The Role of Behavioral Frictions in Health Insurance Marketplace Enrollment and Risk: Evidence from a Field Experiment

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Online Appendix

A1. Sample Exclusions and Comparison of Study Sample to Uninsured and Covered California Enrolled Populations

This section summarizes sample exclusions and presents descriptive statistics on the final study sample, and as a comparison, the 2015 Covered California-enrolled and uninsured populations in California.[1]

A1.1 Sample Exclusions:

As noted in Section 3, the total size of the Funnel prior to open enrollment was 153,146 households of which 64 percent were County Referrals, and 36 percent Open Enrollment Applicants. For budgetary reasons, we reduced the total sample to 126,182 randomly selected households from the full Funnel to be in the study.[2] These households were then randomized into the 5 study arms using the method described in Section 3.4. Since the time of the treatment randomization, we became aware that some households were not eligible to enroll in Covered California, or did not have valid addresses. We excluded these households to create the final study sample. Because treatment assignment was random, these *ex post* exclusions have an identical effect on all study arms in expectation. We report balance tests within this final sample in Section 3.4.

First, we excluded households for whom administrative data reported invalid ages for any member, as invalid ages would have led to incorrect or missing premiums reported in subsidy-reporting letters.[3] Next, we excluded households who had incomes below 100 percent of FPL. These households were generally ineligible for subsidies in ACA exchanges, and hence were unlikely to enroll in an exchange

[1] Data on the uninsured come from the IPUMS (Ruggles, *et al,* 2017) version of the American Community Survey (ACS). We restrict the full ACS to those that are flagged uninsured at the time of interview, not institutionalized, and have incomes above 100 percent FPL.

[2] The 126,182 households were randomly selected in two phases. To guarantee sufficient time to compute subsidies and print personalized letters for a sufficient sample by the deadline, we randomly selected 100,000 households from the Funnel sample as of one month before the enrollment deadline. From the households who entered the Funnel over the following two weeks, we randomly sampled (26,182) additional households until we exhausted our budget. Note, because later entrants to the Funnel had higher baseline enrollment, take-up rates for the "Initial Budgetary Exclusion" group are slightly higher than that of the initial Funnel sample ("All"), reported in Appendix Table 1.

[3] Enrollee ages are based on year of birth. Specifically, we excluded 0.5% of households with any member that was 100 years or older, or in very rare instances had a negative reported age.

plan. We also dropped households that the postal service reported as having moved before the experiment, and for whom we did not have a current mailing address. Finally, we excluded County Referral households who were deemed ineligible for subsidies.[4]

The final sample size after applying these exclusions is 87,394 households. These exclusions and their impact on the sample size are reported in Appendix Table 1. Although these exclusions were made after the initial randomization, their impact on each study arm is the same in expectation.[5]

A1.2 Comparison of Study Sample to Other Populations

Table 1 displays demographic summaries for the RCT study population, the Covered California population, and the population of uninsured individuals in California. The average age in the RCT study sample is 37.7 years old, younger than the Covered California population (43.9) but similar to the uninsured (37.3). Appendix Figure 1 displays the full age distributions, and suggests that the age profile of the study sample is more similar to the uninsured than to the Covered California population.

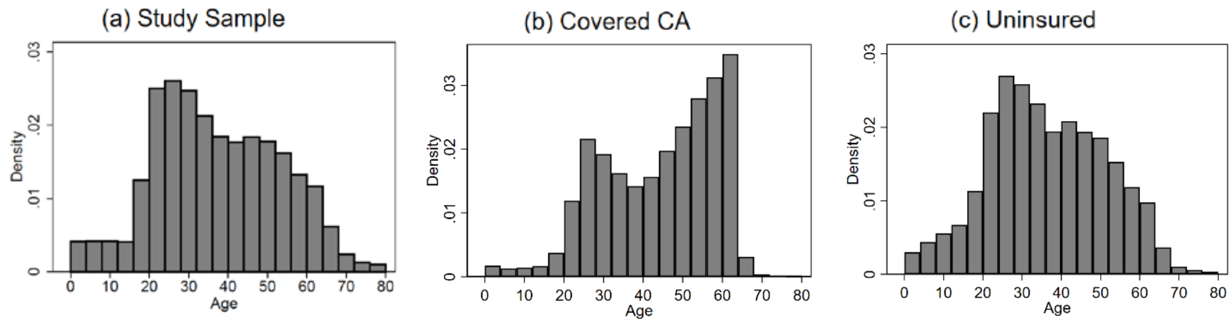
The average income in the study sample is 212 percent of FPL.[6] This is slightly higher than incomes of Covered California (204 percent) enrollees and lower than the uninsured (217 percent). The distribution of race in the study sample also resembles that of the uninsured population. Overall, these statistics suggest that the Funnel population resembles the uninsured, but given their expressed interest in the marketplace, may be slightly more likely to take up insurance than the overall uninsured population in subsequent years. Below, in Appendix Section A6, we provide a more detailed comparison of the RCT sample to other populations for the purpose of assessing generalizability of the RCT results.

[4] After implementing the original intervention, it was determined in consultation with state program administrators that many of these consumers were simultaneously being evaluated for other Medicaid coverage options that existed prior to the ACA. For those who qualified–which would have resulted in the consumers being found ineligible for marketplace subsidies–these programs were more financially beneficial than purchasing unsubsidized plans through Covered California.

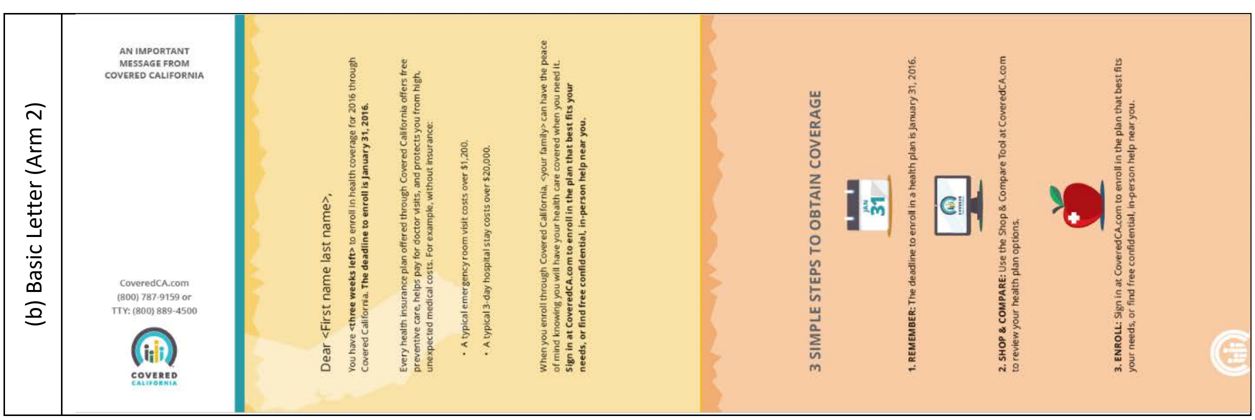
[5] We also replicate all analyses using the full post-randomization pre-exclusion sample of 126,182. As expected, we find that control group take-up is slightly lower in this sample than in the final study sample, given the inclusion of households who are unlikely to take-up; but estimated treatment effects and patterns of heterogeneity are nearly identical to those observed for the final study sample. Results are available upon request.

[6] FPL information is missing for some households with incomes above 400 percent of FPL, so we restrict estimates of average incomes, here, to households with reported income less than 400 percent of FPL. Households with incomes above 400 percent of FPL are ineligible for subsidies, and did not need to provide their income on the application, resulting in missing incomes for some of these households.

Appendix Figure 1. Age Distribution, By Populaton Segment



A2. Intervention Letter Templates



(c) Subsidy-Penalty Letter (Arm 3)

AN IMPORTANT MESSAGE FROM COVERED CALIFORNIA

CoveredCA.com
(800) 787-9159 or
TTY: (800) 889-4500

COVERED CALIFORNIA

Dear <First name last name>,

You have **<three weeks left>** to enroll in health coverage for 2016 through Covered California. **The deadline to enroll is January 31, 2016.**

Every health insurance plan offered through Covered California offers free preventive care, helps pay for doctor visits, and protects you from high, unexpected medical costs. For example, without insurance:

- A typical emergency room visit costs over $1,200.
- A typical 3-day hospital stay costs over $20,000.

When you enroll through Covered California, <your family> can have the peace of mind knowing you will have your health care covered when you need it.

Based on your information, we estimate that:

WITH HEALTH COVERAGE:

<You will likely receive **<$1250>** in tax credits in 2016. This reduces the cost of a Silver plan by about <$> per month, or about <40%>.>

WITHOUT HEALTH COVERAGE:

You may pay an IRS Tax Penalty of **<$600*>**.

<*Penalty estimate based on similar households in your area.>

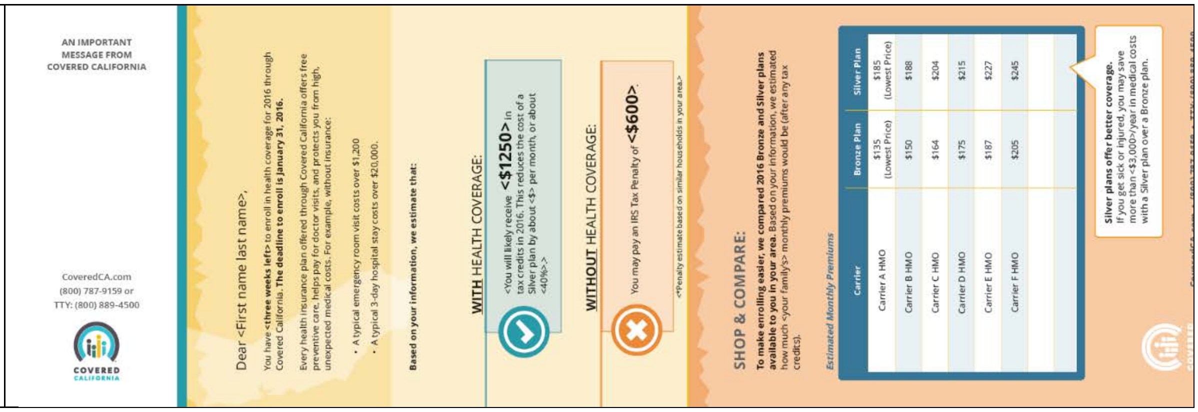
3 SIMPLE STEPS TO OBTAIN COVERAGE

1. REMEMBER: The deadline to enroll in a health plan is January 31, 2016.

2. SHOP & COMPARE: Use the Shop & Compare Tool at CoveredCA.com to review your health plan options.

3. ENROLL: Sign in at CoveredCA.com to enroll in the plan that best fits your needs, or find free confidential, in-person help near you.

(d) Price Compare (Arm 4)



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WITHOUT HEALTH COVERAGE:

You may pay an IRS Tax Penalty of **<$600>**.

<*Penalty estimate based on similar households in your area.>

SHOP & COMPARE:

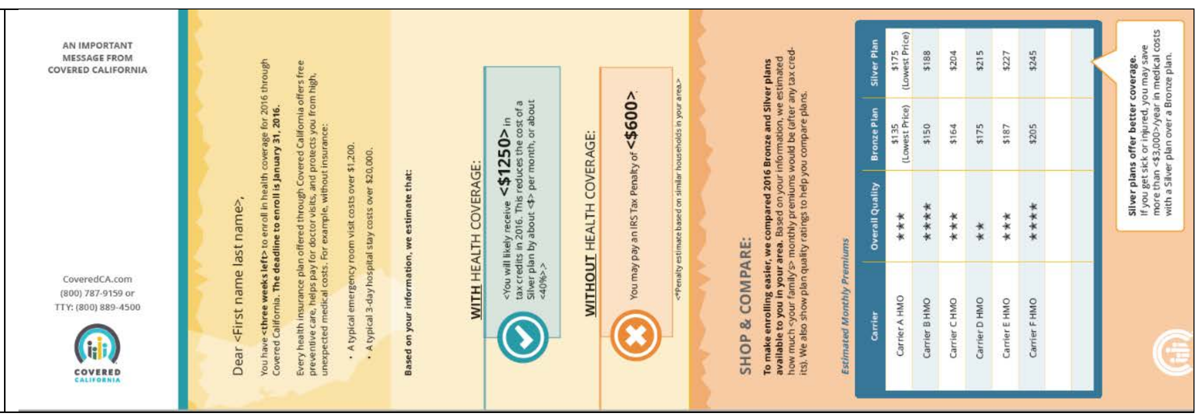
To make enrolling easier, we compared 2016 Bronze and Silver plans available to you in your area. Based on your information, we estimated how much <your family's> monthly premiums would be (after any tax credits).

Estimated Monthly Premiums

Carrier	Bronze Plan	Silver Plan
Carrier A HMO	$135 (Lowest Price)	$185 (Lowest Price)
Carrier B HMO	$150	$188
Carrier C HMO	$164	$204
Carrier D HMO	$175	$215
Carrier E HMO	$187	$227
Carrier F HMO	$205	$245

Silver plans offer better coverage. If you get sick or injured, you may save more than <$3,000>/year in medical costs with a Silver plan over a Bronze plan.

(e) Price/Qual Compare (Arm5)



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- A typical emergency room visit costs over $1,200.
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Based on your information, we estimate that:

WITH HEALTH COVERAGE:

<You will likely receive **<$1250>** in tax credits in 2016. This reduces the cost of a Silver plan by about <$> per month, or about <40%>.>

WITHOUT HEALTH COVERAGE:

You may pay an IRS Tax Penalty of **<$600>**.

<*Penalty estimate based on similar households in your area.>

SHOP & COMPARE:

To make enrolling easier, we compared 2016 Bronze and Silver plans available to you in your area. Based on your information, we estimated how much <your family's> monthly premiums would be (after any tax credits). We also show plan quality ratings to help you compare plans.

Estimated Monthly Premiums

Carrier	Overall Quality	Bronze Plan	Silver Plan
Carrier A HMO	★★★	$135 (Lowest Price)	$175 (Lowest Price)
Carrier B HMO	★★★★	$150	$188
Carrier C HMO	★★★	$164	$204
Carrier D HMO	★★	$175	$215
Carrier E HMO	★★★	$187	$227
Carrier F HMO	★★★★	$205	$245

Silver plans offer better coverage. If you get sick or injured, you may save more than <$3,000>/year in medical costs with a Silver plan over a Bronze plan.

A3. Heterogeneous Treatment Effects by Health Spending Risk

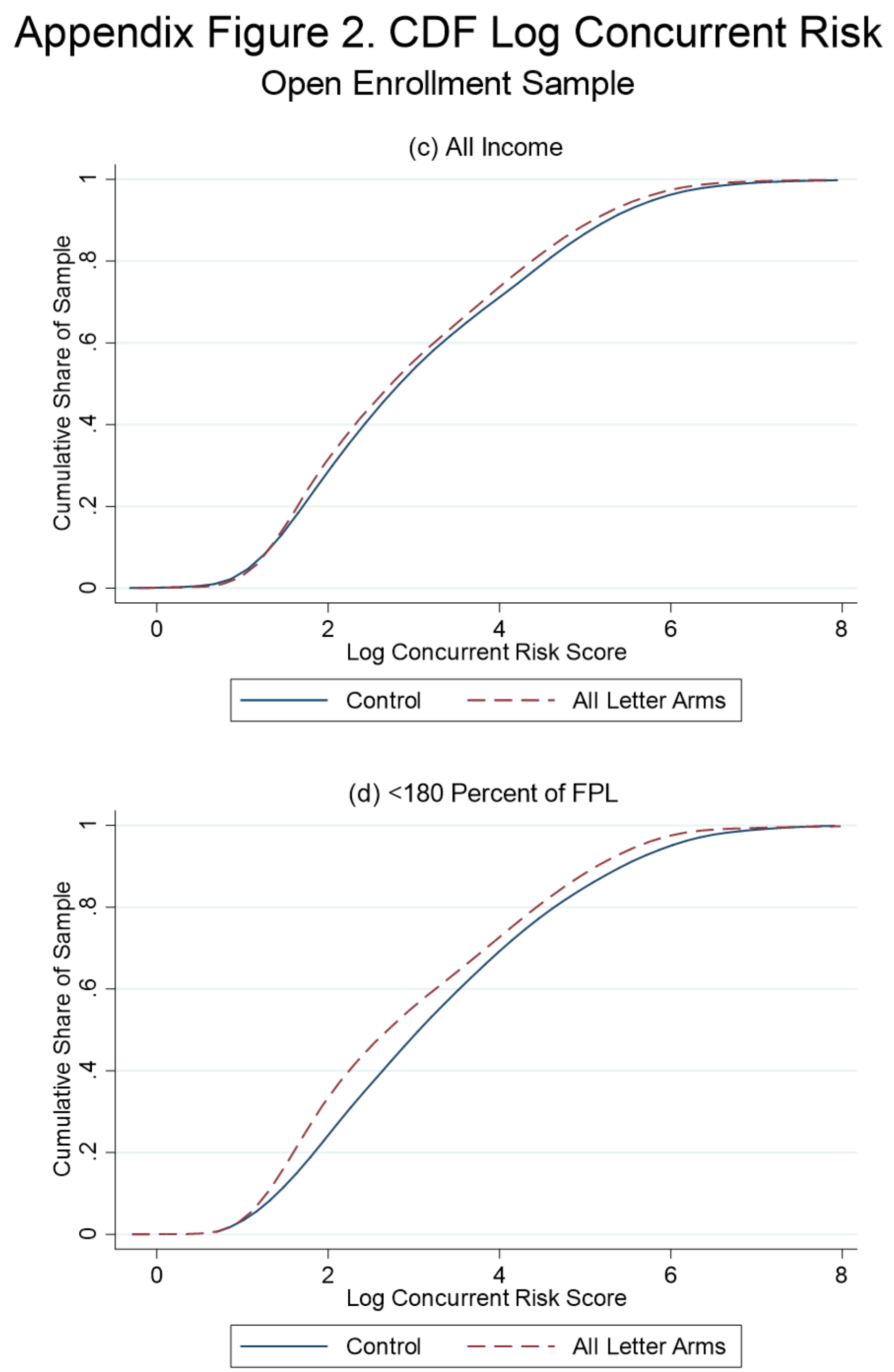
To estimate the differential take-up by health risk, we estimate equation (1) including interactions between treatment assignment and log health spending risk. Results are reported in Appendix Table 5. All specifications control for region and interactions between treatment arms and the consumer age rating factor used for age-based premium pricing. Thus, any heterogeneous take-up risk will represent selection on spending risk that plans are unable to price in.

Four patterns are evident. First, the treatment effects are markedly stronger for healthier consumers. Second, the differential treatment effect among healthier consumers is concentrated in the Open Enrollment sample, who are generally aware of the existence of health plan and premium subsidies on the Exchange. Consistent with a simple adverse selection model with behavioral frictions, sicker Open Enrollment applicants with higher demand for coverage may have already incurred the frictional costs associated with shopping and enrolling; and by lowering these frictions, the letters may have disproportionately induced marginally healthier consumers into the market. By contrast, if the County Referral applicants were typically less aware of marketplace options and had a lower overall baseline take-up rate, the intervention may have induced both healthy and sick consumers into the market.

Third, the positive health selection effects of the letter intervention are concentrated among lower income Open Enrollment consumers. This suggests that lower income individuals may face greater hassle cost and frictions associated with remembering to enroll, choosing a plan, and enrolling by the deadline. If so, letters that reduce these frictions would magnify the overall enrollment effects among healthier consumers.

Finally, we examine whether the differential treatment effect by health risk is explained by heterogeneous treatment effects along observable dimensions that plans are permitted to price in—namely, age factors and region. If, for example, the treatment selection effects are embodied in differential treatment effects by age, then much of the healthier risk response would be reflected in the lower premiums received by plans for younger enrollees. To examine this, we repeat the regression specifications of Appendix Table 5, but do not include the interactions between treatment arm and the consumer age rating factors used for age-based premium pricing. Results are reported in Appendix Table 6. Dropping the age rating factor controls leaves the estimated coefficients on the interaction terms between treatment and baseline risk largely unchanged, implying that the vast majority of the positive risk selection induced by the interventions is not explained by differential take-up by age, but rather by unpriced health risk conditional on age.

A4. The Distribution of Health Spending Risk



Appendix Figure 2 depicts the cumulative distribution function of log concurrent risk scores among enrolled consumers, by control and treatment assignment. Panel (a) is restricted to the Open Enrollment sample. Panel (b) is restricted to households with income <180 percent of FPL within the Open Enrollment sample.

A5: A Simple Model of Frictions and Imperfect Information in Insurance Demand

In this section, we discuss a simple model that supplements the intuition outlined in Section 5.4 of the main paper. The goal of the model here is to introduce a framework to think about the value of the letters and how different frictions can bias measurements of WTP. We do not attempt to estimate this model, but rather use it to motivate the estimated model in Section 5.4. The model is an augmented discrete choice model to consider the effect of enrollment frictions (namely remembrance costs), as well as incomplete information. To handle incomplete information and learning, we adopt a framework similar to that of Bayesian learning models—e.g. as in Chernew, Gowriskankaran, and Scanlon (2008).

Consider household i with the option of enrolling in a representative insurance plan.[7] If the household enrolls, they must face some enrollment costs. As an illustration, we focus only on the mental costs associated with remembering to enroll as examined in this paper (call this $c_i^{Remember}$), but it could just as well be other frictions more directly tied to the enrollment process itself (e.g. collecting and entering information for the application, assessing plan options, etc.); and moreover, assume that the household is perfectly informed about all plan characteristics and prices, but not the subsidy level for which they are eligible.[8] We denote the value of the plan (relative to being uninsured and the value of uncompensated care, etc.) as δ_i.[9] The disutility of premiums is $U_i^{premium}$.[10] The Information set for the household is denoted by I_i and the perceived monthly subsidy (ATPC) given that information is $E(\tau_i|I_i)$. We use this notation to highlight that perceived subsidies can change with new information, such as that included in our letter interventions. Combining the above, the expected indirect utility u_i of enrolling in the plan relative to remaining uninsured is:

(A) $$u_i = \delta_i - U_i^{premium} - c_i^{Remember} + \alpha_i E(\tau_i|I_i) + \varepsilon_i$$

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[7] For the purpose of this exposition, having one plan is sufficient to demonstrate key mechanisms. In reality, each household is given a menu of plans from which to choose. This model could be augmented to include more plans, but it would complicate notation with little added value to communicating the main mechanisms of interest. This paper focuses on take-up effects, so our primary margin of interest is whether or not to enroll in any plan.

[8] One could add learning about plan characteristics (e.g. as targeted by our treatment arms 4 and 5), using the same approach as we use for subsidies.

[9] As noted in the above footnote, we could have considered take-up of any of a set of plans offered—more realistic to our setting. In this case, δ_i more accurately can be thought of as expected value of the "most preferred" of all plans—analogous to the "inclusive value" derived from a plan choice framework ($E[\max\{u_{ik}\}]$, where u_{ik} is the utility of each individual plan). This is how we think about this value when going to the reduced-form version of this model presented in the paper. Note with this interpretation, we assume additive separability in the utility of plans and plan premiums, which is a reduced-form representation of the true indirect utility of the bundled choice set.

[10] If subsidies and premiums had an identical impact on choice, this would be $\alpha_i * premium_i$

ε_i is the idiosyncratic utility of having insurance and is centered at 0. The household takes up insurance if the indirect utility u_i exceeds 0.

Equation (A) departs from a canonical specification of indirect utility in two primary ways: first, we include $c_i^{Remember}$ to reflect the mental cost of remembering to enroll, which is required by all households choosing to enroll (but can differ). This friction is targeted explicitly by the Basic Reminder letter, the content of which is included in all intervention letters. The second difference is that $E(\tau_i|I_i)$ captures *perceived* subsidies, not actual subsidies τ_i. In principle, other frictions could affect take-up—e.g. the hassle cost of enrolling in a plan—but are not explicitly modeled here.[11]

The true value in utils of insurance in this model is the term δ_i. Since $\alpha_i$is the value of perceived subsidy dollars in utils, the dollar valuation for insurance, or "willingness-to-pay" (WTP), could be calculated as δ_i/α_i, using unbiased estimates of δ_i and α_i. Using a revealed preference approach, one could try to estimate this object from the observable data. However, there are two reasons that measurement of the WTP using common revealed preference methods might be biased due to the frictions explored in this study. First, to the extent that $c_i^{Remember}$ must be paid to enroll, standard estimation techniques will include $c_i^{Remember}$ as part of δ_i, since it is generally not observable and not separately identified. Hence, the WTP will generally be calculated as $(\delta_i - c_i^{Remember})/\alpha_i$. If this cost $c_i^{Remember}$ is nonzero, WTP calculations with this approach will be biased downward from the true value of insurance. In short, the *measured* value of insurance will generally be net of costs of enrolling, such as mental costs of remembering. If enrollment costs are high, consistent with evidence from this study, then value of insurance will likely be measured to be too low.

The second reason that common estimation methods could lead to biased measurement of WTP is in the estimation of α_i. If changes in perceived subsidies $E(\tau_i|I_i)$ are not equal to the actual changes (or whatever is observable in the data and used in estimation), then α_i will not be a correct conversion of utils to dollars. At the extreme, if price changes are not perceived at all, then consumers are not at all responsive to prices, and the WTP for insurance will approach infinity—i.e. upward biased from the actual WTP. On the other end, if consumers overestimate price changes (e.g. if they think subsidy

[11] Consumers may face additional frictions and search costs. For example, consumers may face high hassle costs of enrolling in a plan, apart from remembering to enroll and to compare plans and obtaining true prices. As our interventions do not specifically target the ease of enrolling, we do not explicitly model that here, but this framework could be adapted to include enrollment hassle costs. For this reason, the estimated value of our letters can also be thought of as a lower bound on the dollar denominated cost of various frictions associated with the enrollment process.

gradient is steeper than in reality), then the estimation will yield a high value of α_i and hence a low value for δ_i/α_i. In this case, WTP will be downward biased. In summary, misperceptions about prices and subsidies can have an ambiguous effect on the empirical estimation (and bias) of WTP.

Without accounting for behavioral frictions, an econometrician using demand estimation to measure WTP for available plans would confound underlying WTP for insurance with the effects of these behavioral frictions. Distinguishing between an environment where WTP is low and one where WTP is higher but made artificially lower due to the presence of frictions leads to different policy implications.

Finally, a major object of interest in the paper is the "value" of the letters. In the paper, we use an approximation of value which is "the subsidy equivalent effect." However, this model presented here—if estimated—could be used to calculate a more formalized "value" in welfare equivalent units, for example, as done in Chernew, Gowriskankaran, and Scanlon (2008).

A6. Projecting RCT Treatment Effects to Broader Populations

We consider a hypothetical expansion of the intervention to several populations that a marketplace could conceivably target: a) all uninsured consumers (say, through a potential collaboration with a tax authority charged with administering a state mandate); b) any consumer who enters the year-long Funnel, during open enrollment or any time during the year after becoming enrollment-eligible due to a qualifying-event (e.g. divorce, change in immigration status, loss of previous coverage, etc.), whether by active shopping or county referral; c) the subset of this year-long Funnel who do not enroll in a plan after a number of days, as defined by policy-makers.

Appendix Table 9 reports summary demographic statistics for the RCT Funnel sample, Covered California's enrollee population, sample estimates of California's uninsured population, and two subsets of the year-long Funnel population. Consumers in the "3-day" Funnel enter the Funnel but have not enrolled in a plan after three days. The "10-day" Funnel is analogously defined. Naturally, the selection of consumers in a Funnel defined by the shorter period will have higher take-up, potentially indicating the inclusion of consumers with higher unobserved demand for insurance, awareness of the market, or lower frictions. For purposes of generalizability, our target population would ideally have a similar take-up rate as the control arm of the RCT sample. As discussed below, we calibrated the definition of the Funnel such that the resulting marketplace take-up rate equals the take-up rate in our RCT control sample. In this way, we define a population to whom our RCT results may generalize more reliably. Doing so, we arrived at the 10-day Funnel.[12]

Along age, race, and income, the RCT sample appears similar to the uninsured and the 3- and 10-day Funnel populations. The two Funnel populations also have roughly similar share of County Referral consumers as the RCT control sample. However, the two Funnel populations differ in their eventual marketplace take-up rates, a potential indicator of unobserved demand for insurance and awareness of marketplace coverage. Defined using a shorter period, the 3-day Funnel has a marketplace take-up rate of 11 percent, higher than the 8.1 percent in the RCT control sample, raising doubts as to the generalizability of the RCT results to this population. As discussed above, the 10-day Funnel was arrived at by calibrating the definition of the Funnel to obtain take-up rates matching the 8.1 percent take-up observed in the RCT control arm. We therefore use the 10-day Funnel as the basis of projecting a hypothetical expanded informational letter intervention. We also considered using the uninsured population for purposes of the projection, given its similarity to the RCT sample along income and demographic characteristics. But lacking information on eventual take-up rates, we felt that such an exercise would be too speculative.

Back-of-the-envelope projections for the post-intervention market-wide average health spending risk is calculated as a weighted average across:

$$\text{(B)} \qquad Average\ Risk^{Post} = \frac{s_F(\beta_e)\cdot R_M R_F + s_F \cdot R_F + (1-s_F)\cdot 1}{1+s_F(\beta_e)}$$

where s_F is the pre-intervention share of the Covered California insured market originating from the 10-day Funnel, β_e is RCT treatment effect on enrollment, R_M is the average risk of marginal enrollees who enroll in response the intervention (relative to the average risk of enrollees originating from the 10-day Funnel targeted by the intervention), R_F is the relative risk of enrolled consumers originating from the 10-day Funnel (relative to the average risk of enrollees in the rest of the marketplace). Average risk of the market is the post-intervention enrollment share-weighted average risk across marginal respondents from the 10-day Funnel, inframarginal enrollees originating from the 10-day Funnel, and

[12] From our conversations with Covered California, we also think it could be operationally difficult for an exchange to implement a paper mail intervention in fewer than 10 days.

the rest of the enrollees in the market. The risk of marginal enrollees, R_M, can be determined from parameters identified in the RCT.[13]

The average market-wide risk absent an intervention is given by $Average\ Risk^{Pre} = s_F \cdot R_F + (1 - s_F)$. Comparing $Average\ Risk^{Post}$ to $Average\ Risk^{Pre}$ identifies the impact of the intervention on market-wide average risk.

Administrative data shows that about 4 percent of Covered California's covered member-months in any year originate from the 10-day Funnel (in any one year, $s_F = 0.04$). But among the 96 percent that do not originate from any one year's Funnel, a large fraction are renewals who originated a previous year's Funnel. Under the assumption that renewal and attrition rates among enrollees who do and do not originate from the Funnel are similar, then the steady-state share of Covered California's member-months of coverage is the share of *new* enrollees in any given year originating from the 10-day Funnel, which for the 10-day Funnel definition is 14 percent (in steady state, $s_F = 0.14$).[14]

Our RCT suggests that $\beta_{Take-up} = 0.16$, and $\beta_{Risk} = 0.051$ to 0.072—that a similar information intervention would generate a 16 percent increase in enrollment, and cause average risk of enrollees from the 10-day Funnel to fall 5.1 percent, or 7.2 percent if mailings were optimized using the intervention (i.e. the subsidy + plan comparison letter) that generated the largest risk impacts, especially for lower income recipients. This implies marginal enrollees that are 37 to 52 percent healthier than inframarginal enrollees in the treated Funnel. Analysis using full administrative data from Covered California show that expected health risk among enrollees originating from the year-long Funnel is 7.8 percent *higher* than the rest of the marketplace. However, the prospective nature of the CDPS risk score, and comparing across two populations with different pre-period insurance and cost-sharing, suggests these differences may not reflect well differences in realized health spending.[15] Nevertheless, we conservatively assume R_F = 1.078.

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[13] $R_M = \frac{(1-\beta_r)(1+\beta_e)-1}{\beta_e}$ captures the risk of marginal enrollees to the letter intervention, relative to the RCT control group (in this context, the consumers in the 10-day Funnel), where β_r is the reported experimental treatment effect on the average risk of marginal and inframarginal enrollees in the treated population, reported in Table 5.

[14] The share of new enrollees each year is about 28.5 percent. To apply the steady state share to equation (B) requires making an additional assumption that the RCT treatment effects are independent of renewal and attrition decisions. Unfortunately, administrative data for the study years were not structured to test this. In principle, administrative data on Funnel status could be linked across years to renewal behavior to model explicitly steady state share of enrollees originating from the Funnel.

[15] Enrollees originating from the Funnel show higher risk despite having lower average age than the rest of the enrollees. This is primarily driven by a much higher fraction of Funnel consumers (and eventual enrollees from the Funnel) having at least one outpatient OSHPD encounters (relative to all other enrollees in Covered California,

Applying the RCT treatment effects on take-up and average risk, we project that an expanded intervention would lead to a 0.6 percent increase in total enrollment, and a 0.2 to 0.3 percent reduction in market-wide risk in the first year; and in steady state, the intervention would lead to a 2.4 percent increase in total enrollment, and a 0.7 to 1.1 percent reduction in market-wide risk, with the upper end of each range reflecting an optimized mailing using letters that generated the largest impacts on risk.

Findings from Section 5.2 suggest that the vast majority of the average risk reduction—or about 85 percent—is not explained just by positive selection in age or membership in less costly regions, but on healthier risks conditional on those factors. A reduction in market-wide risk of about 1.0 percent, if translated to lower premiums, would lead to meaningful decreases in public subsidy and consumer spending. It would also lead to *additional* increases in enrollment beyond the direct letter effect, in response to premium reductions, particularly among unsubsidized consumers. The most reliable price elasticities are based on discontinuities in Massachusetts' subsidy design, which imply that a decrease in premiums by $40 results in a 25 percent enrollment increase (Finkelstein, Hendren and Shepard 2019). As context, a 1 percent decrease in monthly premiums ($5) would roughly imply a 3 percent increase in enrollment for the low-income unsubsidized consumers, and perhaps 1-2 percent for the above 400 percent of FPL segment of the unsubsidized market. Naturally, lower costs will tend to lower both premiums and (price-linked) subsidies, which will tend to raise net-of-subsidy premiums for other metal tiers, leading to plan switches, changes in risk sorting, and some changes in subsidized take-up. Accounting for these equilibrium dynamics would require a structural model of premium setting and plan choice by risk. As the purpose of the projection is to provide bounds for the actuarial risk impacts of an expanded intervention, we stop short of structurally modeling these equilibrium effects.

These estimates are likely lower bounds on the enrollment and risk effects, given the restrictive definition of the treated population. As a reference, there are roughly 2.2 million uninsured Californians

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comprised primarily of renewing marketplace consumers), making their predictive risk higher than predicted risk for people without encounters (based only on age and sex). As noted in Section 5.3, lower cost sharing among Medicaid and uninsured (uncompensated) care may result in greater OSPHD encounters than consumers in higher cost sharing or managed care Covered California plans. Hence, when comparing populations with different pre-period coverage and cost-sharing, differences in OSHPD-based prospective risk may not capture differences in underlying health or realized claims under the same cost-sharing and coverage. (Importantly, this issue does *not* bias our estimates of β_r using the CDPS score, as β_r was identified by randomizing treatment across a common population.) The much lower age of the Funnel population, as well as anecdotal information from plans, suggests that the enrollees from the year-long Funnel are similar, or even healthier, than other enrollees. In principle, we could estimate R_F directly using concurrent 2016 claims based risk measures detailed in Section 3.5 and 5.3. Unfortunately, currently the claims data sources available to Covered California do not allow us to obtain the concurrent risk measures linked to Funnel status.

at any point in time, of which about 1.4 million are estimated to be marketplace-eligible. If targeted to the entire uninsured marketplace-eligible population, even a smaller risk effect could generate a larger reduction in total market risk than our lower bound estimates.

Appendix Table 1. Sample Exclusions

	Number of Households	Take-up
Universe of Households	153,146	7%
Initial Budgetary Exclusion	26,964	9%
Funnel Sample Size for Budget	126,182	7%
Reason for Sample Exclusion		
Any member with invalid age	50	0%
FPL<100	3,463	1%
Invalid Mailing Address	4,167	3%
SAWS and Deemed Subsidy Ineligible	35,283	1%
Final Study Sample Size	87,394	9%

Appendix Table 1 reports the number of households associated with sample exclusions imposed on the Funnel poulation, and the take-up rate for that exclusion. The December 2015 Funnel included 153,146 households who were initially considered eligible for the study. The initial exclusion dropped a randomly selected 26,964 households, due to study budget constraints. The resulting 126,182 households were then randomization into five study arms, according to the stratified methodology described in Section 3.4. As described in Section 3.2, after randomization, additional exclusions were imposed based on information about household program eligibility and address availibility. Exclusion counts in the table are unconditional on the other exclusions, so households may appear in more than one row. The final study sample size was 87,394.

Appendix Table 2. Average Treatment Effects on Take-up

Model	OLS						Logit	
Funnel Sample	All		Open Enrollment		County Referral		All	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Arm2345: All Letters	0.012***		0.016***		0.009***		0.168***	
	(0.002)		(0.005)		(0.003)		(0.039)	
Arm2: Basic Letter		0.012***		0.016***		0.007**		0.134***
		(0.003)		(0.006)		(0.004)		(0.049)
Arm3: Subsidy-Penalty		0.014***		0.023***		0.012***		0.191***
		(0.003)		(0.006)		(0.004)		(0.048)
Arm4: Price Compare		0.010***		0.011*		0.010***		0.143***
		(0.003)		(0.006)		(0.004)		(0.048)
Arm5: Price-Quality Compare		0.012***		0.015**		0.008**		0.170***
		(0.003)		(0.006)		(0.004)		(0.048)
County Referral							-1.507***	-1.507***
							(0.075)	(0.075)
Arm2345*County Referral							0.051	
							(0.083)	
Arm2*County Referral								-0.011
								(0.104)
Arm3*County Referral								0.036
								(0.104)
Arm4*County Referral								0.106
								(0.104)
Arm5*County Referral								0.073
								(0.103)
Constant	0.071***	0.071***	0.143***	0.143***	0.057***	0.057***	-1.786***	-1.786***
	(0.016)	(0.016)	(0.027)	(0.027)	(0.017)	(0.017)	(0.213)	(0.213)
Control: Covariates x Treatment Arms	Y	Y	Y	Y	Y	Y	Y	Y
Observations	87,394	87,394	44,248	44,248	43,146	43,146	87,394	87,394
R-squared	0.028	0.029	0.036	0.038	0.019	0.022		

Appendix Tables 2 reports OLS treatment effects of letter interventions on take-up of 2016 open enrollment coverage, with a full set of interactions between treatment arms and all control, following Lin (2013). Columns (1)-(2) are estimated on the full study sample. Columns (3)-(4) and (5)-(6) restrict the sample to the Open Enrollment and County Referral samples, respectively. Columns (7)-(8) reports logit specifications on the full sample, including interactions between treatment assignment and an indicator for County Referral. Covariates include household level controls, including family size, number of kids, age, race, language preferences, marital status, Covered California's age-based community-rating premium ratio, and household income (as percent of the FPL). Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Appendix Table 3. Heterogeneous Treatment Effects, by Income (Non-Parametric)

Model	OLS			Logit		
Sample	All	OE	County Ref	All	OE	County Ref
	(1)	(2)	(3)	(4)	(5)	(6)
Arm2: Basic Letter	0.006	0.023*	-0.002	0.092	0.174**	-0.055
	(0.005)	(0.012)	(0.005)	(0.070)	(0.088)	(0.117)
Arm345: Subsidy Arms	0.013***	0.033***	0.003	0.186***	0.240***	0.088
	(0.004)	(0.010)	(0.004)	(0.057)	(0.073)	(0.093)
FPL in [180, 250]	-0.002	-0.000	-0.005	-0.031	0.012	-0.113
	(0.005)	(0.012)	(0.005)	(0.078)	(0.094)	(0.140)
FPL in [250, 400]	-0.011	-0.008	-0.010	-0.124	-0.062	-0.291
	(0.008)	(0.015)	(0.008)	(0.114)	(0.131)	(0.253)
Arm2 x FPL in [180, 250]	0.008	-0.004	0.013*	0.093	-0.027	0.336*
	(0.007)	(0.016)	(0.007)	(0.098)	(0.119)	(0.174)
Arm345 x FPL in [180, 250]	-0.003	-0.020	0.005	-0.046	-0.133	0.141
	(0.006)	(0.013)	(0.005)	(0.081)	(0.099)	(0.144)
Arm2 x FPL in [250, 400]	0.009	0.000	0.007	0.115	0.036	0.224
	(0.008)	(0.016)	(0.007)	(0.110)	(0.127)	(0.257)
Arm345 x FPL in [250, 400]	-0.004	-0.025**	0.007	-0.056	-0.163	0.289
	(0.007)	(0.013)	(0.006)	(0.092)	(0.106)	(0.207)
Constant	0.167***	0.155***	0.035***	-1.797***	-1.715***	-3.443***
	(0.010)	(0.019)	(0.009)	(0.133)	(0.159)	(0.244)
$\Delta_1 = \beta_{Arm345} - \beta_{Arm2}$	0.007*	0.010	0.006	0.094*	0.067	0.143
P-val: (Δ_1)	0.090	0.307	0.121	0.086	0.329	0.132
$\Delta_2 = \beta_{Arm345\ x\ FPL[180,250]} - \beta_{Arm2\ x\ FPL[180,250]}$	-0.011*	-0.015	-0.008	-0.139*	-0.107	-0.196
P-val: (Δ_2)	0.077	0.241	0.149	0.072	0.257	0.156
$\Delta_3 = \beta_{Arm345\ x\ FPL[250,400]} - \beta_{Arm2\ x\ FPL[250,400]}$	-0.013*	-0.026**	0.001	-0.171**	-0.200**	0.065
P-val: (Δ_3)	0.057	0.049	0.925	0.048	0.046	0.746
$\Delta_2 - \Delta_1$	-0.018*	-0.025	-0.014	-0.233*	-0.173	-0.339
P-val: ($\Delta_2 - \Delta_1$)	0.060	0.238	0.105	0.057	0.252	0.115
$\Delta_3 - \Delta_1$	-0.020**	-0.036*	-0.005	-0.265**	-0.267*	-0.078
P-val: ($\Delta_3 - \Delta_1$)	0.044	0.097	0.559	0.039	0.086	0.762
Controls	Y	Y	Y	Y	Y	Y
Observations	75,495	32,698	42,797	75,495	32,698	42,797
R-squared	0.065	0.039	0.018			

Appendix Table 3 reports heterogeneous treatment effects by income brackets. Δ_1 reports the additional treatment effect of the Subsidy Reporting arms over the Basic Reminder for the <180 FPL segment. Δ_3 reports the same effect for the 250-400 FPL bracket. (Δ_3 - Δ_1) reports the difference in the relative effects. All regressions control for a full set of household level characteristics, described in Section 4.1. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Appendix Table 4. Treatment Effects on Coverage Duration, Among Enrolled Consumers

Dependent Variable: Enrollment Length (months)												
Entry Sample	All		Open Enrollment					County Referral				
Income Sample	All		All	All	≤180 FPL	180-250	>250 FPL	All	All	≤180 FPL	180-250	>250 FPL
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Arm2345: All Letters	-0.131		-0.053					-0.343*				
	(0.095)		(0.109)					(0.190)				
Arm2: Basic Letter		-0.121		0.017	0.042	-0.068	0.101		-0.558**	-0.177	-0.923**	-1.594**
		(0.118)		(0.134)	(0.248)	(0.246)	(0.209)		(0.250)	(0.354)	(0.404)	(0.731)
Arm345: Subsidy Arms		-0.134		-0.077	0.085	-0.160	-0.119		-0.276	-0.002	-0.633**	-1.275**
		(0.098)		(0.112)	(0.206)	(0.206)	(0.178)		(0.196)	(0.279)	(0.318)	(0.633)
Control Group Mean (month)	8.44	8.44	8.43	8.43	8.64	8.26	8.42	8.47	8.47	8.66	8.40	8.00
Observations	7,962	7,962	6,214	6,214	1,817	1,934	2,463	1,748	1,748	852	657	239
R-squared	0.031	0.031	0.030	0.030	0.035	0.050	0.034	0.048	0.049	0.073	0.062	0.182

Appendix Table 4 reports treatment effects of letter interventions on duration of coverage, conditional on take-up. Enrollment duration is measured as the average number of months of paid coverage among household policy holders on policies opened during open enrollment. Column headers note sample specifications. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Appendix Table 5. Heterogeneous Treatment Effects, by Baseline Risk (Not Controlling for Age Factors or Regions)

Funnel Sample	All		Open Enrollment					County Referral				
Income Sample	All		All	All	≤180 FPL	180-250	>250 FPL	All	All	≤180 FPL	180-250	>250 FPL
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Arm2345: All Letters	0.000		-0.012					0.007				
	(0.005)		(0.010)					(0.004)				
Arm2: Basic Letter		0.001		-0.007	-0.016	-0.007	0.001		0.005	0.005	0.009	-0.003
		(0.006)		(0.013)	(0.027)	(0.024)	(0.018)		(0.005)	(0.008)	(0.010)	(0.009)
Arm345: Subsidy Arms		-0.000		-0.013	-0.013	-0.014	-0.009		0.007*	0.006	0.008	0.008
		(0.005)		(0.010)	(0.022)	(0.020)	(0.014)		(0.004)	(0.006)	(0.008)	(0.009)
ln(CDPS Score)	-0.001	-0.001	0.005	0.005	0.005	0.002	0.007	-0.001	-0.001	-0.003	0.001	-0.000
	(0.003)	(0.003)	(0.006)	(0.006)	(0.013)	(0.012)	(0.009)	(0.002)	(0.002)	(0.003)	(0.004)	(0.006)
Arm2345 x ln(CDPS Risk)	-0.010***		-0.022***					0.001				
	(0.003)		(0.007)					(0.003)				
Arm2 x ln(CDPS Risk)		-0.009**		-0.020**	-0.032*	-0.020	-0.011		0.001	0.006	-0.002	-0.006
		(0.004)		(0.009)	(0.019)	(0.017)	(0.013)		(0.004)	(0.005)	(0.006)	(0.007)
Arm345 x ln(CDPS Risk)		-0.010***		-0.023***	-0.035**	-0.021	-0.014		0.000	0.002	-0.001	-0.001
		(0.004)		(0.007)	(0.015)	(0.014)	(0.010)		(0.003)	(0.004)	(0.005)	(0.006)
Constant	0.080***	0.080***	0.133***	0.133***	0.166***	0.148***	0.111***	0.034***	0.034***	0.040***	0.036***	0.022***
	(0.004)	(0.004)	(0.009)	(0.009)	(0.019)	(0.017)	(0.012)	(0.004)	(0.004)	(0.005)	(0.007)	(0.008)
Control: Age and Region	N	N	N	N	N	N	N	N	N	N	N	N
Observations	86,876	86,876	44,029	44,029	9,783	12,169	22,077	42,847	42,847	18,977	15,756	8,114
R-squared	0.001	0.001	0.001	0.001	0.003	0.001	0.000	0.000	0.000	0.000	0.000	0.001

Appendix Table 5 reports heterogenous treatment effects on take-up, by baseline health spending risk. Risk is measured using the CDPS prospective risk score, based on diagnoses from 2015 hospitalizations and emergency room encounters. Column headers note sample specifications. Appendix Table 6 reports estimates from analogous specifications controlling for the age-based premium ratios. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Appendix Table 6. Heterogeneous Treatment Effects, by Baseline Risk (Controling for Age Factors and Regions)

Funnel Sample	All		Open Enrollment					County Referral				
Income Sample	All		All	All	≤180 FPL	180-250	>250 FPL	All	All	≤180 FPL	180-250	>250 FPL
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Arm2345: All Letters	0.008		0.002					0.010				
	(0.008)		(0.015)					(0.007)				
Arm2: Basic Letter		0.013		0.008	0.025	0.030	-0.003		0.017*	0.030**	0.010	-0.006
		(0.010)		(0.019)	(0.044)	(0.040)	(0.025)		(0.009)	(0.014)	(0.017)	(0.015)
Arm345: Subsidy Arms		0.007		0.000	0.018	-0.006	-0.003		0.008	0.021*	0.001	-0.011
		(0.008)		(0.015)	(0.035)	(0.033)	(0.020)		(0.007)	(0.011)	(0.014)	(0.012)
Age Rating	0.017***	0.017***	0.012**	0.012**	0.041***	0.032**	0.000	0.019***	0.019***	0.021***	0.022***	0.006
	(0.003)	(0.003)	(0.006)	(0.006)	(0.015)	(0.014)	(0.007)	(0.003)	(0.003)	(0.005)	(0.006)	(0.005)
ln(CDPS Score)	-0.005*	-0.005*	0.003	0.003	-0.003	-0.004	0.008	-0.005**	-0.005**	-0.008**	-0.004	-0.000
	(0.003)	(0.003)	(0.006)	(0.006)	(0.013)	(0.012)	(0.009)	(0.003)	(0.003)	(0.004)	(0.005)	(0.006)
Arm2345 x ln(CDPS Risk)	-0.009**		-0.021***					0.001				
	(0.003)		(0.007)					(0.003)				
Arm2 x ln(CDPS Risk)		-0.008*		-0.018**	-0.027	-0.017	-0.012		0.003	0.010*	-0.001	-0.008
		(0.004)		(0.009)	(0.020)	(0.017)	(0.013)		(0.004)	(0.006)	(0.007)	(0.007)
Arm345 x ln(CDPS Risk)		-0.009**		-0.021***	-0.033**	-0.021	-0.014		0.000	0.004	-0.002	-0.004
		(0.004)		(0.007)	(0.015)	(0.014)	(0.010)		(0.003)	(0.005)	(0.006)	(0.006)
Arm2345 x Age Rating	-0.005		-0.008					-0.002				
	(0.004)		(0.006)					(0.004)				
Arm2 x Age Rating		-0.007		-0.008	-0.024	-0.022	0.002		-0.007	-0.013**	-0.001	0.001
		(0.005)		(0.008)	(0.021)	(0.019)	(0.010)		(0.004)	(0.006)	(0.009)	(0.008)
Arm345 x Age Rating		-0.004		-0.007	-0.019	-0.006	-0.003		-0.001	-0.008	0.004	0.011*
		(0.004)		(0.007)	(0.017)	(0.016)	(0.008)		(0.004)	(0.006)	(0.007)	(0.006)
Constant	0.053***	0.053***	0.117***	0.116***	0.116***	0.082**	0.120***	0.008	0.008	0.008	-0.005	0.034**
	(0.009)	(0.009)	(0.016)	(0.016)	(0.037)	(0.034)	(0.021)	(0.008)	(0.008)	(0.013)	(0.014)	(0.017)
Control: Age and Region	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	86,876	86,876	44,029	44,029	9,783	12,169	22,077	42,847	42,847	18,977	15,756	8,114
R-squared	0.004	0.004	0.003	0.003	0.009	0.007	0.002	0.008	0.008	0.006	0.010	0.018

Appendix Table 6 reports heterogenous treatment effects on take-up, by baseline health spending risk. Risk is measured using the CDPS prospective risk score, based on 2015 hospitalizations and emergency room encounters. Column headers note sample specifications. All regressions control for ACA age-based community-rating premium ratios and region. Appendix Table 5 reports estimates from analogous specifications without controls. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Appendix Table 7. Treatment Effect on the Average Risk of Enrolled Consumers (Controlling for Age Factors and Region)

Panel A: Dependent Variable = ln(CDPS Risk Score)								
Funnel Sample	All		Open Enrollment			County Referral		
Income Sample			<180FPL	180<FPL<250	>250FPL	<180FPL	180<FPL<250	>250FPL
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Arm2345: All Letters	-0.043**							
	(0.017)							
Arm2: Basic Letter		-0.037*	-0.086*	-0.017	-0.041	0.089	-0.014	-0.257**
		(0.020)	(0.045)	(0.039)	(0.033)	(0.067)	(0.072)	(0.120)
Arm345: Subsidy Arms		-0.045**	-0.101**	-0.019	-0.049	0.045	-0.029	-0.126
		(0.017)	(0.039)	(0.032)	(0.030)	(0.052)	(0.057)	(0.109)
Constant	-1.521***	-1.521***	-1.449***	-1.626***	-1.467***	-1.512***	-1.696***	-1.744***
	(0.043)	(0.043)	(0.103)	(0.072)	(0.063)	(0.172)	(0.116)	(0.188)
Observations	7,945	7,945	1,810	1,933	2,458	851	655	238
R-squared	0.065	0.065	0.086	0.085	0.052	0.109	0.114	0.187

Panel B: Dependent Variable = ln(Concurrent Risk Score)								
Funnel Sample	All		Open Enrollment			County Referral		
Income Sample			<180FPL	180<FPL<250	>250FPL	<180FPL	180<FPL<250	>250FPL
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Arm2345: All Letters	-0.023							
	(0.034)							
Arm2: Basic Letter		0.008	-0.085	-0.021	-0.011	0.164	0.059	-0.047
		(0.043)	(0.097)	(0.090)	(0.074)	(0.132)	(0.150)	(0.206)
Arm345: Subsidy Arms		-0.034	-0.189**	-0.122	0.008	0.272***	0.023	-0.242
		(0.035)	(0.080)	(0.075)	(0.061)	(0.104)	(0.135)	(0.179)
Constant	2.087***	2.088***	2.258***	2.052***	2.020***	2.137***	2.090***	2.159***
	(0.081)	(0.081)	(0.180)	(0.170)	(0.144)	(0.236)	(0.318)	(0.325)
Observations	11,472	11,472	2,445	2,565	3,655	1,264	969	548
R-squared	0.111	0.111	0.128	0.111	0.120	0.107	0.111	0.168

Appendix Table 7 reports treatment effects on average risk of enrolled individuals, controlling for age factors and region. Table 5 reports analogous specifications controling for age and region. The dependent variable in Panel A is the log of the CDPS prospective risk score, based on 2015 hospital and emergency room encounters. The dependent variable in Panel B is the log concurrent risk score, based on realized 2016 claims data. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Appendix Table 8. Indirect Utility Model Logit Regressions and Implied Valuations

Entry Sample	All	
Income Sample	All	
	(1)	(2)
γ	0.148***	0.147***
	(0.032)	(0.031)
α	0.276***	
	(0.013)	
Constant	-0.925***	-1.575***
	(0.116)	(0.115)
Controls	Y	Y
Observations	87,394	87,394
Implied Value: Letter ($/m)	53.388***	
	(11.671)	
Calibration using FHS (2019)		
Takeup Effect (%)	15.533	
Implied Value: Letter ($/m)	24.85	

Appendix Table 8 reports estimates from the indirect utility model (equation 3). α represents the effect of the subsidy on indirect utility. γ represents the effect of receiving any treatment letter on indirect utility. The implied value of the letter in subsidy-dollar equivalence is calculated as γ/α. The bottom portion reports implied letter values where α is calibrated to elasticities reported in Finkelstein, Hendren and Shepard (2019). *** p<0.01, ** p<0.05, * p<0.1

Appendix Table 9. Demographics of Comparison Populations

	RCT Sample	Covered California 2015	California Uninsured 2015	Full-Year "3-Day" Funnel	Full-Year "10-Day" Funnel
	(1)	(2)	(3)	(4)	(5)
HH Age (mean)	37.65	43.94	37.28	37.16	37.10
SD of HH Age	14.64	13.30	13.26	13.93	13.94
FPL (FPL<400)	212.25	203.63	216.55	210.45	210.48
SD of FPL	62.68	63.28	80.03	64.02	64.03
FPL > 400 (share)	0.14	0.09	0.22	0.10	0.10
White (share)	0.26	0.34	0.26	0.26	0.26
Latino (share)	0.43	0.20	0.33	0.43	0.43
Asian (share)	0.12	0.17	0.11	0.12	0.12
Black (share)	0.05	0.02	0.05	0.05	0.05
County Referred	0.49	0.02	N/A	0.45	0.46
Marketplace Take-up	0.081	1.000	N/A	0.110	0.082
N (Households)	87,394	800,778	1,354,572	644,586	624,166
N (Individuals)	121,828	1,206,920	2,180,528	947,597	916,697

Appendix Table 9 reports household-level income and demographic characteristics of the RCT study sample; enrollees in California's health insurance marketplace (Covered California) in December 2015, when the RCT sample was drawn; the uninsured population in California in 2015 (based on the American Community Survey); and the two definitions of the "full-year" Funnel population in 2016, corresponding to the study's 2016 coverage year. The 3-day (10-day) Funnel is comprised of consumers who initiated the eligibility process for coverage in Covered California at anytime during 2016, including the open enrollment period at the end of 2015, but did not enroll after 3 (10) days. Statistics for the 3- and 10-day Funnel populations do not include individuals in the RCT treatment arms, but indiviudals in the RCT control arm by five, to reflect the full Funnel population in the absence of the RCT. The marketplace take-up rate in column 1 is retricted to the control arm of the RCT study sample, only.