Reducing Administrative Barriers Increases Take-up of Subsidized Health Insurance Coverage: Evidence from a Field Experiment*

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Abstract

Administrative barriers to social insurance program take-up are pervasive, including in subsidized health insurance. We conducted a randomized controlled trial with Massachusetts' Affordable Care Act marketplace to reduce these barriers and other behavioral frictions. We find that a "check the box" streamlined enrollment intervention raises enrollment by 11%, more than personalized reminder letters (7.9% increase) or generic reminder letters (4.5% increase). Effects are concentrated among individuals eligible for zero-premium plans, who faced no further administrative burdens of setting up payments. Producing this enrollment effect through premium reduction would cost about $6 million in subsidies, highlighting the importance of these burdens.

Keywords: Administrative barriers, information frictions, nudge, health insurance exchange, health insurance marketplace, Affordable Care Act

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1 Introduction

Individuals face administrative barriers to enrollment in social programs, such as lengthy applications and frequent eligibility re-certification (Herd and Moynihan, 2018). These barriers screen some individuals out of these programs, as well as impose significant time and psychological costs on inframarginal program enrollees (i.e., "sludge"; Thaler 2018; Sunstein 2019). Some barriers may be intended to target specific groups (Nichols and Zeckhauser, 1982; Kleven and Kopczuk, 2011), but often they screen out individuals who would benefit most from these programs (Finkelstein and Notowidigdo, 2019; Deshpande and Li, 2019). As a result, low take-up of social programs, including health insurance, is concerning.

We conducted a randomized controlled trial of interventions to increase take-up of free or low-cost insurance in Massachusetts ConnectorCare, a program administered by the state's health insurance marketplace that offers generously-subsidized insurance to certain low-income households. Take-up is relatively low, despite options with monthly premiums ranging from \$0 for the lowest-income individuals to \$100 for individuals making three times the poverty line. Even among those deemed eligible in the first stage of the application process, only about 50% actually enroll. While some unenrolled individuals may not value the insurance, previous research suggests that hassle costs and behavioral frictions also play an important role (Myerson et al., 2022; McIntyre and Shepard, 2022; Domurat et al., 2021; Bhargava and Manoli, 2015).

We designed a streamlined enrollment mechanism aimed at individuals who were determined eligible for ConnectorCare but who did not immediately enroll at the time of eligibility determination. This administrative simplification used information the government already had to enable individuals to enroll by simply checking a box and sending back the form in a pre-paid envelope, and making payment if necessary. In contrast, the standard enrollment process entailed either logging into a website, telephoning during business hours, or meeting in person with a navigator/counselor. These approaches all imposed substantial frictions, including the website: individuals often needed to log in using credentials created by others (such as case workers) that were unknown to them.

The streamlined enrollment mechanism was costly to implement, requiring the marketplace to manually process paper forms returned by mail. The administrative simplification thus partially shifted the administrative burden from the individual to the state. In contrast, "nudges" often do not require changes to underlying administrative processes, which can make them easier to implement. We therefore tested streamlined enrollment against two nudges that did not require any changes to the state's enrollment processes: (1) generic reminders and (2) reminders with personalized information about subsidized premiums.

Compared to standard operating procedures, our streamlined enrollment treatment raises take-up among our sample population by 3.2 percentage points (11% of baseline; $p < 0.01$). The effect is much larger (6.1 pp, 21% of baseline; $p < 0.01$) for individuals who were eligible for zero-cost plans. This intervention greatly reduced hassles for these participants because they faced no additional hurdles associated with owing and paying premiums for their plans. The reminder and information provision interventions are also effective, consistent with past work (Domurat et al., 2021; Yokum et al., 2022). However, they are less effective than administrative simplification. The reminder letter significantly increases enrollment by 1.3 percentage points (4% of baseline). The letter with personalized information is slightly more effective, significantly increasing enrollment by 2.3 percentage points (8%). The overall *incremental* effect of the streamlined enrollment treatment is thus modest (1-2 pp), though generally statistically significant. The incremental effect is also much larger for those eligible for zero-cost plans (almost 5 pp), again highlighting the importance of the streamlined enrollment intervention for this group.

Administrative barriers to accessing public benefits are pervasive. Nudges appear insufficient for getting individuals through these barriers, as our results show administrative simplification helps even after reminders and information. Importantly, administrative simplification was most important for those for whom the simplification was more complete. For individuals eligible for zero-premium coverage, our intervention made enrolling as simple as checking the box and drove a large increase in take-up. For those who owed premiums, our simplification was incomplete: those who checked the box still had to pay the initial premium and set up ongoing monthly premium payments. In this group, we find virtually no incremental effect of administrative simplification compared to the personalized information nudge. Further, 25% of those who returned the streamlined enrollment form failed to effectuate coverage by making the payment, indicating that the hassles associated with premium payment are likely to be a meaningful barrier to continued enrollment. Finally, the effect of administrative simplification also declines in this group over time, consistent with individuals who enrolled via the "check-the-box" pathway failing to pay subsequent premiums. Thus, our results suggest that administrative simplification can be highly effective for boosting take-up in the absence of premiums, but premiums introduce administrative hassles of their own that limit the effectiveness of enrollment simplification.

One interpretation of our findings is that marginal enrollees placed relatively low value on insurance. However, the incremental effects of the administrative simplification intervention were concentrated among older individuals who likely have greater healthcare needs, pushing against this interpretation. Further, among the group where the administrative simplification was most effective (those eligible for zero-premium coverage), effects on take-up were highly persistent: the incremental effect of this treatment relative to the personalized nudge was equally large a full year after the intervention as it was 2-3 months after the intervention. This persistence suggests that valuation of the coverage among marginal enrollees was unlikely to be particularly low, as we would expect that take-up effects would be relatively short-lived for those with low valuation.

We benchmark the effect of our intervention to the reduction in premiums necessary to increase enrollment by the same amount. We identify the effect of premiums on enrollment using discontinuous price changes at certain incomes (Finkelstein et al., 2019). To increase enrollment as much as our intervention, premiums would have to fall by about \$39 per year. Reducing premiums also benefits the many inframarginal enrollees, and so the government would have to spend about \$6 million in premium subsidies to match the enrollment of scaled-up administrative simplification. The marginal costs of administrative simplification are comparatively trivial (sending the letter and manually enrolling the 7% of individuals who return it). Moreover, administrative simplification can be targeted to those who are eligible but do not enroll, while the premium reduction must be given to all consumers.

We also discuss our interventions' impact on individual welfare. Interpreting welfare in the presence of behavioral frictions is the subject of debate (Bernheim and Rangel, 2009; Beshears et al., 2008) and requires additional structure. In stylized models, we find a range of estimates suggesting our intervention raised welfare by between about \$5 and \$100 per person in our sample, with differences stemming from assumptions about whether the interventions reduced welfare-relevant hassle costs or psychological barriers to enrollment.

Prior studies have tested informational nudges. Domurat et al. (2021) tests a variety of such nudges on the California health insurance marketplace and finds that they increased enrollment by 1.3 percentage points on average and induced favorable selection on health into the marketplace. Similarly, nudges in Yokum et al. (2022)

increased enrollment by 0.3 percentage points in the federally facilitated marketplace. Ericson et al. (2017) tested nudges in the Colorado marketplace to re-enrolling consumers, raising shopping rates by 6 percentage points without detecting e ects on plan switching. Finally, Goldin et al. (2021)'s randomized outreach to households who paid a tax penalty for being uninsured raised enrollment by 1.1 percentage points.

We build on this work by comparing these nudges to deeper changes to marketplace architecture that permit a more intensive intervention: administrative simpli cation for enrollment. While we are unable to observe enrollee health, we can use age as a proxy. Our results suggest that the selection e ects of administrative simpli cation may di er from those of nudges | while our personalized information nudge had similar e ects for older and younger individuals, the incremental e ects of our administrative simpli cation intervention were signi cantly larger for older individuals who likely have greater healthcare needs.

While we are unaware of any randomized evidence on streamlined enrollment interventions on health insurance, in a di erent domain Choi et al. (2009) show that a \Quick Enrollment" intervention increased participation in a retirement savings plan. Likewise, Finkelstein and Notowidigdo (2019) show that informative letters with enrollment assistance increase take-up of SNAP more than informative letters alone.

2 Background

The ConnectorCare program provides subsidized private insurance to low-income individuals not eligible for Medicaid and is administered by the state's ACA marketplace, the Massachusetts Health Connector. ConnectorCare was established by the 2006 Massachusetts health reform. With the enactment of the ACA, it became part of the state's subsidized insurance marketplace. ConnectorCare has additional, state-sponsored subsidies beyond those provided by the ACA.

Individuals are eligible for ConnectorCare if their household income is under 300% of the federal poverty line (FPL) and they do not have access to Medicaid, Medicare, or employer-sponsored coverage. Eligible individuals have a choice of up to 5 di erent insurers. Each insurer can only o er one ConnectorCare plan, which is standardized in its covered bene ts and cost-sharing parameters; premiums, networks, and formularies may vary across insurers. Appendix Figure A1 shows how after-subsidy premiums vary by income, with discontinuous changes at 150, 200, and 250% FPL. Subsidies are linked to the price of the lowest-priced plan and enrollees face the full incremental premium for more expensive plans.

Enrollment in ConnectorCare involves three steps: eligibility determination, plan selection, and payment of the first month's premium. First, there is a unified eligibility application for all state health insurance benefits, including Medicaid and ConnectorCare. This application is often filled out by a social worker or hospital employee when they determine that a patient or client is uninsured. When an application is made on the website, the state almost instantly determines whether the individual is eligible for Medicaid, ConnectorCare, or other (potentially unsubsidized) coverage. An individual may also be determined eligible for ConnectorCare through automatic redetermination processes, including losing Medicaid through its eligibility redetermination process. All ConnectorCare-eligible individuals are notified of their eligibility (by mail or email, depending on their indicated communication preference). Next, they must make an active choice between one of up to 5 available plans. Finally, to effectuate enrollment, they must pay the first month's premium.

Despite the comparative simplicity and high subsidy rates of ConnectorCare, take-up is incomplete. We estimate the take-up rate for individuals who have been determined by the state to be eligible to be 50%. Moreover, there are eligible individuals who are unknown to the state because they never filed an application. Finkelstein et al. (2019) estimate take-up rates of all eligibles for an earlier version of this market, finding take-up rates between 37 and 63%.

3 Field Experiment Details and Data

Our intervention targeted individuals whose eligibility was known to the Health Connector, but who had not enrolled immediately at the time of eligibility determination. We focus on this group so that eligibility is certain and so that we can determine the actual premiums these individuals must pay, which vary by income.

Our sample is comprised of two groups. The first consists of individuals churning out of Medicaid coverage. These individuals have received an eligibility redetermination indicating that they are no longer eligible for Medicaid but are now eligible for ConnectorCare. The second is made up of individuals who applied for coverage and were confirmed eligible for ConnectorCare, but have not completed the plan selection step.[1]

[1] Unfortunately, our data does not permit us to determine which group an individual is in.

We restrict our intervention to non-elderly adults ages 18-64 in households where only one person was eligible for ConnectorCare. This restriction greatly simpli ed the calculation of plan prices and options. These criteria do not necessarily exclude married people, since spouses might have health insurance through other sources like their employer. It also did not typically exclude parents because their children would be eligible for Medicaid or CHIP, not ConnectorCare | both programs had income eligibility limits up to 300% FPL. About 80% of new coverage initiations are single-adult plans depending on the month.

These groups of individuals { and thus our experiment which targeted them { played a substantial role in this market. Appendix Figure A2 shows participants in our study accounted for more than a third of people who enrolled in ConnectorCare during the periods our study was running. This nding re ects that only a minority of consumers complete all the enrollment steps simultaneously. Around two-thirds of those determined eligible for ConnectorCare did not immediately enroll upon eligibility determination, and they thus entered our study.

We developed a set of interventions aimed at increasing take-up in this population. There are four arms of the experiment:

Arm 1: Control/business-as-usual . Individuals received no intervention as a result of our study. Like all people in the state who had an eligibility determination but had not picked a plan, including individuals in the other study arms, the Health Connector sent them twice-monthly reminder emails.

Arm 2: Generic reminder letter . Individuals were sent letters via postal mail that reminded them of their eligibility for ConnectorCare insurance and provided information about how to apply for coverage. These letters did not contain any personalized information.

Arm 3: Personalized information letter . Individuals were sent letters similar to Arm 2, but with the addition of a table with personalized after-subsidy premium costs for each of their plan options.

Arm 4: Streamlined-enrollment (\check-the-box") letter . Individuals were sent letters similar to Arm 3, but which also allowed them to enroll by simply checking a box for their selected plan and sending back the form in a pre-paid envelope.

Behavioral science informed the intervention design. Arm 2 aims to address procrastination and forgetting (O'Donoghue and Rabin, 1999; Ericson, 2011, 2017). Arm 3 targets information and beliefs by providing personalized price information, as individuals may over- or under-estimate the costs of plans. All letters are available in Appendix D.

Our most novel intervention, Arm 4, aims to reduce the administrative burden necessary to enroll. Developing this arm required extensive collaboration with the Health Connector and reviews by its legal counsel to determine a process that would be permissible under state and federal law.

During July-September 2018 and April-June 2019, we enrolled participants into our study every 2 weeks. While typically individuals need to sign up for ConnectorCare during an annual open enrollment period, many individuals become eligible to sign up throughout the year due to qualifying life events such as losing Medicaid eligibility or employer-sponsored insurance. These events create a Special Enrollment Period (SEP) during which the individual may enroll in a ConnectorCare plan. Participants in this study were individuals in SEPs who had not yet picked a plan. We paused enrollment from October 2018 to March 2019 to avoid interactions with the open enrollment period.

On a biweekly basis, the Health Connector drew the universe of those eligible for the study. All eligible households were then entered into the study, excluding those who had already been enrolled in previous weeks. Upon entry to the study, the Health Connector randomly assigned individuals to study arms using a sequence of assignments derived from random numbers provided by the study team. Based on these assignments, for individuals in arms 2-4, the Health Connector sent them the appropriate study letter 7 days after the data pull. Individuals typically had 45 days after receiving the letter to sign up for a plan.

We measured enrollment e ectuation, de ned as enrollment within 90 days of entering the study, and enrollment duration over the next two years. To e ectuate coverage, enrollees had to select a plan and, except in cases of zero-premium plans, make their rst month's premium payment. We measured FPL using records from Massachusetts' eligibility database, taking the average FPL among records in the month up to the letter mailing.

Table 1 shows the characteristics of our study population by arm. The nal number of participants was 58,238 (Appendix Figure A3). The average age was 38 years, and average income was about 190% FPL. About one in four participants was eligible for a zero-premium plan. Finally, about one in five participants had previously been enrolled in a Health Connector plan sometime since Jan 2016; however, by construction, these individuals were not coming directly from a Health Connector plan. As expected given randomization, characteristics across arms were balanced (Wald joint test of significance $p = 0.35$).

4 Field Experiment Results

To estimate the effect of our interventions, we run the following regression:

$$Y_i = {}_1 + {}_2\mathrm{Arm\,2}_i + {}_3\mathrm{Arm\,3}_i + {}_4\mathrm{Arm\,4}_i + {}_{b(i)} + X_i + {}_i \qquad (1)$$

where $\mathrm{Arm\,2}_i$, $\mathrm{Arm\,3}_i$, and $\mathrm{Arm\,4}_i$ indicate the assignment of individual i to the respective study arms; ${}_{b(i)}$ refers to "batch" fixed effects, where b indexes the biweekly batch in which the individual was enrolled; and ${}_i$ is a random error term. X_i is a vector of control variables included to improve precision, as controls are not needed to address bias given the randomized design. Controls vary across specifications as pre-specified in our pre-analysis plan, ranging from no controls to a host of controls for pre-determined covariates (see Table 2 notes). Following our analysis plan, we also implement the Lasso double-selection method (Jones et al., 2019) to optimally pick power-raising controls.

Table 2 shows our main results examining the effect of our intervention on enrollment within 90 days after the letter was sent. The choice of controls matters little for our point estimates or precision, so we focus on the results in column 1, which only control for batch fixed effects.

The baseline probability of enrolling with business-as-usual (Arm 1) was 29.1%. Receiving the additional reminder in Arm 2 raised enrollment by 1.3 percentage points (4.5%), with a 95% CI of 0.3 to 2.3 percentage points. Reminder mailings thus can be an effective intervention to increase enrollment. The personalized information received by Arm 3 raised enrollment by 2.3 percentage points (7.9%) over baseline, with 95% CI ranging from 1.3 to 3.3 percentage points. These results suggest that personalized information was more effective than the simple reminder. We reject the equality of effects of Arms 2 and 3 at the 10%, but not 5%, level.

Finally, the streamlined enrollment process offered to Arm 4 had the largest effect, raising enrollment by 3.2 percentage points (11.0%) over baseline (95% CI 2.3 to 4.3 percentage points). This finding indicates that the barriers and costs imposed by the standard enrollment process were screening out some enrollees. The increase attributed to the streamlined enrollment processes over and above the personalized information of Arm 3 is 1 percentage point, a difference that is statistically significant at the 10% (but not 5%) level. We strongly reject that the effects of the three arms all equal 0 ($p < 0.001$).

While Arm 4's streamlined enrollment had the largest effect relative to baseline, the Arm 3 result suggests about two-thirds of Arm 4's effect could be attributed to it serving as a reminder and conveying personalized information. Thus, much of the enrollment increases could have been achieved with the technically simpler (from the government's perspective) interventions in Arm 2 or 3. However, the benefit of the streamlined enrollment process also accrued to all enrollees who used it and thus bore fewer hassle costs, not just those whose enrollment was marginal to being assigned to Arm 4.

Our results suggest that many individuals in Arm 4 benefited. If we assume that all of the marginal enrollees over Arm 3 used the streamlined channel, then one-seventh of inframarginal Arm 4 enrollees also benefited from the streamlining, even though it was not pivotal for their enrollment.[2]

4.1 Persistence

We next explore the dynamics of the interventions' effects on enrollment and describe their impact on enrollment duration. Figure 1 plots the impact of the intervention by month, broken out by arm, from the month before letters were sent through 12 months after the mailings. We present separate plots for those eligible (Panel A) and ineligible (Panel B) for zero-premium plans, as the patterns differ markedly between these groups.

Panel A shows that, while all of the interventions initially increased take-up for the zero-premium eligible group, the increase was much larger for the streamlined enrollment intervention. For this group, the effects of streamlined enrollment were also highly persistent, remaining large up through one year after the intervention. In contrast, effects of the generic and personalized reminders for this group were initially smaller and fade over time.

[2] Specifically, we assume the share of Arm 4 enrollees for whom the letter was pivotal equals about 3%: the difference in effects between Arms 3 and 4, or roughly 1%, scaled by the enrollment rate in Arm 4, or 33.7%. Data on letter returns shows that 17.3% of Arm 4 enrollees mailed back a letter and enrolled. Removing those for whom it was pivotal leaves about 14% of Arm 4 enrollees benefiting due solely to streamlining.

Panel B shows that for the zero-premium ineligible group, the story was quite different. Again, all interventions initially increased take-up over the status quo. However, for this group, the initial increase in take-up from streamlined enrollment was virtually identical to that of the personalized reminder. Further, while the effects of the generic and personalized reminders persist through the end of the year, the effects of the streamlined enrollment intervention fade over time, dropping below the effects of the personalized nudge after just a few months.

The ConnectorCare payment architecture provides a potential explanation for this reversal of the relative effects of the streamlined enrollment intervention and the personalized nudge. We speculate that some of the marginal streamlined enrollment participants opted to pay their initial premium by including a check when mailing back their "check-the-box" letter. If these individuals then failed to set up automatic recurring payments, they might have neglected to pay their subsequent premiums. They then would have been removed from coverage after 1-2 months of non-payment.

To analyze persistence more formally, we estimate the effect of the interventions on the number of months of enrollment (i.e. member-months) over the year after individuals entered the study. (See Appendix Table A1 for details.) Matching the visual evidence, we see differences between the effects of the interventions for those eligible versus ineligible for zero-premium coverage. For zero-premium eligibles, the estimated effects of Arms 2 and 3 are smaller than in the full sample, though confidence intervals are wide. However the streamlined enrollment intervention has a large effect, equal to an increase of 0.44 months (17.4% of baseline). For zero-premium ineligibles, all interventions increased enrollment, but the personalized nudge had the largest point estimate, not the streamlined enrollment.

These results further emphasize the importance of administrative burdens. We find that streamlined enrollment is very effective for zero-premium eligibles, for whom it eliminated the bulk of the hassle associated with enrolling. In contrast, for zero-premium ineligibles, this intervention reduced some of the hassle of initially enrolling but left intact many of the hassles of making monthly premium payments, especially payments due after the first month. Consistent with these hassles acting as a significant barrier, the initial effect of streamlined enrollment is smaller for this group, and it fades out as subsequent payments become due.

4.2 Heterogeneous Effects

Examining further heterogeneity in our intervention's effects can illuminate the channels by which it changed behavior and help target future interventions to the groups who were most affected. Figure 2 shows the results of our prespecified sub-group analyses, with Panel A reporting the effect of streamlined enrollment vs. business-as-usual and Panel B reporting streamlined enrollment vs. personalized nudges (Table A2 shows the coefficients).[3] We present the results of our regression specification without controls, but results are quite similar for specifications with controls.

The need to make payments is important for interpreting the effect of streamlined enrollment intervention. We first repeat the split of the previous section and consider heterogeneity by zero-premium eligibility, this time looking at any enrollment rather than duration. The results continue to highlight the efficacy of streamlined enrollment for individuals who faced no ongoing hassle of paying premiums. Moreover, Appendix Table A3 shows that of the 7.7% of Arm 4 participants who returned the streamlined channel's "check-the-box" letter, only 75.8% actually enrolled. Some individuals who returned the letter did not include a check for the first month's premium payment or set up recurring electronic payments online.[4]

Next, we consider heterogeneity by age. Our interventions have a larger effect on those with ages above the median, as compared to those below the median. This is true both for the effects of the streamlined enrollment intervention relative to control and relative to the personalized nudge, suggesting that streamlined enrollment was particularly impactful for older consumers. As we do not observe health status, age is our primary proxy for health, and, based on this (imperfect) proxy, this result suggests that the marginal enrollees were possibly sicker than inframarginals. This stands in contrast to Domurat et al. (2021), which found that simple nudges in California induced healthier individuals to enroll.

We also examined heterogeneity by the time individuals had from letter receipt to the enrollment deadline to examine whether short deadlines influence the interventions' effectiveness. The estimated effects were similar for groups needing to apply within 30 days (due sooner) or with more time (due later). Similarly, there is no clear

[3] We also prespecified two analyses that we were unable to run, due to not being able to get access to the data: 1) a split based on source of entry – leaving Medicaid versus not – and 2) healthcare utilization in the All Payer Claims Database.

[4] This is consistent with findings that zero-premium-plan availability leading to increased and faster take-up (Dague, 2014; Drake and Anderson, 2020; Drake et al., 2022), and that automatically switching enrollees who failed to pay their premiums to alternative free plans increased insurance coverage (McIntyre et al., 2021).

difference in effects for those receiving Spanish versus English letters, though power is low due to the smaller Spanish-language sample.

5 Quantifying the Enrollment Impact

Here, we quantify the impact of the most effective intervention, Arm 4's administrative simplification. First, we find the equivalent decline in premiums necessary to yield the same increase in enrollment, which requires that we estimate how enrollment responds to changes in premiums. Then we discuss our welfare interpretation of these results and the cost-benefit analysis from the Connector's perspective.

5.1 Effects of Premiums on Enrollment

Determining the effect of premiums on enrollment requires an identification strategy to handle unobserved characteristics that affect demand, such as individual preferences that covary with premiums. We use a regression discontinuity strategy devised in Finkelstein et al. (2019) that takes advantage of the discontinuous premium changes at 200% and 250% FPL. (See Figure A1[5]).

We first show how premiums change at the discontinuities. The coefficient here will become the denominator in the elasticity[6]. Appendix Tables A4 and A5 estimate models showing how premiums per month change at these discontinuities under varying income bin sizes and bandwidths. We find that premiums increase by approximately $40 per month at both 200% and 250% FPL.

We next turn to changes in enrollment at the discontinuities to estimate the numerator of the elasticity. Figure 3 confirms visually that there are discontinuities in enrollment at these FPLs, but also that enrollment declines continuously in income. Appendix Tables A6 and A7 estimate models showing how the log number of new

[5] There are also discontinuities in premiums at 150% and 300% FPL. We do not include the 150% FPL discontinuity because it is close to the 138% FPL threshold for Medicaid eligibility for non-immigrants, providing few income bins below the threshold not contaminated by Medicaid eligibility. We omit the 300% FPL discontinuity because above this threshold individuals no longer have access to ConnectorCare plans and must enter a different market.

[6] One complication, also noted in Finkelstein et al. (2019), is that at 200% FPL, plans also change in cost-sharing characteristics. However, when we run separate analyses at the 200% and 250% cutoffs, we estimate similar semi-elasticities of enrollment. This is consistent with DeLeire et al. (2017), who look at income-based discontinuities in eligibility for cost-sharing-reductions (CSRs) in federally-facilitated Marketplaces. They find that CSR eligibility does not impact the decision to enroll. Finkelstein et al. (2019) also have this problem and justify the inclusion of the 200% cutoff with a similar argument.

enrollees per month changes at these discontinuities under varying binsizes and bandwidths. The exact estimate varies by specification, but we can summarize the effects as an approximately 22% decline per month at both cutoffs. We use a semi-elasticity model of enrollment (% change in enrollment from $1 per month increase in price) to summarize this data. Our estimates imply a semi-elasticity of $0.22/40 = \quad 0.0055$.

5.2 Premium Decrease Yielding the Same Impact as Intervention

To increase ConnectorCare enrollment, an alternative to administrative simplification would be to lower premiums. Here we ask, compared to running our simplified enrollment intervention for a full year, how much lower would premiums have to be to induce the same number of individuals to enroll?

We consider a uniform premium reduction that is applied to all single-member ConnectorCare households. A targeted premium reduction just for individuals eligible for our intervention would be infeasible, as households would face strong incentives to become eligible by not immediately enrolling upon eligibility determination. The premium reduction p necessary to induce the same increase in enrollment as our intervention is the product of two terms: the ratio of intervention-eligibles S to total ConnectorCare enrollment E_0, and the ratio of our intervention's effect on enrollment to the semi-elasticity of enrollment with respect to premiums. Total enrollment E_0 is approximately 184,000, while intervention-eligibles S total about 105,000 per year (8500 per month).[7] That is,

$$p = \frac{S}{E_0} \quad -$$

Thus, the premium reduction necessary to increase enrollment by the same amount as our intervention is:

$$p = \frac{102{,}000}{184{,}000} \quad \frac{0.032}{0.0055} = \$3.23 \text{ lower premiums per member per month.} \qquad (2)$$

The cost of the premium reductions necessary to increase enrollment by the same amount as running our intervention for a year is then: $(184{,}000)(12)(3.23)$ = approximately $5.9 million. This cost clearly exceeds the cost of the intervention (sending letters and some minor admin costs for manually enrolling the 7.7% of Arm 4 indi-

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[7] The intervention induces S enrollees. To find enrollment induced by a price change, we multiply the change by the semi-elasticity of demand and by total ConnectorCare enrollment. Setting these equal and solving for p gives the expression.

viduals who returned the letter).

A key advantage to nudges and administrative simplifications like the ones we provided is that they can be targeted to those who need additional help. A major reason for the large cost of the premium reductions is that they would have to apply to the entire ConnectorCare population, not just those who did not immediately enroll at eligibility determination.

5.3 Cost-Benefit analysis from the Health Connector's Perspective

Our administrative simplification intervention increased enrollment. However, DellaVigna et al. (2022) shows that many effective nudges are not adopted by the very governments that tested them. They point to the possibility that nudges entail excess costs of sending additional communications without clear benefits to the agency charged with implementing them. In our case, the Health Connector receives an administrative fee of about 3% of unsubsidized premiums, which we estimate is about $129 per additional enrollee. This gives them an expected gain in agency revenue of $3-4 per intervention recipient from the administrative simplification, which exceeds the marginal costs and may cover the fixed costs. Revenue gains were lower for the personalized reminder ($2-3 per intervention recipient) and generic reminder ($1-2 per intervention recipient) arms. We provide a detailed discussion the cost-benefit analysis in Appendix A.

5.4 Welfare Impact

The magnitude of intervention's impact on enrollee welfare is uncertain without substantially more structure.[8] One interpretation is that our interventions reduced a hassle cost of enrollment.

If individuals are all homogeneous except for their valuation of insurance, the intervention has the same effect (and attracts the same people) as lowering the price by h. In this model, our intervention's estimated effect, , must be equal to h , the percentage change in enrollment from a h drop in monthly price. This implies $h=\$70$ in annual premiums. All inframarginal enrollees save on this amount of hassle costs, while those induced to enroll gain by (approximately) half this amount.

However, the intervention may have reduced hassle costs in an individual-specific manner, h_i. Without restrictions on the distribution of h_i and how it covaries

[8] For more details on these models, see Appendix B.

with individuals' value of insurance, it is difficult to make definitive statements about welfare. For instance, our observed effect is consistent with h_i being very large for the 3.2% of individuals induced to enroll if those individuals also had very high value of insurance.

Finally, we could instead model the intervention as removing a psychological friction (e.g. by reminding them) that enabled individuals to make an active choice. Here, removing the psychological friction affects whether the individual makes an active choice, but not the welfare conditional on making the active choice (as in Ericson (2020)). For instance, the treatment may lower the probability of forgetting or procrastinating. If the psychological friction that is removed by the intervention is large and distributed independently of the value of insurance, new enrollees will have an expected value of insurance equal to that of all the existing enrollees. The average value of insurance—especially for the always-takers—is difficult to measure. Appendix B shows that with a linear extrapolation from the demand curve, the expected value of insurance to the enrolled is $1091 per year, giving a welfare impact of Arm 4 per person who received the intervention equal to about 0.032 times that, or $35. Alternatively, Finkelstein et al. (2019) show that the average public subsidy per enrollee is $4226 per year. If enrollees valued these subsidies dollar for dollar, then Arm 4 raised enrollee welfare by $135.[9]

6 Conclusion

In 2020, 28 million Americans did not have health insurance at any time during the year. This is despite massive efforts under the Affordable Care Act of 2010 to increase insurance coverage via large subsidies and expanded public insurance programs. We test whether simplifying the enrollment process can improve take-up, finding significant improvements (3.2 pp, 11%) over the status quo and modest improvements over a simple information intervention (0.9 pp, 3.1%). We show that the simplified enrollment intervention was most impactful for those for whom the hassle-removal was most complete, those eligible for zero-premium coverage. We show that the equivalent premium reductions (via government-financed subsidies) necessary to achieve these gains are enormous, vastly exceeding the almost trivial costs of these interventions, implying that these types of interventions can be highly

[9] Finkelstein et al. (2019) show that, for the portion of the demand curve they observe, individuals value these plans below cost; however, they also note that they cannot identify valuations in the upper 30% of the willingness-to-pay distribution.

cost-e ective ways to improve insurance take-up.

In the end, however, even with the interventions, take-up is far from complete. Over 65% of those eligible for the intervention did not enroll. This suggests that if universal coverage is the goal, other, more aggressive policies may be necessary. While one strategy to intervene on choices is to encourage or facilitate active decisions, another is the use of defaults. Streamlined enrollment mechanisms still require active choices, and our results show that they can be e ective when defaults are not possible, due to either legal, ethical, or practical constraints. But in the end, if legal barriers can be overcome, defaults may be the better policy, with recent evidence indicating that their e ects on enrollment can be much larger (Shepard and Wagner, 2021).

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Tables and Figures

Figure 1. E ects of Interventions on Enrollment over Time

(A) E ects for Zero-Premium Eligibles

(B) E ects for Zero-Premium Ineligibles

Notes: Figures present coe cients on each arm from a set of regressions, each one estimating the e ect on enrollment m months after letters were sent (or would have been sent in the control group) for m 2 (1; 12). Panel A presents coe cient estimates for those eligible for zero-premium plans. Panel B presents coe cient estimates for those ineligible for zero-premium plans. All regressions include only arm e ects and batch xed e ects.

Figure 2. Heterogeneous E ects for Sub-Groups

(A) E ect of Streamlined Enrollment (Arm 4)

(B) E ect of Streamlined Enrollment vs. Personalized Reminder (Arm 4 vs. Arm 3)

Notes: Figure presents results from regressions of an indicator for any enrollment in the rst 90 days after letters were sent (or would have been sent for the control group) on indicators for assignment to each of the three intervention arms and various controls. Each regression restricts to a di erent sub-group. All regressions include only controls for study `batch.' Panel A presents coe cients for Arm 4 versus Arm 1. Panel B presents coe cients for Arm 4 versus Arm 3.

Figure 3. Enrollment Around Subsidy Cuto s

(A) Premium vs. FPL

(B) Enrollment Levels vs. FPL

Notes: Figure shows average premiums and total enrollment counts for bins of income (as % of FPL). Red lines indicate income cuto s where subsidies (and thus premiums) change discontinuously. Figure based on all enrollments in a given year (including January re-enrollments), averaged across 2018-2020.

Table 1. Descriptive Statistics by Arm

	Arm 1 Mean	Arm 2 Mean	Arm 3 Mean	Arm 4 Mean	P-Value
Age	38.1	38.4	38.3	38.3	0.232
	(13.1)	(13.3)	(13.2)	(13.2)	
Income (FPL)	189.8	192.2	189.1	189.8	0.865
	(89.3)	(536.3)	(92.4)	(178.5)	
Zero Premium Eligible	0.224	0.235	0.230	0.236	0.068
Female	0.598	0.600	0.596	0.599	0.897
Prior Connector Enrollment	0.189	0.194	0.189	0.191	0.685
Observations	14,501	14,724	14,383	14,630	

Notes: Arm 1 is the control arm, Arms 2-4 are the intervention arms. Cells present means with standard deviations in parentheses. Standard deviations in parentheses. Age is measured in years. Prior Health Connector Enrollment refers to enrollment in any Connector plan during the prior 12 months. Income is measured in the eligibility data, and zero premium eligibility is determined by income. P-value column gives result from F-tests for equality of the variable across all arms. Wald test of joint signi cance across all variables has $p = 0.35$.

Table 2. E ect of Intervention on Enrollment

	(1) No Controls	(2) Basic Controls	(3) Enhanced Controls	(4) Post-LASSO Controls
Arm 2: Generic reminder	0.013	0.012	0.012	0.012
	(0.005)	(0.005)	(0.005)	(0.005)
Arm 3: Personalized reminder	0.023	0.022	0.022	0.023
	(0.005)	(0.005)	(0.005)	(0.005)
Arm 4: Streamlined enrollment	0.032	0.032	0.032	0.033
	(0.005)	(0.005)	(0.005)	(0.005)
Control Mean	0.304	0.304	0.304	0.304
Observations	58,238	57,890	57,890	57,890
Adjusted R^2	0.011	0.032	0.033	0.034
P-Values:				
Arm 2 = Arm 3	0.063	0.061	0.070	0.059
Arm 2 = Arm 4	0.000	0.000	0.000	0.000
Arm 3 = Arm 4	0.082	0.054	0.053	0.053
Arm 2 = Arm 3 = Arm 4 = 0	0.000	0.000	0.000	0.000

Notes: Dependent variable: indicator for any enrollment in the rst 90 days after letters were sent (or would have been sent for the control group). All regressions include xed e ects for the study `batch' de ned as all individuals who entered the study on the same date. Basic controls include indicators for gender, prior Health Connector enrollment, whether the individual was (or would have been) sent a Spanish letter, the time between when the letter was sent and when the person had to enroll, income splines, and age splines. Enhanced controls add xed e ects for 3-digit zip codes. Post-Lasso controls chooses controls from all enhanced controls plus all two-way interactions. Robust standard errors in parentheses.

Online Appendix For: Reducing Administrative Barriers Increases Take-up of Subsidized Health Insurance Coverage: Evidence from a Field Experiment

Ericson, Layton, McIntyre, and Sacarny

A Cost-Benefit Analysis from the Health Connector's Perspective

We randomized 58,328 prospective enrollees to our four arms, requiring $24,179 in fixed costs and $96,677 in marginal costs. Fixed costs included salary and fringe benefit expenses for Connector staff who devoted time to intervention planning and implementation ($13,650) and the cost of having the letters professionally designed ($10,529).

The largest marginal expense ($60,533) was the cost of printing and mailing intervention letters to the 43,737 enrollees who were randomized to one of our three treatment arms, which translates to $1.38 per enrollee. First-class postage was approximately $0.50 per letter during our intervention; the remaining cost covered materials and printing.

In addition to mailing expenses, there were call volumes associated with the intervention.

Recorded expenses related to inbound call volume ($31,096 for 1,728 calls) represented all inbound calls from anyone who had been assigned to one of our three intervention arms. Call volume for the control arm was not recorded; as a result, we need to make assumptions about how many inbound calls were marginal (versus calls that would have happened even in the absence of intervention).

We estimate that 7.2% of people assigned to treatment arms who enrolled in coverage during our study were marginal enrollees. If we assume that marginal enrollees are as likely to make an inbound call as inframarginal enrollees, the estimated marginal inbound call costs would be $2,239. If marginal enrollees were twice as likely as inframarginal enrollees, on average, to call, estimated marginal inbound call costs would be $4,177; if marginal enrollees were three times more likely to call, the marginal costs would be $5,871.

For our streamlined enrollment letter, there were also costs related to outbound calls ($2,656 for 148 calls) that might be necessitated by intervention; for example, outbound calls were made in cases where someone returned an incomplete enrollment form. We considered these to be fully marginal costs.

For each additional ConnectorCare enrollee, the Connector receives an administrative fee (3% of unsubsidized premiums), which we estimate is about $129 per additional enrollee per year over a calendar year. We assume that the Connector captures this full administrative fee.[10] To calculate revenue generated by our interventions, we multiply this $129 by the e ect size found for each of our arms and the total number of people randomized to each arm.

Using this information, we're able to estimate whether any of our interventions have the potential to generate positive return on investment (ROI). We assume xed costs are distributed equally across treatment arms. Because our inbound call expenses are not separated by arm, we also assume that those are equally distributed across arms. We calculate ROI by subtracting costs from revenue, then dividing by costs.

The table below shows costs, revenues, and ROI under each of the three assumptions described above: that marginal enrollees are equally likely to make an inbound call to the Health Connector as inframarginal enrollees, that they're twice as likely to make a call, and that they're three times as likely to make a call. Even in the implausible scenario (not shown in the table) where we assume all inbound calls should be attributed to marginal enrollees, the ROI would remain positive for Arms 3 (+11%) and 4 (+46%).

Costs, Revenues, and Return on Investment of Letters

[10] We know the Connector gets at least 6 additional months of enrollment per additional enrollee in the rst year: this comes from the estimated increased months of enrollment from Table A1 divided by the main enrollment e ect. Table A8 indicates they get about 9 months additional enrollment per new enrollee in the rst 18 months; we cannot measure enrollment persistence at longer horizons, and so approximate with one full year.

B Welfare Impact of Intervention

We can also quantify the impact of the intervention by evaluating the welfare consequences. Evaluating the welfare impact of our intervention requires more modeling assumptions. We assess the welfare impact of our intervention in two models: one in which the intervention is interpreted as reducing the hassle cost of enrolling, and a behavioral model in which the intervention is interpreted as reducing a psychological friction that prevented consumers from enrolling.

B.1 Homogeneous Hassle Cost Model

Consider an individual i choosing whether to purchase insurance or not. An individual purchases insurance if it provides net utility greater than zero, that is if $v_i - p_i - h_i > 0$, where v_i is unobserved value of insurance, and p_i is premiums. Finally, h_i is the hassle cost of completing the enrollment process, which we initially assume is homogeneous ($h_i = h \; \forall i$). Let the intervention reduce hassle costs by Δh, as in Handel (2013), which induces all individuals with $v_i - p_i \in [h - \Delta h; h]$ to enroll in insurance. Our estimated effect of Arm 4 indicates this is fraction $\beta = 0.032$ of the targeted population.

In this hassle cost model, the intervention has the same effect (and attracts the same people) as lowering the price by Δh. A key feature of this model is that the individuals induced to enroll by our intervention have lowest value of insurance. However, inframarginal enrollees also save on the hassle cost Δh by taking advantage of the streamlined enrollment or by not needing to engage in costly effort to remember to enroll.

We can use the semi-elasticity to estimate Δh, as $\varepsilon \Delta h$ is the percentage change in enrollment from a \$1 drop in premiums. This must be equal to our intervention's estimated effect, β. Then, using $\beta = 0.032$ for Arm 4 and $\varepsilon = -0.0055$:

$$\Delta h = -\frac{\beta}{\varepsilon} = \$5.81 \text{ per month} \tag{3}$$

This implies $\Delta h = \$70$ in annual premiums. The consumer welfare impact of our intervention is $\frac{1}{2}\beta\Delta h$ for the 3.2% people induced to enroll by the intervention (\$1.12 per person, per year), and Δh for the 29.1% of individuals who would have enrolled regardless (\$20.29 per person, per year). Thus, in this model the intervention raised welfare by $\$1.12 + \$20.29 = \$21.40$ per intervention recipient per year. Given around 8,500 individuals eligible for the intervention each month, this would imply a welfare gain of $21.40(8,500)(12) = \$2.18$ million, the vast majority of which accrued to individuals who already enrolled.

The model above imposes that the intervention had a common effect on hassle costs for all individuals. However, the intervention may have reduced hassle costs in an individual-specific manner, Δh_i. Without restrictions on the distribution of Δh_i and how it covaries with v_i and h_i, it is difficult to make definitive statements about welfare. For instance, our observed effect is consistent with h_i being very large for the 3.2% individuals induced to enroll if those individuals also had very high v_i. Nonetheless, this model gives a sense of the possible welfare effect of the intervention. It also captures the intuition that relieving hassle costs can pay large dividends because doing so helps inframarginal individuals who would have otherwise experienced the hassles.

This model also does not capture some features of our intervention. Since we found that other letters also raised enrollment, the effect of administrative simplification in Arm 4 is comprised of reduced hassle, but also information provision and reminders. Reminders are not best thought of as reducing hassle costs, but in changing the probability of action (but not welfare conditional on action, see Ericson, 2020). Moreover, not everyone in our enrollment simplification arm used the newly designed form. To the extent that the intervention did not reduce hassles for some individuals, our calculations would overstate the benefits it produced.

B.2 Psychological Frictions Model

In the psychological frictions model, we interpret the effect of the intervention not as changing the hassle costs for individuals, but as removing a psychological friction (e.g. by reminding them) that enabled individuals to make an active choice. A key distinction is that removing the psychological friction affects whether the individual makes an active choice, but not the welfare conditional on making the active choice.

We use a simple model of psychological frictions, based on the framework of Ericson (2020). In this model, an individual enrolls if $v_i - p > \varepsilon_i$, where $\varepsilon_i \geq 0$ is a wedge between action and welfare as the individual themselves would judge it. We assume that $\varepsilon_i \in \{0, \infty\}$ so that individuals who draw a high value of ε never enroll, and individuals who draw $\varepsilon = 0$ only enroll when the net utility to doing so is positive.

In this model, the treatment lowers the probability of "forgetting" and drawing a high ε. As a result, the individuals induced to enroll by the intervention are no longer those with the lowest values of insurance v_i. Rather, they come from the whole distribution of v. Moreover, the treatment only has a welfare-relevant impact on individuals induced to enroll; it does not affect the welfare of those who would have enrolled in the absence of the treatment.

Under these assumptions, the welfare impact of the intervention per person who received the intervention is:

$$E[v_i - p | v_i - p > 0]; \quad (4)$$

that is, the probability the intervention induces an individual to enroll times the expected value of insurance to those who enrolled. This requires knowing the entire distribution of v_i among people who enroll, which is a very difficult object to estimate.

We can use a simple linear extrapolation of the demand curve from our regression discontinuity price estimate. To get an enrollment of zero in this subsidy eligible population, we would need a 100% decline in enrollment, or premiums to increase by $1/\varepsilon$: about $181.81 per month, or $2181.81 per year. This tells us that the highest v_i on the linear extrapolation from the current demand curve has a value of $2181.81 above current premiums, and hence $E[v_i - p | v_i - p > 0]$ is half that amount (given the linear model), or $1090.90. Finally, this implies the welfare impact of Arm 4 per person who received the intervention is $1090.90*0.032= $34.91.

We can use alternative methods to estimate $E[v_i - p | v_i - p > 0]$. For instance, Finkelstein et al. (2019) show that the average public subsidy per enrollee is $4226 per year. If enrollees simply valued these subsidies dollar for dollar, then $E[v_i - p | v_i - p > 0]$ = $4226 and Arm 4 raised welfare by $135.23. However, Finkelstein et al. (2019) show that, for the portion of the demand curve they observe, individuals value these plans below cost, even though theory suggests (due to risk aversion) that individuals should be willing to pay above own expected costs. (Though, Finkelstein et al. (2019) note that they cannot identify the valuation of plans for individuals in the upper 30% of the willingness-to-pay distribution, which likely makes these calculations conservative.) Taken together, these estimates suggest that an individual themselves would have valued this intervention at about $35.

C Appendix Tables

Appendix Table A1. E ect of Intervention on Enrollment Member Months

	(1) Full Sample	(2) Zero-Prem. Eligible	(3) Zero-Prem. Ineligible
Arm 2: Generic reminder	0.129	0.103	0.142
	(0.052)	(0.107)	(0.060)
Arm 3: Personalized reminder	0.181	0.036	0.225
	(0.053)	(0.108)	(0.061)
Arm 4: Streamlined enrollment	0.213	0.439	0.145
	(0.053)	(0.109)	(0.060)
Control Mean	2.745	2.520	2.810
Observations	58,238	13,465	44,773
P-Values:			
Arm 2 = Arm 3	0.333	0.532	0.178
Arm 2 = Arm 4	0.113	0.002	0.964
Arm 3 = Arm 4	0.546	0.000	0.191
Arm 2 = Arm 3 = Arm 4 = 0	0.000	0.000	0.002

Notes: + p < 0:1, p < 0:05, p < 0:01
Dependent variable is the number of months enrolled in the rst year since letter was mailed. Sample: Analysis Sample. All regressions include only controls for study `batch.' Robust standard errors in parentheses.

Appendix Table A2. Heterogeneous E ects for Sub-Groups

	(1) Overall	(2) Above Median Age	(3) Below Median Age	(4) Zero-Prem. Eligible	(5) Zero-Prem. Ineligible	(6) Spanish Letter	(7) English Letter	(8) Deadline Sooner	(9) Deadline Later
Arm 2: Generic reminder	0.013	0.013	0.012+	0.017	0.012+	0.019	0.012	0.005	0.022
	(0.005)	(0.009)	(0.007)	(0.011)	(0.006)	(0.017)	(0.006)	(0.007)	(0.008)
Arm 3: Personalized reminder	0.023	0.026	0.020	0.014	0.026	0.037	0.021	0.022	0.024
	(0.005)	(0.009)	(0.007)	(0.011)	(0.006)	(0.017)	(0.006)	(0.007)	(0.008)
Arm 4: Streamlined enrollment	0.032	0.040	0.027	0.061	0.024	0.030+	0.033	0.033	0.032
	(0.005)	(0.009)	(0.007)	(0.011)	(0.006)	(0.017)	(0.006)	(0.007)	(0.008)
Control Mean	0.304	0.338	0.282	0.273	0.313	0.316	0.303	0.330	0.272
Observations	58,238	23,294	34,944	13,465	44,773	6,235	52,003	32,600	25,638
P-Values:									
Arm 2 = Arm 3	0.063	0.132	0.225	0.809	0.030	0.277	0.117	0.023	0.841
Arm 2 = Arm 4	0.000	0.002	0.026	0.000	0.059	0.498	0.000	0.000	0.231
Arm 3 = Arm 4	0.082	0.130	0.318	0.000	0.778	0.685	0.043	0.151	0.322
Arm 2 = Arm 3 = Arm 4 = 0	0.000	0.000	0.001	0.000	0.000	0.146	0.000	0.000	0.001

Notes: + $p < 0.1$, $p < 0.05$, $p < 0.01$
Dependent variable: indicator for any enrollment in the rst 90 days after letters were sent (or would have been sent for the control group).
Sample: Analysis Sample. Each regression restricts to a di erent sub-group. All regressions include only controls for study `batch.' Robust standard errors in parentheses.

Appendix Table A3. Summary Statistics for Arm 4 and Letter Returner Sub-Groups

	(1) Full Arm 4 Sample	(2) Did Not Return Letter	(3) Did Return Letter	(4) Returned and Enrolled	(5) Returned and Didn't Enroll	(6) Didn't Return and Enrolled	(7) Didn't Return and Didn't Enroll
Age	38.311	37.976	42.319	42.989	40.213	38.868	37.591
	(13.211)	(13.129)	(13.544)	(13.545)	(13.348)	(13.288)	(13.041)
Female	0.599	0.599	0.599	0.599	0.599	0.619	0.590
Prior Connector Enrollment	0.191	0.187	0.241	0.237	0.254	0.228	0.169
Income (FPL)	189.809	190.554	180.918	179.404	185.690	191.856	189.987
	(178.510)	(182.068)	(128.449)	(143.991)	(56.155)	(87.502)	(210.368)
Zero Premium Eligible	0.236	0.230	0.303	0.335	0.206	0.212	0.238
Observations	14,630	13,503	1,127	855	272	4,073	9,430

Notes: This table presents characteristics of the Arm 4 (streamlined enrollment) sample and several sub-groups. Cells present means with standard deviations in parentheses. The first column presents the full Arm 4 sample while subsequent columns present sub-groups by whether the participant returned the streamlined enrollment letter or did not return it, and interactions of those sub-groups with whether the participant did or did not enroll in ConnectorCare coverage within 90 days.

Appendix Table A4. Premiums Regression Discontinuity Estimates at 200 FPL

VARIABLES	(1) Ave. Net-of-Subsidy Premium	(2) Ave. Net-of-Subsidy Premium	(3) Ave. Net-of-Subsidy Premium	(4) Ave. Net-of-Subsidy Premium	(5) Ave. Net-of-Subsidy Premium	(6) Ave. Net-of-Subsidy Premium	(7) Ave. Net-of-Subsidy Premium
Above Cutoff	40.06***	39.90***	39.99***	40.14***	40.42***	40.23***	40.41***
	(0.525)	(0.438)	(0.336)	(0.358)	(0.299)	(0.532)	(0.414)
FPL (Centered)	0.0344***	0.0354***	0.0342***	0.0362***	0.0404**	0.0321**	0.0414
	(0.00961)	(0.00718)	(0.00358)	(0.00887)	(0.0141)	(0.00950)	(0.0250)
Above Cutoff x FPL (Centered)	-0.00352	-0.000484	-0.000389	-0.0147	-0.0485**	-0.0110	-0.0651
	(0.0165)	(0.0136)	(0.0107)	(0.0200)	(0.0189)	(0.0498)	(0.0531)
FPL-Squared (Centered)							0.000144
							(0.000478)
Above Cutoff x FPL-Squared (Centered)							0.00101
							(0.00101)
Constant	52.17***	52.19***	52.16***	52.19***	52.22***	52.18***	52.22***
	(0.309)	(0.240)	(0.114)	(0.134)	(0.148)	(0.118)	(0.173)
Observations	100	50	20	16	12	8	20
R-squared	0.998	0.999	1.000	1.000	1.000	1.000	1.000
Bins	1%	2%	5%	5%	5%	5%	5%
Banwdth	50% FPL	50% FPL	50% FPL	40% FPL	30% FPL	20% FPL	50% FPL

Notes: $* p < 0.1$, $** p < 0.05$, $*** p < 0.01$
Sample: First enrollment period per Unique ID (2016-2019). Cutoff = 200% FPL. Robust standard errors in parentheses.

Appendix Table A5. Premiums Regression Discontinuity Estimates at 250 FPL

VARIABLES	(1) Ave. Net-of-Subsidy Premium	(2) Ave. Net-of-Subsidy Premium	(3) Ave. Net-of-Subsidy Premium	(4) Ave. Net-of-Subsidy Premium	(5) Ave. Net-of-Subsidy Premium	(6) Ave. Net-of-Subsidy Premium	(7) Ave. Net-of-Subsidy Premium
Above Cutoff	41.47***	41.35***	41.40***	41.10***	40.66***	41.54***	40.74***
	(0.686)	(0.515)	(0.354)	(0.338)	(0.428)	(0.339)	(0.463)
FPL (Centered)	0.0309**	0.0349***	0.0338***	0.0465***	0.0691***	0.0123	0.0915*
	(0.0134)	(0.0116)	(0.0101)	(0.0121)	(0.0149)	(0.0415)	(0.0432)
Above Cutoff x FPL (Centered)	0.00809	0.00367	0.00317	-0.00197	-0.00813	-0.00179	-0.0339
	(0.0231)	(0.0176)	(0.0116)	(0.0154)	(0.0270)	(0.0438)	(0.0572)
FPL-Squared (Centered)							0.00115
							(0.000885)
Above Cutoff x FPL-Squared (Centered)							-0.00156
							(0.00114)
Constant	93.77***	93.84***	93.84***	94.04***	94.28***	93.84***	94.32***
	(0.320)	(0.290)	(0.311)	(0.280)	(0.266)	(0.282)	(0.324)
Observations	100	50	20	16	12	8	20
R-squared	0.996	0.999	1.000	1.000	1.000	1.000	1.000
Bins	1%	2%	5%	5%	5%	5%	5%
Banwidth	50% FPL	50% FPL	50% FPL	40% FPL	30% FPL	20% FPL	50% FPL

Notes: $* p < 0.1$, $** p < 0.05$, $*** p < 0.01$
Sample: First enrollment period per Unique ID (2016-2019). Cutoff = 250% FPL. Robust standard errors in parentheses.

Appendix Table A6. Log Enrollment Regression Discontinuity Estimates at 200 FPL

VARIABLES	(1) Log New Enroll	(2) Log New Enroll	(3) Log New Enroll	(4) Log New Enroll	(5) Log New Enroll	(6) Log New Enroll	(7) Log New Enroll
Above Cutoff	-0.254**	-0.243**	-0.226***	-0.175*	-0.190	-0.328***	-0.167
	(0.0995)	(0.0949)	(0.0745)	(0.0864)	(0.104)	(0.0689)	(0.133)
FPL (Centered)	-0.00301**	-0.00290**	-0.00311**	-0.00346**	-0.00159	0.00501*	-0.000360
	(0.00142)	(0.00136)	(0.00120)	(0.00126)	(0.00223)	(0.00205)	(0.00484)
Above Cutoff x FPL (Centered)	-0.00274	-0.00281	-0.00292	-0.00553	-0.00743	-0.00360	-0.0154
	(0.00327)	(0.00310)	(0.00303)	(0.00392)	(0.00622)	(0.00853)	(0.0129)
FPL-Squared (Centered)							5.50e-05
							(0.000103)
Above Cutoff x FPL-Squared (Centered)							0.000140
							(0.000253)
Constant	7.593***	7.606***	7.606***	7.602***	7.622***	7.670***	7.629***
	(0.0461)	(0.0404)	(0.0326)	(0.0327)	(0.0269)	(0.0145)	(0.0331)
Observations	100	50	20	16	12	8	20
R-squared	0.522	0.719	0.877	0.889	0.854	0.860	0.888
Bins	1%	2%	5%	5%	5%	5%	5%
Banwidth	50% FPL	50% FPL	50% FPL	40% FPL	30% FPL	20% FPL	50% FPL

Notes: $* p < 0.1$, $** p < 0.05$, $* * * p < 0.01$
Sample: First enrollment period per Unique ID (2016-2019). Cutoff = 200% FPL. Robust standard errors in parentheses.

Appendix Table A7. Log Enrollment Regression Discontinuity Estimates at 250 FPL

VARIABLES	(1) Log New Enroll	(2) Log New Enroll	(3) Log New Enroll	(4) Log New Enroll	(5) Log New Enroll	(6) Log New Enroll	(7) Log New Enroll
Above Cutoff	-0.258**	-0.242**	-0.220	-0.100	-0.114	-0.211*	-0.0383
	(0.129)	(0.117)	(0.131)	(0.131)	(0.120)	(0.0952)	(0.134)
FPL (Centered)	-0.00574*	-0.00571**	-0.00603**	-0.00711	0.000272	0.0132*	0.00372
	(0.00295)	(0.00279)	(0.00278)	(0.00475)	(0.00720)	(0.00597)	(0.0119)
Above Cutoff x FPL (Centered)	0.00125	0.000772	0.000684	-0.00501	-0.0189**	-0.0334**	-0.0404***
	(0.00448)	(0.00424)	(0.00452)	(0.00539)	(0.00739)	(0.00760)	(0.0126)
FPL-Squared (Centered)							0.000195
							(0.000231)
Above Cutoff x FPL-Squared (Centered)							0.000432*
							(0.000244)
Constant	7.052***	7.077***	7.079***	7.061***	7.149***	7.260***	7.160***
	(0.0792)	(0.0706)	(0.0914)	(0.117)	(0.114)	(0.0892)	(0.126)
Observations	100	50	20	16	12	8	20
R-squared	0.447	0.660	0.834	0.873	0.876	0.868	0.918
Bins	1%	2%	5%	5%	5%	5%	5%
Banwidth	50% FPL	50% FPL	50% FPL	40% FPL	30% FPL	20% FPL	50% FPL

Notes: $* p < 0.1$, $** p < 0.05$, $*** p < 0.01$
Sample: First enrollment period per Unique ID (2016-2019). Cutoff = 250% FPL. Robust standard errors in parentheses.

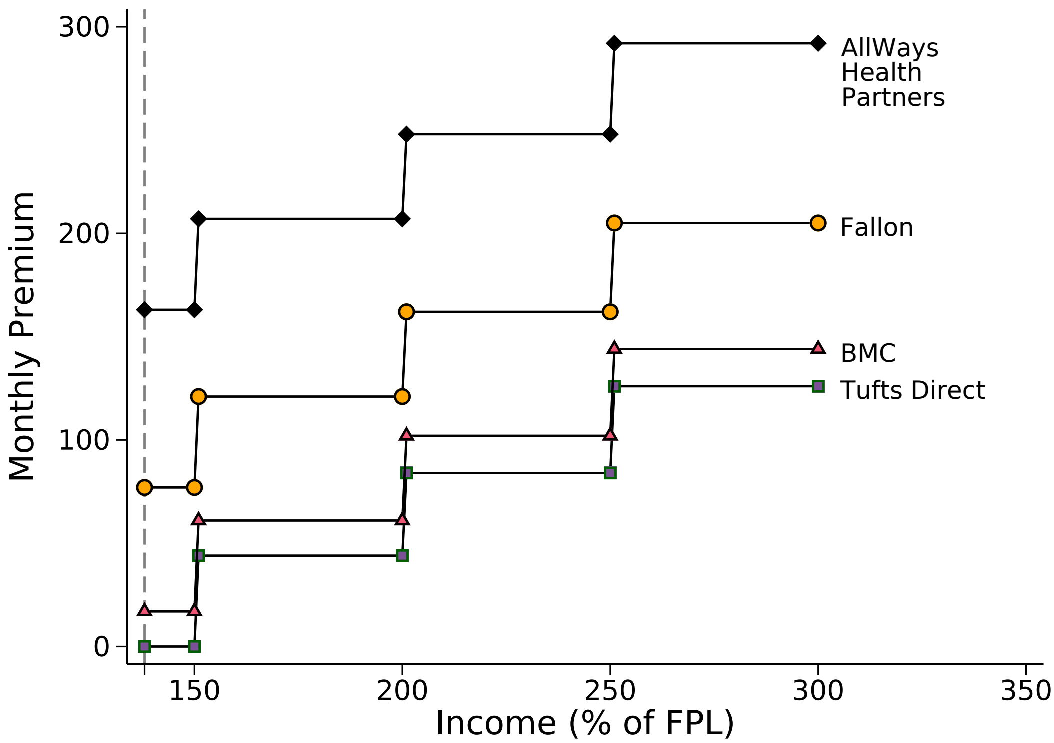
Appendix Table A8. Effect of Intervention on Enrollment Member Months for First 18 Months

	(1) Full Sample	(2) Zero-Prem. Eligible	(3) Zero-Prem. Ineligible
Arm 2: Generic reminder	0.176	0.128	0.196
	(0.075)	(0.152)	(0.085)
Arm 3: Personalized reminder	0.248	0.070	0.302
	(0.075)	(0.153)	(0.086)
Arm 4: Streamlined enrollment	0.284	0.585	0.192
	(0.075)	(0.156)	(0.085)
Control Mean	3.955	3.699	4.029
Observations	58,238	13,465	44,773
P-Values:			
Arm 2 = Arm 3	0.342	0.701	0.225
Arm 2 = Arm 4	0.152	0.003	0.963
Arm 3 = Arm 4	0.637	0.001	0.206
Arm 2 = Arm 3 = Arm 4 = 0	0.001	0.001	0.005

Notes: $+ p < 0.1$, $* p < 0.05$, $** p < 0.01$
Sample: Analysis Sample. Dependent variable is the number of months enrolled in the first 18 months since letter was mailed. All regressions include only controls for study 'batch.' Robust standard errors in parentheses.

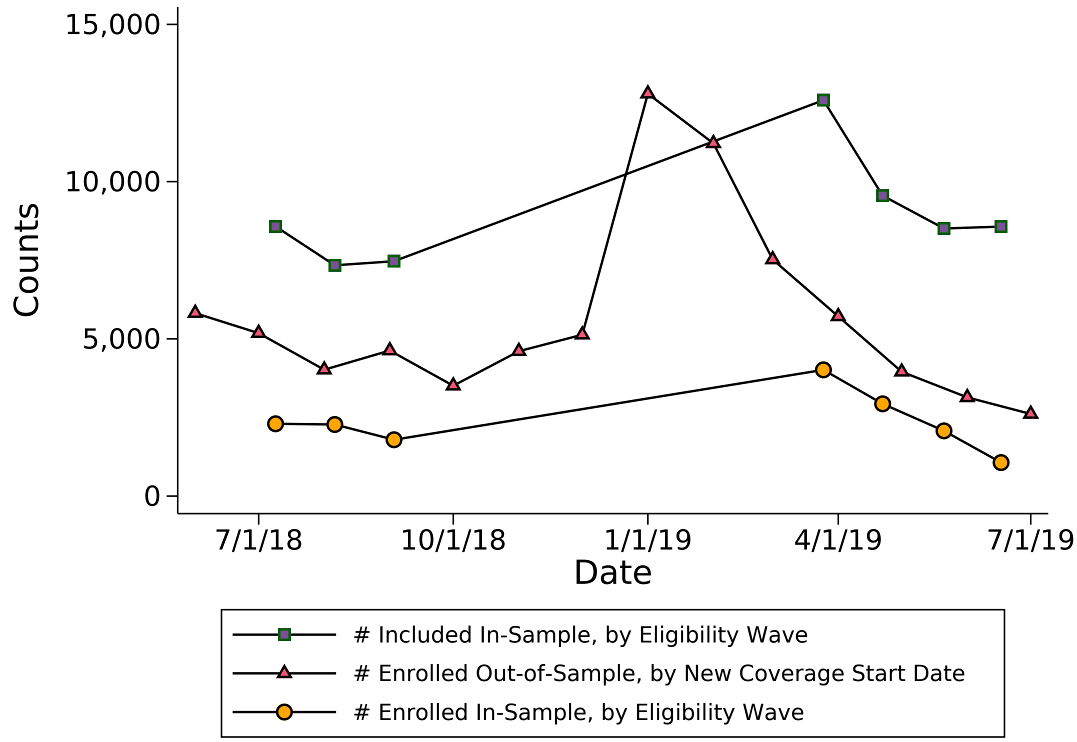
D Appendix Figures

Appendix Figure A1. Premiums by Income Level



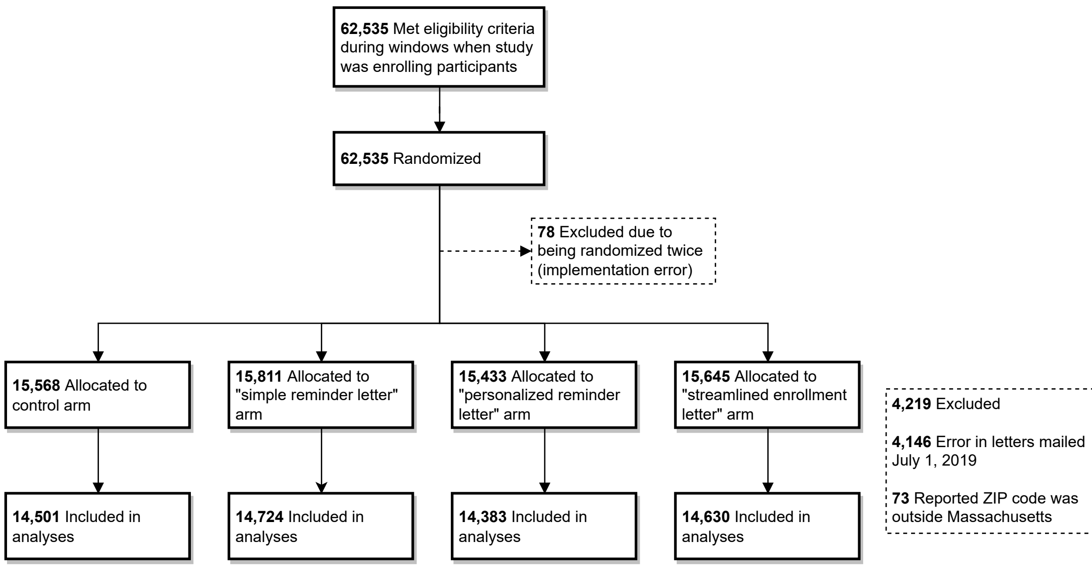
Notes: This figure shows net-of-subsidy monthly premiums of available ConnectorCare plans as a function of enrollee income (percent of FPL) in one Massachusetts rating area in 2018. The dashed vertical line at 138% FPL indicates the threshold for Medicaid eligibility.

Appendix Figure A2. Sources of Enrollment in ConnectorCare During Our Study



Notes: By month, this graph describes number of individuals in our sample, number of individuals enrolling but not in our sample, and number of individuals enrolling in our sample.

Appendix Figure A3. CONSORT Flow Diagram of Study Enrollment and Randomization



Appendix Figure A4. Generic Reminder Letter (Sent to Arm 2)

Dear [Name]:

Congratulations! You qualify for free or low-cost ConnectorCare health coverage through the Massachusetts Health Connector. Here are your next steps for enrolling in your new ConnectorCare plan.

1. Compare your plan choices

All ConnectorCare plans have the same low out-of-pocket costs. The plans have different monthly premiums (costs). ConnectorCare plans may also have different providers in their networks. Providers include doctors, hospitals and health centers. Plans may also cover different prescription medicines. To see providers and medicines in the plans, go to PlanFinder.MAhealthconnector.org.

2. Choose a plan

To see your plan choices and monthly premiums, register or log into your account at MAhealthconnector.org.

3. Enroll online, by phone or in person

- To enroll online, register or log into your account at MAhealthconnector.org.
- If you need help or want to enroll by phone, call the Massachusetts Health Connector at 1-877-623-6765 (TTY: 1-877-623-7773). Monday to Friday, 8:00 a.m. to 6:00 p.m.
- If you want to enroll in person, you can find a Navigator or Certified Application Counselor in your area at www.mahealthconnector.org/here-to-help.

4. Make your first premium payment

- If you choose a plan with a monthly premium of $0, skip this step. Your enrollment will start and you can begin to use your coverage in [Month].
- If your plan has a monthly premium cost, you will need to pay your first premium before your coverage can start. You will get an enrollment bill in the mail. To have coverage that starts in [Month], you need to pay your premium by [Month] [DD].

Appendix Figure A5. Personalized Reminder Letter (Sent to Arm 3)

(A)

Dear [Name]:

Congratulations! You qualify for free or low-cost ConnectorCare health coverage through the Massachusetts Health Connector. Here are your next steps for enrolling in your new ConnectorCare plan.

1. Compare your plan choices

All ConnectorCare plans have the same low out-of-pocket costs. The plans have different monthly premiums (costs). ConnectorCare plans may also have different providers in their networks. Providers include doctors, hospitals and health centers. Plans may also cover different prescription medicines. To see providers and medicines in the plans, go to PlanFinder.MAhealthconnector.org.

2. Choose a plan

Look at the back of this letter to see your ConnectorCare plan choices and monthly premiums.

3. Enroll online, by phone or in person

- The list on the back of this letter shows your ConnectorCare plan choices.
- To enroll online, register or log into your account at MAhealthconnector.org.
- If you need help or want to enroll by phone, call the Massachusetts Health Connector at **1-877-623-6765** (TTY: 1-877-623-7773). Monday to Friday, 8:00 a.m. to 6:00 p.m.
- If you want to enroll in person, you can find a Navigator or Certified Application Counselor in your area at www.mahealthconnector.org/here-to-help.

4. Make your first premium payment

- If you choose a plan with a monthly premium of $0, skip this step. Your enrollment will start and you can begin to use your coverage in [Month].
- If your plan has a monthly premium cost, you will need to pay your first premium before your coverage can start. You will get an enrollment bill in the mail. To have coverage that starts in [Month], you need to pay your premium by **[Month] [DD]**.

(B)

Your ConnectorCare plan choices

Below are you plan choices. To learn more about these plans or to enroll, go to MAhealthconnector.org. Or call 1-877-623-6765 (TTY: 1-877-623-7773).

Insurer	Monthly premium
[Insurer name]	$0
[Insurer name]	$30
[Insurer name]	$45
[Insurer name]	$60