Jameel Poverty Action Lab (J-PAL), in Cambridge.

Introduction

An increasingly large fraction of Americans without access to employer-provided health insurance purchases their health insurance coverage on the marketplaces established by the Affordable Care Act (ACA). Marketplaces seek to deliver affordable, high-quality insurance to individuals, relying on consumer choice to incentivize firms to deliver value. In theory, when consumers actively shop on Marketplaces, they are able to enroll in plans that best meet their needs, placing competitive pressure on insurers that raise rates too far or cut provider networks too much.

Unfortunately, evidence from other markets suggests consumer choice may not provide the needed competitive pressure. Inertia in health plan choices leads consumers to simply stay in their default plan rather than switching to lower cost plans when they become available.[1,2,3] This provides insurers with an incentive to raise prices, since these inattentive consumers will not change plans as a result.[2]

As a result, policymakers have sought to encourage consumer choice and thereby strengthen insurer incentives. One aggressive option, adopted by Rhode Island, is to require returning enrollees to shop for coverage and actively choose their future plan each year, lest they lose coverage entirely.[4] This approach forces consumers to pay attention to their options, but risks disenrolling inattentive consumers who assumed their coverage would continue. In light of this concern, most marketplaces have opted to automatically renew passive enrollees into their old plan – though in the following year, these enrollees must pay the plan's new, and potentially higher, premiums and cost-sharing, and they are limited to the plan's new provider network.[5] Both of these options have potentially negative consequences: In the first approach, inattentive consumers lose coverage altogether, and in the second, they are re-enrolled in plans with new prices and provider networks that may not align with their preferences.

Other policymakers have proposed so-called "targeted defaults" where rather than re-enrolling consumers who fail to make an active choice in their previous plans, the Marketplace would assign these consumers to plans that appear to be a good match, such as the lowest premium plan or the lowest premium plan with a given set of providers in its network.[6] However, this type of proposal carries its own set of risks for consumers whose plans are switched. The problem of how to handle re-enrollment for consumers thus presents a dilemma for policymakers with no easy solution.

Fortunately, there exists an alternative to the more extreme renewal policies. Previous research on Medicare Advantage and Medicare Part D suggests that even with automatic renewal,

information "nudges" – interventions that provide individuals with information to help them act in their best interest, without forcing them to do so – can potentially induce shopping and switching.[7,8] However, until now such information nudges have not been tested in the ACA's health insurance marketplaces, where enrollees are younger and potentially more (or less) attentive to plan options than consumers in Medicare Part D.[9]

In this paper we use a large-scale randomized intervention to test the effects of a nudge that encouraged actual Marketplace enrollees in Colorado to shop; the intervention targeted enrollees who were slated to be automatically renewed for coverage in 2016. Specifically, we tested two types of messaging and compared them to the "as-usual" messaging, allowing us to address two questions. First, do generic "you could save money" messages induce shopping and plan switching? Second, does it help to add personalized information that indicates the amount of money the household could potentially save by switching? We tested the effects of these two interventions on the probability that enrollees visited the Marketplace website during open enrollment to shop, and, ultimately, whether they switched to another (potentially lower cost) plan.

Study Data and Methods

Research Design and Implementation

We worked with the Colorado health insurance marketplace, Connect for Health Colorado (the Marketplace), to design and implement the randomized information intervention for the open enrollment period leading up to coverage year 2016. By the end of 2015, the Marketplace covered 156,124 lives in its individual marketplace, making it the fifth-largest of the thirteen state-operated marketplaces in that year.[10,11] Colorado is a medium size state (5.5 million residents), with a diverse population living in both large urban and rural areas.

Connect for Health Colorado offers a more robust set of plan choices than the marketplaces in most other states. Depending on where they reside in the state, many Marketplace enrollees in 2015 could choose from over 70 plans from 10 carriers, including a large staff model HMO, other HMOs and PPOs. By contrast, consumers in the 38 states served by the Federally Facilitated Marketplace face on average a choice of only 46 health plans.[12] Compared to the rest of the nation, Colorado has a similar fraction of individuals who are potentially eligible for Marketplace coverage and choose to enroll (53% in Colorado, 46% nationally) and a similar age distribution of enrollees. However, enrollees in Colorado are more likely to choose a bronze tier plan (45% in Colorado, 23% nationally) and less likely to receive subsidies in the form of tax credits or cost-sharing reductions (61% in Colorado, 84% nationally).[13]

We developed two information treatments aimed at those 2015 enrollees who would be auto-renewed in their existing health plans for 2016 unless they actively shopped. The first was a "generic" treatment consisting of a letter and email encouraging the household to visit the Marketplace website or call its call center to actively shop for a plan. The second was a "personalized" treatment that was almost identical to the generic treatment with the addition of personalized information on the amount of money the household could potentially save by switching to the lowest premium plan in the same metal tier (i.e. similar coverage generosity) as their 2015 plan (see Appendix A for additional information about the interventions and Appendix C for samples of the messages that were sent).[14]

Most consumers in the Marketplace had access to plans of greatly varying generosity, from bronze tier plans expected to cover 60% of their health costs to platinum plans expected to cover 90% of costs. We limited the savings messages to plans in the same metal tier as the consumers' existing plans to keep the set of insurance products as similar as possible along the dimensions of generosity and financial protection.

Using administrative enrollment data from the Marketplace and plan premium data provided by Wakely Consulting Group, we identified a study sample of 15,534 households enrolled in a Marketplace plan in November 2015 and set to be re-enrolled in their plan for 2016 if they took no action. Because the personalized message told consumers they could save money by switching, we excluded from the study any household whose automatic enrollment 2016 plan (i.e. the plan they would receive without deliberately making an alternative selection – their current plan or, if their current plan was ending, a similar one selected by their insurer) was already the lowest-premium plan in their metal tier. This exclusion was necessary to ensure the personalized message was accurate; in a randomized trial, the study arms must be randomly drawn from the same sample, so the exclusion was applied to the whole study sample.

The data included information about the household's maximum potential savings from switching to other plans in their metal tier, accounting for expected tax credits and household structure. We used this value in the "personalized" treatment. The potential savings communicated in the "personalized" treatment were substantial, with a median of $420, a mean of $731, and a range from $50 per year to $10,590 per year.[15] Because these comparisons were limited to plans in the same metal tier, they suggested that consumers could pay significantly less to get the same level of coverage (albeit from a different insurer with potentially different provider networks).

We randomly assigned each household in the study sample to one of three groups (the randomization was performed in Stata and is described in Appendix A).[14,16] The first group, the as-usual group, did not receive either of our information treatments. This group otherwise received the usual messages sent by the Marketplace, including a late October 2015 message to all enrollees announcing the start of open enrollment (see Appendix C for additional information on the non-intervention communications sent to Marketplace enrollees).[14] The second group, the "generic" treatment group, received all of the usual messages sent by the Marketplace plus two letters and two emails that included the generic treatment.[17] The third group, the "personalized" treatment group, received all of the usual messages sent by the Marketplace plus two letters and two emails that included the personalized potential savings message described above. The generic and personalized treatments are displayed in Appendix C.[14] The initial pair of letters and e-mails was sent in the third week of open enrollment, and the subsequent pair was sent in the second to last week before the deadline to enroll for coverage effective January 1, 2016.[18]

Following the open enrollment period, we acquired administrative enrollment and shopping behavior data from the Marketplace in March 2016. The final dataset included information on each household's plan choices from 2014-2016, including tax credits and household structure. It also included measures of shopping activity, like whether the household logged into the Marketplace website and called the call center. This data was used to construct the key shopping and switching outcomes.[19]

Exhibit 1 presents summary statistics on each of the study groups along with comparable statistics for the full population of Marketplace enrollees. Due to the randomization, we observed no meaningful differences between the study groups before the intervention began. However, there are differences between our study sample and the full population due to our sample restrictions that all households must be eligible for auto-renewal and no household could be enrolled in the lowest premium plan in a given tier.

Exhibit 1 illustrates the differences between our sample and the full population of consumers who carried coverage through the Colorado Marketplace. In comparison to the full population, a larger portion of our sample received email, a larger portion of our sample was enrolled in silver plans, and a smaller portion of our sample was enrolled in the lowest cost or the second lowest cost plan in a given metal tier in 2015. This last difference reflects the exit of a low-cost issuer from the Marketplace (removing its former customers from the study sample; we describe the implications of this exit in the Limitations section) and the exclusion of members who were already in the lowest premium plan in their metal tier in 2016.

In addition to these differences, the households in our study sample were slightly more likely than the full population to receive a subsidy; they also received larger subsidies when they received any subsidy at all. The households in our sample chose more expensive plans, again likely reflecting our sample restriction that in order to be eligible for the study, a household could not be enrolled in the lowest premium plan in a given tier.

Analytical Approach

We publicly registered this trial and pre-specified an analysis plan before we observed the post-intervention study data.[20] The plan indicated that the two primary outcomes – the key results by which the study would be evaluated – were whether the intervention induced consumers to shop on the Marketplace website and whether the intervention induced consumers to switch plans. Secondary outcomes included other measures of shopping and plan choice, like whether the consumer called the call center and the premium of the consumer's chosen 2016 plan.

Our approach, as stated in the plan and further described in Appendix A, was to analyze differences in shopping and switching rates between the three study groups.[14] We used linear regression to conduct statistical tests of significance. Linear regression also allowed us to add control variables, which can be helpful even in randomized trials because they can reduce noise (increase the precision with which we estimate our effects); note that in a randomized trial, control variables are not otherwise needed. We report results from our preferred statistical approach using the full set of control variables as we pre-specified in our analysis plan. The controls included all variables used to ensure balance in the randomization process, listed in Appendix B; the study outcome's value in the previous open enrollment period, if available; and variables indicating the consumer's 2015 plan. Appendix B shows that we find the same results with fewer or no controls.[14]

Limitations

The key limitation of our study is that its results may not generalize to other health insurance marketplaces with different patient populations and health plan options. The study population was unique because it excluded two groups of price sensitive Marketplace enrollees. Colorado HealthOP, Colorado's federally subsidized co-op health plan, exited the Marketplace before the 2016 open enrollment period, removing its enrollees from automatic re-enrollment and thus the study sample. HealthOP had often been the lowest premium plan, and many of Colorado's most price sensitive enrollees were likely enrolled in it. We also excluded consumers who

would be auto-reenrolled in the lowest-premium plan in their metal tier because these consumers could not save money by switching plans. The remaining consumers in our study had often deliberately avoided the cheapest plans and may have had strong ties to their existing insurers. Interventions targeting more price-sensitive consumers may show stronger effects, and this topic is an important one for future study.

Another limitation comes from the "choice architecture" used by the Colorado Marketplace in 2015.[21] A consumer shopping on the Colorado marketplace would see information about many plans – sometimes over 60 of them. Decision-support tools such as an out-of-pocket cost calculator were not available.[22] For the consumer, determining which of the many plan options best matched her needs could require a great deal of time and research, potentially weakening the effectiveness of our intervention aimed at getting people to visit the website and choose a new plan.

Relatedly, differences in plan attributes like cost-sharing and provider networks prevented us from offering "choice architecture" improvements in our letters and emails – for example, we did not recommend a specific plan to consumers. We lacked information on the previous health care providers that the consumers utilized and therefore could not recommend a plan on the basis of networks, despite the great potential value of such information. To minimize the concern that the focus on saving on premiums would drive consumers into plans with more deductibles, co-pays, and coinsurance, we limited the comparisons in the interventions to plans in the same metal tier – though some consumers may have benefited from switching to plans in other metal tiers with more (or less) risk protection, depending on their preferences. These consumers may have struggled to ascertain the benefit of switching metal tiers during their shopping experience.

Study Results

Exhibit 2 presents the regression-adjusted primary outcomes for each group. In the first panel, we show the regression-adjusted percent of households that visited the website during open enrollment. While the control group had a website visit rate of 24.9%, we estimate that the generic intervention raised the rate by 5.7 percentage points to 30.6% ($p < 0.01$; 95% CI: [28.8%, 32.4%]), and the personalized intervention raised the rate by 5.8 percentage points to 30.7% ($p < 0.01$; 95% CI: [28.9%, 32.5%]). We find no difference in efficacy between the generic

and personalized treatments. This implies that both treatments increased the rate of shopping on the website by 23%.

In the second panel, we show the regression-adjusted switching rate for each group. Switching is rare in the control group, with only 10.5% of households choosing to enroll in a different plan in 2016.[23] We estimate that neither the personalized nor the generic treatments had any detectable effect on switching. For the generic group the 95% confidence interval ranges from 8.8% to 11.2%, and for the personalized group the 95% confidence interval ranges from 9.8% to 12.2%. Neither intervention yields a significant change in switching, and we can rule out the possibility that the letters caused large increases: our 95% confidence interval rules out an increase of greater than 0.7 percentage points (6% of the baseline rate) for the generic treatment and 1.7 percentage points (17% of the baseline rate) for the personalized treatment.

In addition to estimating the average effect of the treatments on shopping, we estimate the effects for various subgroups of interest. All subgroup analyses are adjusted using the same set of control variables as in the main analysis. For most subgroups, we find no statistically significant differences across groups in the effect of the treatment on shopping (full results are reported in Appendix B).[14] We also find that the treatment effects do not vary with potential savings. However, on two dimensions, age and subsidy status, we do find differences in the treatment effects. These results are presented in Exhibit 3.

The young (people aged less than 50) respond more strongly than the old (50 and older). The generic treatment raised website visits by 4.7 percentage points ($p < 0.01$; 95% CI: [2.3,7.2]) for the old and by 6.6 percentage points for the young ($p < 0.01$; 95% CI: [4.3,8.9]). The difference in the effects between the two age groups was practically meaningful but not statistically significant. The personalized treatment, on the other hand, raised website visits by only 3.9 percentage points for the old ($p < 0.05$; 95% CI: [1.5,6.3]) but by 7.6 percentage points ($p < 0.01$; 95% CI: [5.2,9.9]) for the young, a practically large and statistically significant difference in effects between the age groups ($p < 0.05$).

Next we show the effects of the treatments separately for households receiving subsidies and households with no subsidy. We find that households with no subsidy were much more responsive to the treatments. Specifically, the generic treatment raised website shopping by 9.0 percentage points ($p < 0.01$; 95% CI [6.2,11.7]) for unsubsidized households but only 3.9 percentage points ($p < 0.01$; 95% CI [1.7,6.0]) for subsidized households; the difference in effects between the two subsidy groups was highly significant ($p < 0.01$). Similarly, the personalized treatment increased shopping by 10.5 percentage points ($p < 0.01$; 95% CI [7.8,13.3]) for unsubsidized households and 3.1 percentage points ($p < 0.01$; 95% CI [1.0,5.2])

for subsidized households; again the difference in effects between the two groups was highly significant ($p < 0.01$).

In Appendix B we present the effects of the letters on alternative measures of shopping – calling the call center and whether an enrollee was passively re-enrolled in their 2015 plan via the auto-reenrollment mechanism – as well as effects on the premium of the consumers' chosen plan.[14] In general, the communications appear to raise consumer engagement on the shopping metrics. The effects of the personalized treatment passed the threshold for statistical significance for both of these metrics, while the generic treatment yielded a practically meaningful (but statistically insignificant) effect on passive re-enrollment and no effect on the call center metric. Unsurprisingly, given that the treatments did not yield effects on switching, we also see no effects on plan premiums.

Discussion

In this study we used a randomized controlled trial to test whether letters and emails encouraging Marketplace enrollees to shop and revealing how much money they could save by switching to a lower cost plan affect the shopping and plan switching behavior of these enrollees. The mean amount of potential savings was substantial – $735 per year, or 18% of the average net (after tax credit) premium of $4,042. By comparison, the 43% of existing enrollees in the federally facilitated marketplace who actually switched plans for 2016 saved on average $502 for 12 months.[12] Looking to our primary study outcomes, we found that letters and emails did induce shopping but that personalized potential savings information had no additional effect over generic shopping encouragement. We found that the effect of the interventions on shopping was stronger for younger and unsubsidized consumers, perhaps reflecting easier access to email (one of our primary methods of communication) and a computer with which to visit the website.

While the letters and emails induced consumers to visit the Marketplace website, there was no detectable effect on plan switching or the premium paid. This finding suggests that the individuals induced to shop by the intervention were not further induced to switch.

The lack of an effect on switching from this intervention stands in contrast to the results of a previous randomized intervention for Medicare Part D prescription drug plan enrollees. The study surveyed enrollees about their prescriptions and used that information to provide them with personalized information about their expected savings from switching. That intervention increased switching rates by 11.5 percentage points over the control group level of 17%.[7] Our study differed in the product considered, the population studied, and the details of the

intervention. First, the previous intervention provided the name of the lowest cost plan based on total out-of-pocket costs. In contrast, our intervention did not include such an implicit recommendation and did not address the issue of variation in out-of-pocket costs across plans, though we attempted to hold out-of-pocket costs constant by presenting savings information only for plans with similar levels of cost sharing to those in the consumer's current plan (i.e. the same metal tier). Second, health insurance products are more differentiated than prescription drug products, so Marketplace enrollees who were not in the lowest available price plan may have had strong reasons for choosing more expensive plans. Third, the population examined in the previous intervention may have been more motivated to save money than those examined in our study – participants in the Part D study were willing to disclose their current prescriptions in a phone survey.

Fourth, consumers in our study faced many more plan options than the typical Part D enrollee. Many consumers in Colorado faced over 60 health plan options. Under "choice overload", as this scenario has frequently been called in the literature, consumers may have been induced to shop but found it too difficult or confusing to make an alternative selection. Indeed, evidence from Massachusetts suggests that when the state reduced the number of options by requiring plans to standardize, consumers changed the relative weights they placed on plan attributes, increasing the weight they placed on cost-sharing characteristics (e.g. deductible) relative to premiums and brand. In supplemental analyses, we did not find that the effect of the intervention on plan switching differed depending on the number of issuers offering plans to the consumer. Still, it is possible that a similar intervention would have had stronger effects on plan choice if consumers had to weigh fewer attributes and make fewer comparisons, as would be the case in a state with standardized plans or only a handful of insurers.

The key strength of our study is its gold-standard, randomized design, which yields high quality, statistically unbiased evidence. By randomly assigning Marketplace members to receive as-usual (control) treatment or the generic or personalized messages, we ensured that the only systematic difference between the groups was the kind of messaging that they received. The shopping effects that we observed were far beyond what could be attributed to chance alone, and so we are confident that they were due to the letters and e-mails; the confidence intervals on our estimates indicate that we can also reject the hypothesis that the letters induced many consumers to switch plans.

Conclusion

Our intervention nudged a substantial number of Marketplace enrollees to shop for health insurance rather than passively reenroll. An implication of this finding is that marketplaces can

nudge shopping with relatively little expense and administrative burden, since the generic invitation to shop appears to have been just as effective as the personalized one.

However, we do not find that the intervention led enrollees to switch plans or save on premiums. One interpretation of these results is that enrollees who were induced to shop by our intervention saw the price savings from alternative plans, but were nonetheless happy with their choices. Another interpretation, however, is that the enrollees who shopped as a result of our nudge were unable to determine which plan was better for them, or were unable to follow through with switching intentions due to procrastination, forgetting, choice overload, or status quo bias.[24,25,26]

Additional research on why current enrollees renew more expensive plans is needed to determine how best to communicate with them and facilitate switching Marketplace plans. Given research indicating that individuals in other contexts neither fully understand health insurance nor necessarily choose the best plan for themselves, decision support and choice architecture matters. Finding the right information to communicate to consumers – and the most effective way to communicate it to them – will be an important task for health insurance marketplaces going forward.[27,28,29]

---

1 Handel BR. Adverse Selection and Inertia in Health Insurance Markets: When Nudging Hurts. American Economic Review. 2013 December;103(7):2643–82.

2 Ericson KMM. Consumer Inertia and Firm Pricing in the Medicare Part D Prescription Drug Insurance Exchange. American Economic Journal: Economic Policy. 2014 February;6(1):38–64.

3 Sinaiko AD, Afendulis CC, Frank RG. Enrollment in Medicare Advantage plans in Miami-Dade County: evidence of status quo bias? Inquiry. 2013 Aug;50(3):202–15.

4 Sanger-Katz M. A Rhode Island Rule on Health Enrollment Offers a Consumer Experiment. The New York Times: The Upshot. January 8, 2015. Available from: http://www.nytimes.com/2015/01/08/upshot/a-rhode-island-rule-on-health-enrollment-offers-a-consumer-experiment.html (Accessed July 18, 2016)

5 If the same health plan is no longer being offered, people can be automatically enrolled in a similar plan offered (and chosen) by the insurer.

6 Handel B, Kolstad J. Getting the Most from Marketplaces: Smart Policies on Health Insurance Choice. Washington (DC): Hamilton Project; 2015 Oct. (Discussion Paper 2015-08).

7 Kling J, Mullainathan S, Shafir E, Vermeulen LC, Wrobel MV. Comparison Friction: Experimental Evidence from Medicare Drug Plans. The Quarterly Journal of Economics. 2012;127(1):199–235.

8 Friedman AS, Frank RG. Designed for Decision-Making: Behavioral Experiments in Medicare Plan Choice. Working Paper. 2016.

9 Sanger-Katz M. High Rate of Shopping and Switching in Obamacare Plans Is a Good Sign. The New York Times: The Upshot. February 26, 2015. Available from: http://www.nytimes.com/2015/02/27/upshot/high-rate-of-shopping-and-switching-in-obamacare-plans-is-a-good-sign.html (Accessed July 18, 2016)

10 Connect for Health Colorado. Marketplace Dashboard for December 2015. Available from: http://connectforhealthco.wpengine.netdna-cdn.com/wp-content/uploads/2016/04/Marketplace-Dashboard-2015-December-2015.pdf

11 Department of Health and Human Services. Health Insurance Marketplaces 2015 Open Enrollment Period: March Enrollment Report. ASPE Issue Brief. March 10, 2015. Available from: https://aspe.hhs.gov/sites/default/files/pdf/83656/ib_2015mar_enrollment.pdf (Accessed June 19, 2016)

12 Department of Health and Human Services. Marketplace Premiums After Shopping, Switching, And Premium Tax Credits, 2015-2016. ASPE Issue Brief. April 12, 2016. Available from: https://aspe.hhs.gov/sites/default/files/pdf/198636/MarketplaceRate.pdf (Accessed June 19, 2016)

13 These calculations were taken from the Kaiser Family Foundation's website, "Health Reform Indicators: 2016 Marketplace Open Enrollment Period". Available from: http://kff.org/state-category/health-reform/2016-marketplace-open-enrollment-period/

14 To access the Appendix, click on the Appendix link in the box to the right of the article online.

15 Savings were rounded to multiples of $10. Households that could save less than $50 per year after rounding were excluded from the study. The $731 mean savings in the personalized arm was nearly identical to the $735 mean savings in the full study sample (there were no statistically significant differences in this mean between the arms).

16 The Institutional Review Board of the Harvard T.H. Chan School approved and oversaw this study. With its permission, individuals were not notified that they were taking part in a study; the study materials were reviewed extensively by the study team and the Marketplace to ensure that they were appropriate for the population.

17 Only about 80% of households received emails because not all households provided the Marketplace with an email address. The portion of households receiving emails did not differ across the study groups.

18 Open enrollment 2016 began on November 1, 2015, and the deadline to sign up for coverage for January 2016 was December 15, 2015. For both the generic and the personalized treatment groups the intervention letters were sent on November 20, 2015 and December 4, 2015 and the intervention emails were sent on November 18, 2015 and December 7, 2015.

19 Our measure of visiting the website is an indicator for whether the individual logged into their account. While consumers were able to anonymously “window shop” for plans without

actually logging in, doing so would not provide the consumers with their personalized (net of subsidy) price, and the website encouraged consumers to log in and see the available plans.

20 The study was registered on the AEA Social Science Registry (RCT ID AEARCTR-0000982), available at https://www.socialscienceregistry.org/trials/982

21 Johnson EJ, Hassin R, Baker T, Bajger AT, Treuer G. Can Consumers Make Affordable Care Affordable? The Value of Choice Architecture. PLoS ONE. 2013; 8(12): e81521.

22 A provider search tool was available on the Marketplace website, though it was not displayed prominently.

23 The low rate of switching in the control arm was likely due to the study excluding anyone who was already set to be re-enrolled in the lowest premium plan in her metal tier and because a low-premium insurer exited the Marketplace for 2016, removing its enrollees from the study. Most of the remaining consumers had deliberately avoided low-priced plans in 2015, yielding a group with potentially stronger attachment to their insurers. This issue is also discussed in the Limitations section.

24 Samuelson W, Zeckhauser R. Status Quo Bias in Decision Making. Journal of Risk and Uncertainty. 1988;1(1):7–59.

25 Ericson KMM. Forgetting We Forget: Overconfidence and Memory. Journal of the European Economic Association. 2011;9(1):43–60.

26 Ericson KMM. On the Interaction of Memory and Procrastination: Implications for Reminders, Deadlines and Empirical Estimation. Journal of the European Economic Association. 2016; Forthcoming.

27 Bhargava S, Loewenstein G, Sydnor J. Do Individuals Make Sensible Health Insurance Decisions? Evidence from a Menu with Dominated Options. Cambridge (MA): National Bureau of Economic Research; 2015 May. (NBER Working Paper No. 21160).

28 Loewenstein G, Friedman JY, McGill B, Ahmad S, Linck S, Sinkula S, et al. Consumers' misunderstanding of health insurance. J Health Econ. 2013 Sep;32(5):850–62.

29 Sinaiko AD, Ross-Degnan D, Soumerai SB, Lieu T, Galbraith A. The experience of Massachusetts shows that consumers will need help in navigating insurance exchanges. Health Affairs. 2013 Jan;32(1):78-86.

Exhibits

		Study Sample		
	Full Population	As-Usual Arm	Generic Arm	Personalized Arm
Number of enrollees	58,603	5,169	5,161	5,167
Age of Subscriber	45.44	46.27	46.30	46.31
Household Size	1.44	1.44	1.45	1.44
In Denver County	15%	13%	13%	14%
Receives Email	58%	83%	83%	82%
Smokers	8%	7%	7%	7%
Subsidized HHs (2015)	59%	63%	62%	62%
2015 Subsidy (Subsidized HHs)	$330.28	$341.39	$338.18	$336.22
Gross 2015 Monthly Premium	$493.51	$559.93	$558.31	$553.69
Net 2015 Monthly Premium	$299.40	$345.52	$347.37	$344.03
2015 Plan Tier				
Catastrophic	4%	1%	1%	2%
Bronze	39%	28%	28%	28%
Silver	47%	61%	61%	61%
Gold	9%	10%	10%	10%
Platinum	1%	*	*	*
Enrolled in Lowest Cost or 2nd Lowest Cost Plan in Tier in 2015	32%	6%	6%	6%

EXHIBIT 1 (Table)

Caption: Summary statistics about study sample and overall Marketplace population

Source/Notes: SOURCE Authors' analysis of Connect for Health Colorado enrollment data for open enrollment 2016 NOTES Statistics for the study groups cover the 90% of the sample whose full household could be tracked through the most up-to-date data from the Marketplace (N=14,051). Age was masked for 24% of households to maintain confidentiality, and these households are excluded from the age statistics. Asterisks indicate cells containing fewer than 11 households that were suppressed for confidentiality.

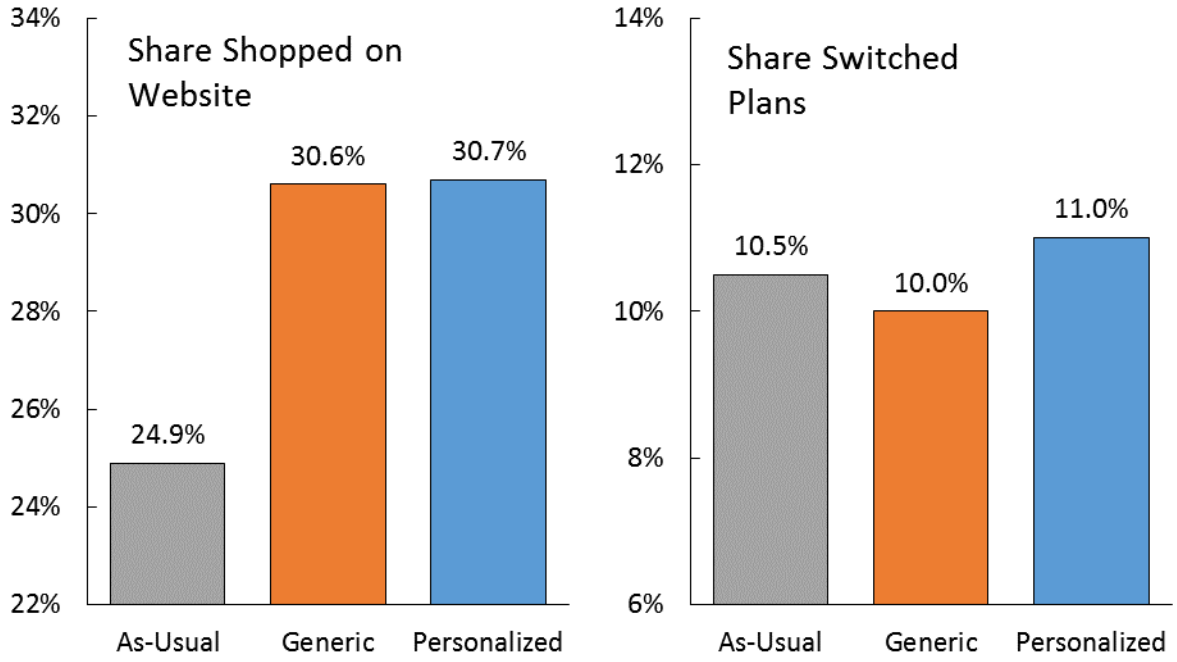

EXHIBIT 2 (Figure)

Caption: Effect of interventions on website shopping and plan switching

Source/Notes: SOURCE Authors' analysis of Connect for Health Colorado enrollment data for open enrollment 2016 NOTES Bars show the average shopping and switching rates for the control group and regression-adjusted rates for the generic and personalized groups.

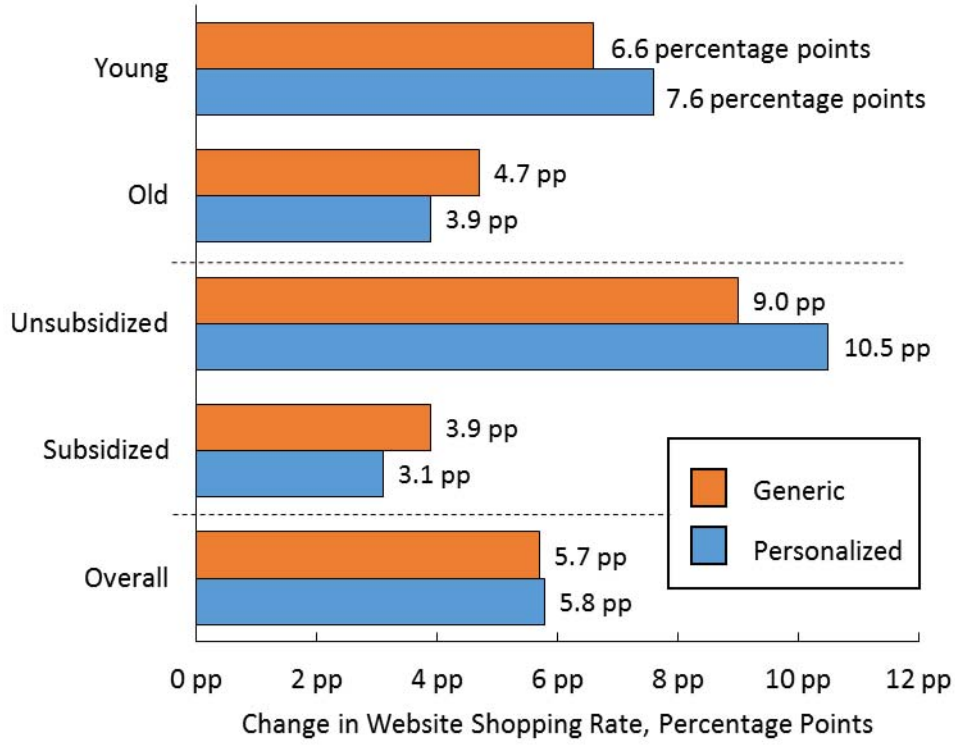

EXHIBIT 3 (Figure)

Caption: Effect of interventions on website shopping for subgroups

Source/Notes: SOURCE Authors' analysis of Connect for Health Colorado enrollment data for open enrollment 2016 NOTES Bars show the regression-adjusted change in the website shopping rate, in percentage points, due to the generic (orange) and personalized (blue) interventions. Young is defined as being less than 50 years of age, while old is 50 years and above. Unsubsidized is defined as anyone who did not receive an Advance Premium Tax Credit (APTC) in 2015; subsidized is anyone who received the credit in that year.

Online Appendix to:
Nudging Leads Consumers In Colorado To Shop But Not Switch ACA Marketplace Plans

Keith Marzilli Ericson, Jon Kingsdale, Timothy Layton, Adam Sacarny

Online Appendix Table of Contents

Online Appendix A – Detailed Description of Data and Intervention

Data and Sample

The data extract we received from Connect for Health Colorado (the Marketplace) included each household's 2015 plan, their 2015 tax credit, their predicted 2016 tax credit, demographics of household members, and auto-reenrollment status. We merged the enrollment dataset with a dataset provided by Wakely Consulting Group that included detailed information about each plan, including gross premium by age, rating area, and smoking status; plan service areas; plan provider networks; and the default 2016 plan choice for the enrollees of each 2015 plan. To construct our sample we started with the population of all renewing Marketplace enrollees who were eligible for auto-reenrollment. We then excluded all households who were slated to be auto-enrolled in the lowest premium plan available to them in 2016, as these enrollees could not save money by switching. We also removed households that could not save at least $45 annually by switching to the cheapest plan in their tier, as messages that communicated only small potential savings were unlikely to trigger shopping and switching.

For each household in our sample, we determined the total annual premium the household would pay if they did not shop and were auto-reenrolled in their 2015 plan (the default plan). We used the Wakely data on plan premiums to determine for each household the total gross premium for the plan. We then applied the 2016 predicted tax credit for the household (provided by the Marketplace) to determine the total net premium for the plan. We repeated this exercise for each plan available to the household. We then calculated the difference between the total net premium for the default plan and the total net premium for the 2016 plan in the same metal tier as the default plan that had the lowest total net premium. We called this difference the household's potential savings from switching, and used this value in the "personalized" treatment presented in panel B of Exhibit 1.

Randomization Process

We randomized in Stata version 14 by stratifying and then randomly allocating subscribers to study arms within each strata. We used a re-randomization procedure to improve the balance in characteristics across the arms. The description of this procedure is quoted from our pre-specified analysis plan (https://www.socialscienceregistry.org/trials/982):

We stratified by constructing 8 categorical variables. Then we used the categorical variables, one by one, to divide and further subdivide the sample into blocks. The procedure was as follows.

We start with blocks for each category of the first variable, then split each of these blocks into sub-blocks along the dimension of the second variable so long as each of the sub-blocks would have at least 6 observations. If 1 or more sub-blocks would have less than 6 observations, the parent block is not split. (If a sub-block would have zero observations, the parent block is still

split – this poses no problem for blocked randomization as the zero observation block needs no randomized allocation.)

We then repeat this process for each of the sub-blocks that were created using the third variable. Blocks that weren't split in the earlier round are not further split.

We continue with the fourth variable and so on until we are out of variables and/or we run out of sub-blocks to continue splitting.

The categorical variables we used, in order, were:

1. Maximum potential savings from switching (5 quantiles)
2. Advance Premium Tax Credit (APTC)/Cost-Sharing Reduction (CSR) (No APTC/APTC, no CSR/APTC, CSR)
3. Rating Area (5 areas, some low-population CO rating areas rolled up into one)
4. Method of notification (E-mail preferred / E-mail then snail mail preferred / Snail mail preferred)
5. Age of subscriber (8 bins, first bin 0-29, then 5-year bins, last bin 60+)
6. Household structure (Single, Couple with no kids/dependents, Single or couple with kids/dependents)
7. Metal tier (Catastrophic and bronze / silver / gold and platinum)
8. Gender of subscriber (male/female)

The result of this process was 418 strata.

The balancing variables we used for re-randomization were:

- Method of notification categories (as above)
- Age of subscriber categories (as above)
- Household structure categories (as above)
- Metal tier categories (as above)
- Gender of subscriber categories (as above)
- Maximum potential savings from switching (included as absolute $ amount)
- Maximum potential savings from switching ($ squared)
- 2016 Projected APTC amount (included as absolute $ amount)

- Rating area categories (indicators for those categories that were rolled up into one for blocking)
- Age of subscriber (in years)
- Age of subscriber (in years squared)
- Household size (in number of members)

Balance in these variables across the three study arms was tested jointly using MANOVA. Each candidate randomization was tested and if the Wilks p-value of differences in means across the arms was less than 0.9999999, the allocation was rejected and a new one was drawn. The 7,727th candidate allocation was the first that passed the cutoff and was used for the study.

Post-Intervention Analysis

We acquired administrative enrollment and shopping behavior data from the Marketplace in March 2016. The data included information on whether each household visited the website or called the call center, the plan the household ultimately chose for 2015, whether the household opted out of auto-reenrollment, and whether the household put a plan in their "shopping cart" on the website but did not complete the enrollment process. The dataset also included the actual realized 2016 tax credit for each household. We merged this enrollment dataset with the pre-open enrollment dataset we used to randomize enrollees to the treatment and control groups. We also merged the datasets with the 2015-16 plan information dataset provided to us by Wakely. Finally, we also acquired a similar administrative enrollment dataset describing each household's enrollment for 2014.

The final dataset included information on each household's plan choice, tax credit, gross premium, net premium, website use, call center use, auto-reenrollment status, shopping behavior, and demographics for 2014, 2015, and 2016. Using this data, we constructed indicators for whether the household visited the website, called the call center, switched plans, or put a plan in their shopping cart. We also constructed a variable describing the amount of money the household saved by switching. These variables formed the primary and secondary outcomes for our study.

We use linear regression to estimate differences in our primary and secondary outcomes across our treatment and control groups. Specifically, we estimated linear regressions of each outcome on indicators for the generic treatment group and for the personalized treatment group. We also performed statistical tests to determine whether the two treatment groups are statistically different from each other and to determine whether being in *either* treatment group was associated with a difference in the outcome.

Online Appendix B – Full Set of Results

To test the effects of our intervention on our primary and secondary outcomes, we use ordinary least squares regressions. We run two sets of three regressions for each outcome variable. In the first set of regressions, we test the effects of the generic and personalized treatment separately. In the second set of regressions, we pool the generic and personalized treatments and test for one treatment effect. Within each set, we include one regression where we control only for the variables we used to ensure balance in the randomization process, one regression where we add a lagged outcome from 2015, and one regression where we add 2015 plan fixed effects. We chose these controls for the following reasons. First, it is customary to control for the variables used to ensure balance in the randomization process. Second, the lagged outcome variable and the 2015 plan fixed effects are powerful predictors of the current outcome, potentially dramatically increasing the power of the statistical test. Because the intervention was randomized, our results represent unbiased estimates of the effects of each treatment. However, controls can improve the power of the statistical analysis.

For the first primary outcome, visiting the Marketplace website and shopping for a plan during the open enrollment period, we also include a set of analyses investigating treatment effect heterogeneity on a number of observables. For these regressions, we also include interactions between the household characteristic and either the generic and personal treatment indicators (set 1) or the pooled treatment indicator (set 2).

Primary Outcomes

Probability of Shopping on Website

Effect of Intervention on Probability of Shopping

	No Controls	2015 Behavior	2015 Behavior and 2015 Plan Fixed Effects
Generic	0.056**	0.057**	0.057**
	(0.009)	(0.009)	(0.009)
Personal	0.057**	0.058**	0.058**
	(0.009)	(0.009)	(0.009)
Joint	0.056**	0.057**	0.058**
	(0.007)	(0.007)	(0.007)
Observations	15497	15497	15497

Standard errors in parentheses
+ p<0.1, * p<0.05, ** p<0.01

Switching

Effect of Intervention on Probability of Switching

	No Controls	2015 Behavior	2015 Behavior and 2015 Plan Fixed Effects
Generic	-0.004	-0.005	-0.005
	(0.006)	(0.006)	(0.006)
Personal	0.006	0.006	0.005
	(0.006)	(0.006)	(0.006)
Joint	0.001	0.001	0.000
	(0.005)	(0.005)	(0.005)
Observations	15497	15497	15497

Standard errors in parentheses
+ p<0.1, * p<0.05, ** p<0.01

Primary Outcome (Website) Treatment Effect Heterogeneity

Old/Young

Effect of Intervention on Probability of Shopping - Old/Young

	No Controls	2015 Behavior	2015 Behavior and 2015 Plan Fixed Effects
Generic	0.044**	0.045**	0.047**
	(0.012)	(0.012)	(0.012)
Personal	0.037**	0.039**	0.039**
	(0.012)	(0.012)	(0.012)
Generic	0.022	0.022	0.019
Young	(0.017)	(0.017)	(0.017)
Personal	0.039*	0.038*	0.037*
Young	(0.017)	(0.017)	(0.017)
Joint	0.041**	0.042**	0.043**
	(0.011)	(0.011)	(0.011)
Joint	0.031*	0.030*	0.028+
Young	(0.015)	(0.015)	(0.015)
Observations	15497	15497	15497

Standard errors in parentheses. Young is <50 years of age. Old is ≥ 50 years of age
+ p<0.1, * p<0.05, ** p<0.01

APTC Status

Effect of Intervention on Probability of Shopping - APTC Status

	No Controls	2015 Behavior	2015 Behavior and 2015 Plan Fixed Effects
Generic	0.092**	0.091**	0.090**
	(0.014)	(0.014)	(0.014)
Personal	0.108**	0.107**	0.105**
	(0.014)	(0.014)	(0.014)
Generic	-0.056**	-0.054**	-0.051**
Subsidized	(0.018)	(0.018)	(0.018)
Personal	-0.079**	-0.077**	-0.074**
Subsidized	(0.018)	(0.018)	(0.018)
Joint	0.100**	0.099**	0.097**
	(0.012)	(0.012)	(0.012)
Joint	-0.068**	-0.065**	-0.063**
Subsidized	(0.015)	(0.015)	(0.015)
Observations	15497	15497	15497

Standard errors in parentheses
+ p<0.1, * p<0.05, ** p<0.01

Carrier

Effect of Intervention on Probability of Shopping - Carrier

	No Controls	2015 Behavior	2015 Behavior and 2015 Plan Fixed Effects
Generic	0.095**	0.097**	0.093**
	(0.023)	(0.023)	(0.023)
Personal	0.075**	0.076**	0.072**
	(0.023)	(0.023)	(0.023)
Generic	-0.088*	-0.088**	-0.084*
COChoice	(0.034)	(0.034)	(0.034)
Generic	-0.035	-0.036	-0.046
Elevate	(0.131)	(0.129)	(0.127)
Generic	-0.026	-0.030	-0.029
Humana	(0.036)	(0.036)	(0.036)
Genric	-0.048+	-0.048+	-0.044+
Kaiser	(0.026)	(0.026)	(0.026)
Generic	-0.028	-0.029	-0.021
Rocky	(0.034)	(0.034)	(0.034)
Generic United	*Suppressed for confidentiality – coefficients not statistically significant*		
Personal	-0.026	-0.030	-0.023
COChoice	(0.035)	(0.034)	(0.034)
Personal	-0.136	-0.135	-0.130
Elevate	(0.139)	(0.136)	(0.138)
Personal	0.016	0.016	0.015
Humana	(0.037)	(0.036)	(0.036)
Personal	-0.020	-0.019	-0.014
Kaiser	(0.026)	(0.026)	(0.026)
Personal	-0.040	-0.038	-0.037
Rocky	(0.034)	(0.034)	(0.033)
Personal United	*Suppressed for confidentiality – coefficients not statistically significant*		
Treatment	0.085**	0.086**	0.082**
	(0.020)	(0.020)	(0.020)
Treatment	-0.056+	-0.059*	-0.053+
COChoice	(0.030)	(0.029)	(0.029)
Treatment	-0.076	-0.076	-0.077

Elevate	(0.115)	(0.112)	(0.113)
Treatment	-0.005	-0.008	-0.007
Humana	(0.031)	(0.031)	(0.031)
Treatment	-0.034	-0.034	-0.029
Kaiser	(0.022)	(0.022)	(0.022)
Treatment	-0.034	-0.033	-0.029
Rocky	(0.029)	(0.029)	(0.029)
Treatment United	*Suppressed for confidentiality – coefficients not statistically significant*		
Observations	15497	15497	15497

Standard errors in parentheses
+ p<0.1, * p<0.05, ** p<0.01

Cost-Sharing Reduction (CSR) Status

Effect of Intervention on Probability of Shopping - CSR Status

	No Controls	2015 Behavior	2015 Behavior and 2015 Plan Fixed Effects
Generic	0.0577***	0.0598***	0.0579***
	(0.011)	(0.011)	(0.011)
Personal	0.0660***	0.0679***	0.0671***
	(0.011)	(0.011)	(0.011)
Generic	-0.0054	-0.0085	-0.0026
CSR	(0.018)	(0.018)	(0.018)
Personal	-0.0234	-0.0256	-0.0239
CSR	(0.018)	(0.018)	(0.018)
Joint	0.062**	0.064**	0.063**
	(0.009)	(0.009)	(0.009)
Joint	-0.014	-0.017	-0.013
CSR	(0.015)	(0.015)	(0.015)
Observations	15497	15497	15497

Standard errors in parentheses
+ p<0.1, * p<0.05, ** p<0.01

Family Structure

Effect of Intervention on Probability of Shopping - Family Structure

	No Controls	2015 Behavior	2015 Behavior and 2015 Plan Fixed Effects
Generic	0.065**	0.065**	0.065**
	(0.017)	(0.017)	(0.017)
Personal	0.063**	0.063**	0.063**
	(0.017)	(0.017)	(0.017)
Generic	-0.013	-0.011	-0.011
Single	(0.020)	(0.020)	(0.020)
Personal	-0.008	-0.006	-0.007
Single	(0.020)	(0.020)	(0.020)
Joint	0.064**	0.064**	0.064**
	(0.015)	(0.015)	(0.014)
Joint	-0.010	-0.009	-0.009
Single	(0.017)	(0.017)	(0.017)
Observations	15497	15497	15497

Standard errors in parentheses. Single is defined as living in a household with one policyholder.
+ p<0.1, * p<0.05, ** p<0.01

Number of Issuers

Effect of Intervention on Probability of Shopping - Number of Issuers

	No Controls	2015 Behavior	2015 Behavior and 2015 Plan Fixed Effects
Generic	0.045*	0.044+	0.046*
	(0.023)	(0.023)	(0.022)
Personal	0.041+	0.039+	0.039+
	(0.023)	(0.023)	(0.023)
Generic	0.001	0.001	0.004
4-5 Issuers	(0.038)	(0.038)	(0.038)
Generic	-0.007	-0.007	-0.008
6-7 Issuers	(0.029)	(0.029)	(0.029)
Generic	0.024	0.027	0.025
8-9 Issuers	(0.026)	(0.026)	(0.025)
Personal	-0.019	-0.016	-0.018
4-5 Issuers	(0.038)	(0.038)	(0.038)
Personal	0.025	0.026	0.024
6-7 Issuers	(0.029)	(0.029)	(0.029)
Personal	0.024	0.029	0.030
8-9 Issuers	(0.026)	(0.026)	(0.025)
Joint	0.043*	0.041*	0.042*
	(0.020)	(0.020)	(0.019)
Joint	-0.009	-0.007	-0.007
4-5 Issuers	(0.033)	(0.033)	(0.033)
Joint	0.009	0.009	0.008
6-7 Issuers	(0.025)	(0.025)	(0.025)
Joint	0.024	0.028	0.027
8-9 Issuers	(0.022)	(0.022)	(0.022)
Observations	15497	15497	15497

Standard errors in parentheses
+ p<0.1, * p<0.05, ** p<0.01

Enrolled in Low Cost Plan in 2015

Effect of Intervention on Probability of Shopping - Low Cost 2015 Plan

	No Controls	2015 Behavior	2015 Behavior and 2015 Plan Fixed Effects
Generic	0.051**	0.053**	0.053**
	(0.009)	(0.009)	(0.009)
Personal	0.060**	0.061**	0.062**
	(0.009)	(0.009)	(0.009)
Generic	0.063+	0.056	0.048
2015 Low Cost	(0.036)	(0.037)	(0.036)
Personal	-0.034	-0.037	-0.049
2015 Low Cost	(0.036)	(0.036)	(0.036)
Joint	0.015	0.010	-0.000
	(0.031)	(0.031)	(0.031)
Joint	-0.023	-0.025	0.023
2015 Low Cost	(0.028)	(0.028)	(0.047)
Observations	15497	15497	15497

Standard errors in parentheses. 2015 Low Cost indicates that the individual was enrolled in the lowest or second lowest cost plan in her metal tier in 2015.

+ p<0.1, * p<0.05, ** p<0.01

Rural/Urban

Effect of Intervention on Probability of Shopping - Rural/Urban

	No Controls	2015 Behavior	2015 Behavior and 2015 Plan Fixed Effects
Generic	0.0598***	0.0610***	0.0606***
	(0.010)	(0.010)	(0.010)
Personal	0.0636***	0.0650***	0.0650***
	(0.010)	(0.010)	(0.010)
Generic	-0.0170	-0.0185	-0.0152
Rural	(0.021)	(0.021)	(0.021)
Personal	-0.0259	-0.0275	-0.0283
Rural	(0.021)	(0.021)	(0.021)
Joint	0.062**	0.063**	0.063**
	(0.008)	(0.008)	(0.008)
Joint	-0.021	-0.023	-0.022
Rural	(0.018)	(0.018)	(0.018)
Observations	15497	15497	15497

Standard errors in parentheses. Rural is defined as living in a county with a Rural-Urban Continuum Code (see http://www.ers.usda.gov/data-products/rural-urban-continuum-codes.aspx) greater than 3.
+ p<0.1, * p<0.05, ** p<0.01

Potential Dollars Saved

Note: this analysis was not pre-specified, but rather was added in response to a suggestion of a referee.

Effect of Intervention on Probability of Shopping – Linear Savings

	No Controls	2015 Behavior	2015 Behavior and 2015 Plan Fixed Effects
Generic	0.054**	0.055**	0.056**
	(0.011)	(0.011)	(0.011)
Personal	0.059**	0.058**	0.059**
	(0.011)	(0.011)	(0.011)
Generic * Savings	0.000	0.000	0.000
(hundreds of $)	(0.000)	(0.000)	(0.000)
Personal * Savings	-0.000	-0.000	-0.000
(hundreds of $)	(0.000)	(0.000)	(0.000)
Joint	0.057**	0.057**	0.057**
	(0.010)	(0.010)	(0.010)
Joint * Savings	-0.000	0.000	0.000
(hundreds of $)	(0.000)	(0.000)	(0.000)
Observations	15497	15497	15497

Standard errors in parentheses. Savings (in hundreds of dollars) is defined as difference between net of subsidy price if the consumer remains in their 2015 plan in 2016 and the net of subsidy price of the lowest premium plan in the consumer's 2015 plan's metal tier.

+ p<0.1, * p<0.05, ** p<0.01

Quartiles of Potential Dollars Saved

Effect of Intervention on Probability of Shopping - Carrier

	No Controls	2015 Behavior	2015 Behavior and 2015 Plan Fixed Effects
Generic	0.071**	0.069**	0.068**
	(0.017)	(0.017)	(0.017)
Personal	0.076**	0.075**	0.073**
	(0.017)	(0.017)	(0.017)
Generic	-0.023	-0.019	-0.019
2nd Quartile Savings	(0.024)	(0.024)	(0.024)
Generic	-0.016	-0.011	-0.007
3rd Quartile Savings	(0.024)	(0.024)	(0.024)
Generic	-0.020	-0.020	-0.017
4th Quartile Savings	(0.025)	(0.025)	(0.025)
Personal	-0.003	0.000	-0.000
2nd Quartile Savings	(0.024)	(0.024)	(0.024)
Personal	-0.040	-0.036	-0.033
3rd Quartile Savings	(0.024)	(0.024)	(0.024)
Personal	-0.033	-0.030	-0.027
4th Quartile Savings	(0.025)	(0.025)	(0.025)
Joint	0.073**	0.072**	0.070**
	(0.015)	(0.014)	(0.015)
Joint	-0.013	-0.009	-0.009
2nd Quartile Savings	(0.020)	(0.020)	(0.020)
Joint	-0.028	-0.023	-0.020
3rd Quartile Savings	(0.021)	(0.021)	(0.021)
Joint	-0.027	-0.025	-0.022
4th Quartile Savings	(0.021)	(0.021)	(0.021)
Observations	15497	15497	15497

Standard errors in parentheses
+ p<0.1, * p<0.05, ** p<0.01

Secondary Outcomes

Passive Reenrollment

Effect of Intervention on Probability of Passive Reenrollment

	No Controls	2015 Behavior	2015 Behavior and 2015 Plan Fixed Effects
Generic	-0.015+	-0.015+	-0.013
	(0.009)	(0.009)	(0.009)
Personal	-0.024*	-0.026**	-0.023*
	(0.009)	(0.009)	(0.009)
Joint	-0.020*	-0.021**	-0.018*
	(0.008)	(0.008)	(0.008)
Observations	15497	15497	15497

Standard errors in parentheses
+ p<0.1, * p<0.05, ** p<0.01

Calling the Call Center

Effect of Intervention on Probability of Calling the Call Center

	No Controls	2015 Behavior	2015 Behavior and 2015 Plan Fixed Effects
Generic	0.001	0.001	0.001
	(0.008)	(0.008)	(0.008)
Personal	0.016*	0.016*	0.015*
	(0.008)	(0.008)	(0.008)
Joint	0.009	0.009	0.008
	(0.007)	(0.007)	(0.007)
Observations	15497	15497	15497

Standard errors in parentheses
+ p<0.1, * p<0.05, ** p<0.01

Net Premium

Effect of Intervention on Probability of 2016 Net Premium

	No Controls	2015 Behavior	2015 Behavior and 2015 Plan Fixed Effects
Generic	2.609	3.033	1.911
	(2.286)	(2.106)	(2.050)
Personal	-3.381	-1.464	-1.449
	(2.389)	(2.212)	(2.157)
Joint	-0.388	0.783	0.232
	(2.034)	(1.874)	(1.819)
Observations	15497	15497	15497

Standard errors in parentheses
+ p<0.1, * p<0.05, ** p<0.01

Online Appendix C – Intervention Materials

Timeline of Intervention

Before our interventions went into the field, the Marketplace engaged in its standard communications with its renewing customers. On October 22, customers were sent an "opening bell" reminder that they would soon be able to re-enroll in their plan for coverage in 2016. This reminder included important dates such as the start of open enrollment, a discussion of how premium tax credits work, and contact information for the Marketplace. Between October 26-29, customers were sent a reminder to re-enroll that was co-branded with their current insurer. This communication included information about the consumer's premium subsidy and the net-of-subsidy price of their 2015 plan in 2016. Our interventions differed from these communications in timing, quantity, and content. Our communications went into the field in November and December, after the Marketplace's standard communications were sent. They were sent twice in the form of email and twice in letter form. Finally, they were focused on encouraging shopping, and the personalized arm also included information about the premiums of other plan options. The table below provides a timeline of the communications received by members of our study arms, including the standard Marketplace communications that were sent to all customers.

Date	Message	As-Usual Arm	Generic Arm	Personalized Arm
October 22	"Opening Bell" Reminder to Re-Enroll	Yes	Yes	Yes
October 26-29	Co-branded Reminder to Re-enroll	Yes	Yes	Yes
November 18 (E-mail) November 20 (Letter)	Initial Intervention Communications	No Message	Generic Savings Message	Personalized Savings Message
December 4 (Letter) December 7 (E-mail)	Second Intervention Communications	No Message	Generic Savings Message	Personalized Savings Message

Intervention Letters

The generic and personalized materials differed in the savings message they provided. The graphic below shows excerpts of the two messages one after the other.

Generic Letter

COMPARE 2016 PREMIUMS and you will see some big differences across plans.

YOU MAY BE ABLE TO SAVE ON PREMIUMS IF YOU SWITCH
With the options available to you in 2016, there's a great chance you will be able to save by switching plans.

Personalized Letter

COMPARE 2016 PREMIUMS for all plans offered at your current coverage level (Platinum). You will see some big differences: you could save as much as $3000 compared to your current plan's 2016 premium.

YOU CAN SAVE ON PREMIUMS IF YOU SWITCH
With the options available in the Platinum tier for 2016, next year you could:

⇩ **SAVE $3000 with the LOWEST PREMIUM plan**

57% OF PLANS in your tier have LOWER PREMIUMS

On the following pages, we present samples of the full first page of the letters that were sent in the study.

Connect for Health Colorado
Individual Customer Support
Suite 123
Colorado Springs, CO 81234

CONNECT for HEALTH COLORADO

Mr. William Whiskers
456 Fake Street
Suite 234
Boulder, CO 11111

November 13, 2015

STAY COVERED IN 2016!

Compare new plans and prices now

It's time to renew your health insurance for 2016! You have until December 15 to enroll if you want your plan to start in January, and you can get free, in-person help from one of our certified Brokers or Health Coverage Guides to find the plan that's right for you. Unless you shop for a different plan by December 15, we will automatically renew your current coverage. So visit ConnectforHealthCO.com or call 1-855-752-6749 to check out your options today.

IF YOU DO NOTHING, you will be automatically re-enrolled in your current plan, and your premiums may go up.

COMPARE 2016 PREMIUMS and you will see some big differences across plans.

YOU MAY BE ABLE TO SAVE ON PREMIUMS IF YOU SWITCH

With the options available to you in 2016, there's a great chance you will be able to save by switching plans.

VISIT CONNECTFORHEALTHCO.COM OR CALL 1-855-752-6749 TO SHOP AND COMPARE TODAY!

Generic Arm – Initial Letter

Connect for Health Colorado
Individual Customer Support
Suite 123
Colorado Springs, CO 81234

CONNECT for HEALTH COLORADO

Mr. William Whiskers
456 Fake Street
Suite 234
Boulder, CO 11111

November 13, 2015

STAY COVERED IN 2016!

Compare new plans and prices now

It's time to renew your health insurance for 2016! You have until December 15 to enroll if you want your plan to start in January, and you can get free, in-person help from one of our certified Brokers or Health Coverage Guides to find the plan that's right for you. Unless you shop for a different plan by December 15, we will automatically renew your current coverage. So visit ConnectforHealthCO.com or call 1-855-752-6749 to check out your options today.

IF YOU DO NOTHING, you will be automatically re-enrolled in your current plan, and your premiums may go up or down.

COMPARE 2016 PREMIUMS for all plans offered at your current coverage level (Platinum). You will see some big differences: you could save as much as $3000 compared to your current plan's 2016 premium.

YOU CAN SAVE ON PREMIUMS IF YOU SWITCH

With the options available in the Platinum tier for 2016, next year you could:

⇩ **SAVE $3000 with the LOWEST PREMIUM plan**

57% OF PLANS in your tier have LOWER PREMIUMS

VISIT CONNECTFORHEALTHCO.COM OR CALL 1-855-752-6749 TO SHOP AND COMPARE TODAY!

Personalized Arm – Initial Letter

Savings Message for Personalized Arm

The message at the bottom of the personalized letter was varied depending on the number of plans in the customer's metal tier that had lower premiums. If at least half the plans in the metal tier had lower premiums, the following message was displayed:

If fewer than half the plans in the metal tier had lower premiums, but still at least 2 plans had lower premiums, the following message was displayed:

Finally, if only one plan in the tier had lower premiums, the following message was displayed: