ACTIVE CHOICE, IMPLICIT DEFAULTS, AND THE INCENTIVE TO CHOOSE

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ABSTRACT

Home-delivered prescriptions have no delivery charge and lower copayments than prescriptions picked up at a pharmacy. Nevertheless, when home delivery is offered on an opt-in basis, the take-up rate is only 6%. We study a program that makes active choice of either home delivery or pharmacy pick-up a requirement for insurance eligibility. The program introduces an implicit default for those who don't make an active choice: pharmacy pick-up without insurance subsidies. Under this program, 42% of eligible employees actively choose home delivery, 39% actively choose pharmacy pick-up, and 19% make no active choice and are assigned the implicit default. Individuals who financially benefit most from home delivery are more likely to choose it. Those who benefit least from insurance subsidies are more likely to make no active choice and lose those subsidies. The implicit default incentivizes people to make an active choice, thereby playing a key role in choice architecture.

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Introduction

Revealed preferences are tastes that rationalize an economic agent's observed actions. Economists usually assume that these revealed preferences are also *normative preferences*— preferences that represent the economic actor's true interests. In some situations, giving revealed preferences normative status makes sense. There are many cases, however, in which "choices" do not reveal a true preference, but reflect the combined influence of true preferences and decision-making biases. For example, in some situations, economic agents do not actively make choices. Instead, they passively accept defaults that are chosen by others. Preferences revealed through passive choice are often inconsistent, since variation in defaults generates large variation in outcomes (e.g., Madrian and Shea, 2001; Johnson and Goldstein, 2003; Beshears, Choi, Laibson and Madrian, 2008b). To the extent that the wedge between normative and revealed preferences is created by inertia around the default, active choice mechanisms that encourage individuals to explicitly state their preferences may produce outcomes closer to the normative benchmark.

However, such active choice mechanisms generally don't work in practice the way they are hypothesized to work in theory.[1] In an active choice enrollment system, choice is either encouraged or even "required," but not choosing is usually also a de facto option, whether or not it is officially acknowledged.[2] In an active choice regime, not choosing triggers an "implicit default." Our empirical analysis shows that such an implicit default is an important element of choice architecture.

We examine the implementation of an active choice regime designed to encourage home delivery for long-term prescription medications in an employer-sponsored prescription drug

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[1] See Sunstein (2015) for a full enumeration of the problems with active choice programs.
[2] Sometimes the option of not choosing is shrouded, as in the active choice regime studied by Carroll et al. (2009).

benefit plan. Under an opt-in regime for home delivery, take-up is very low: only 6% of prescriptions eligible for home delivery are filled through home delivery. The employer's adoption of an active choice regime leads to a sizeable increase in home delivery enrollment. Under the active choice regime, among those eligible for home delivery, 42% actively choose home delivery, 39% actively choose retail pharmacy pick-up, and 19% make no active choice and therefore use retail pharmacy pick-up *without* an insurance subsidy, which is the implicit default. Because home delivery is cheaper than retail pharmacy pick-up for many drugs, the active choice program substantially reduces average out-of-pocket expenditures for individuals with costly long-term prescriptions.

Although there is no variation in the implicit default in our setting, we do observe variation in the cost to individual employees of accepting the implicit default. We find that the likelihood of not choosing increases as the benefit of choosing decreases: Individuals with the smallest insurance subsidies are more likely not to make an active choice and thereby temporarily lose their subsidy. The implicit default generates an incentive to make an active choice *and* becomes the default for those who don't actively choose. For both of these reasons, implicit defaults are a key design feature in active choice programs. By decreasing the attractiveness of the implicit default, institutional designers can encourage active choices, albeit at the cost of decreasing the welfare of those who still refuse to make an active choice.

The Active Choice Intervention

The employer sponsoring the prescription drug plan we study, Company A, is a large national U.S. retailer. Full-time, regular employees of the firm are eligible for healthcare and

prescription drug benefits 90 days after hire.[3] Approximately 85% of benefits-eligible employees at Company A enroll at in the prescription drug plan managed by the pharmacy benefit manager (PBM) that introduced the active choice intervention.

Home delivery has several potential advantages over retail pharmacy pick-up for individuals on long-term maintenance medications. First, home delivery prescriptions are processed in large automated facilities located where land and labor are cheap, so filling a prescription through home delivery is cheaper than filling it at a retail pharmacy (Federal Trade Commission, 2005). These cost savings are shared among beneficiaries, the employer, and the PBM. Home delivery copayments tend to be lower than retail pharmacy copayments (Appendix Table A1). Second, home delivery saves beneficiaries the hassle of making trips to the pharmacy. Third, while retail pharmacy fills for long-term maintenance medications are generally for 28 or 30 days, home delivery fills are typically for 84 or 90 days, reducing the need to remember to refill a prescription. Fourth, the automatic dispensing system used in home delivery has a lower error rate than retail pharmacists counting pills. (However, the error rate is extremely low no matter how a prescription is filled.)

Home delivery is not the best option for all individuals. It would be inappropriate for a short-term prescription needed for an acute condition, such as an antibiotic to treat strep throat. Although the PBM does have pharmacists available for phone consultation, individuals may prefer an in-person conversation with a pharmacist. (On the other hand, some individuals may prefer the confidentiality of speaking to a pharmacist on the phone rather than at the front of a line in a neighborhood pharmacy.) In urban areas, individuals may be reluctant to have prescriptions delivered in the mail if packages are dropped off in a public area that may not be

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[3] With a small number of exceptions, part-time and temporary employees are not eligible for benefits.

secure. The convenience advantage of home delivery may also be lower in urban areas where retail pharmacies are densely located.

Prior to the active choice intervention, the PBM made home delivery available to its beneficiaries on an opt-in basis. Beneficiaries could sign up for home delivery by filling out a paper form, calling a toll-free phone number, or enrolling online. The view of the PBM was that low take-up of home delivery did not reflect a lack of true demand, but rather a lack of consideration or follow-through. Thus, the PBM and Company A adopted an active choice program in 2008.

The active choice program targeted employees who (a) had a retail claim for a prescription maintenance medication[4] within the past 30 days, (b) did not have an unexpired home delivery prescription for that drug on file with the PBM, and (c) had not previously made an active choice to use retail pharmacy pick-up instead of home delivery.[5] The company applied these criteria in the middle of November 2008 and again at the end of November 2008 to identify an initial set of employees to target. Starting on January 1, 2009, the criteria were applied on a daily basis to target additional employees.

The PBM contacted employees several times to explain how the active choice program affected them. In September 2008, letters were mailed to all employees enrolled in the prescription drug plan describing the general nature of the program. In November and December 2008, the initial set of targeted employees received both letters and automated telephone calls with details on the drugs for which they would be required to make an active choice about their

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[4] A maintenance medication is one taken on a regular basis to treat a chronic condition, such as insulin or high blood pressure medication. The PBM was inclusive when defining maintenance medications. For example, ibuprofen was on the list because it is sometimes used regularly for chronic pain.

[5] This third criterion was not relevant when the program was first rolled out. After the initial roll-out, this criterion ensured that employees who had declined home delivery were not contacted again.

future refill channel. Targeted employees were asked to affirmatively indicate to the PBM whether their preference was to continue filling targeted prescriptions at a retail pharmacy or to switch to receiving those prescriptions by home delivery. The benefits of home delivery were described to them, and employees were encouraged to choose home delivery. Those wanting to switch to home delivery could communicate their decision as well as some additional required information to the PBM by mailing a paper form, calling a toll-free telephone number, or visiting a website. Employees were also told that if they did not indicate an active choice in advance, they would have to make an active choice between the retail and home delivery channels after their first two retail pharmacy fills in 2009.

Switching to home delivery additionally required authorization from the prescribing physician(s) for the PBM to fill the relevant prescription(s) by mail. Employees could obtain this authorization independently, or the PBM offered to contact physicians on behalf of (and with the consent of) employees. Individuals targeted for multiple prescriptions could elect home delivery for some and retail pharmacy pick-up for others.

Targeted employees who did not choose home delivery in advance were restricted to two retail pharmacy claims for each targeted drug after January 1, 2009. After each of these claims, the PBM contacted the employee with a letter and an automated phone call telling her of the need to make an active choice between home delivery and pharmacy pick-up via mail, phone, or website. If the employee attempted to make a third retail pharmacy claim for a targeted drug without making an active choice, the insurance claim was rejected at the pharmacy. The pharmacist was asked to explain that the PBM would not accept the claim unless the employee called the PBM to make an active choice. If the employee called the PBM and opted for retail pharmacy pick-up, the employee could proceed with the retail pharmacy fill as intended with the

PBM subsidy. If the employee called the PBM to opt for home delivery, the PBM initiated the process of switching to home delivery, including contacting the employee's physician to authorize the change, and the employee was permitted to receive the PBM subsidy on the intended retail pharmacy pick-up.[6] If an employee did not call the PBM to make an active choice, the employee could either decline to pick up the prescription at that time or pay the full retail cost of the medication out of pocket without any PBM subsidy.

Other than the new incentive to make an active choice, the PBM did not change the pre-existing home delivery and retail pharmacy pickup programs. The PBM had the same mechanisms in place to help individuals with the logistics of switching to home delivery (e.g., contacting physicians) before and after the introduction of the active choice program.

Materials and methods

Data

We use data from two sources. From Company A, we have employee-level demographic and employment data, including gender, age, and original hire date, as well as monthly snapshots of employees' hourly pay, employment category (regular or temporary), scheduled hours (full-time or part-time), and employment status (active or leave of absence). From the PBM, we have claim-level data for each employee enrolled in the prescription drug plan. These data include the National Drug Code (NDC) and Generic Product Identifier (GPI) of the drug, an indicator for whether the drug is generic, the number of days' supply dispensed, the supplier (a retail pharmacy or a PBM mail distribution center), the employee copayment, the cost to the employer

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[6] In about 90% of cases, home delivery was successfully set up before the employee needed another prescription fill, but if there were delays in setting up home delivery (e.g., because of difficulty reaching the physician), the PBM honored additional retail pharmacy claims from the employee.

(total cost of the prescription minus the employee copayment), and the claim date. We also have

from the PBM data on the implementation of the active choice program, including the date on

which an employee-drug class combination was added to the targeted list and information about

rejected claims, including drug identity and the date on which a claim was rejected.

Empirical Approach

To measure the impact of the active choice program, we compare employees who were

targeted for the active choice program to a similar group of employees who were not targeted.

We define the Before (control) cohort as employee-drug class[7] combinations that would have

been targeted had the active choice program been rolled out a year earlier in 2007-2008. The

After (treatment) cohort is employee-drug class combinations targeted when the active choice

program was actually implemented in 2008-2009.

Although we know which employees and prescriptions were actually targeted after the

program was rolled out, identifying the counterfactual Before control group requires creating an

algorithm to select the employee-drug class combinations that would have been targeted had the

program been rolled out in 2007-2008. To ensure that our Before and After cohorts are selected

on a consistent basis, we use this algorithm to identify the employee-drug class combinations

that comprise both cohorts in our analysis.

The selection criteria we use for inclusion in the cohorts are:

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[7] We deem two medications to be in the same drug class if they share the same four-digit Generic Product Identifier (GPI4); drugs with in a GPI4 have similar chemical properties and are used to treat similar conditions. It is not uncommon for individuals to switch between different medications within the same drug class. For example, an individual might switch from a brand-name drug to a generic drug, or from a drug that has proven ineffective to another similar drug.

- Full-time, regular employees at the end of November 2007 (Before cohort) or 2008 (After cohort)

- A retail claim for a maintenance drug was submitted between October 12 and November 26 of 2007 (Before cohort) or 2008 (After cohort).[8]

- No claim for home delivery was submitted from May 1 to November 1 of 2007 (Before cohort) or 2008 (After cohort). The targeting list excludes employee-drug class combinations for which an unexpired home delivery prescription was on file with the PBM. We do not observe home delivery prescriptions on file with the PBM unless they are associated with a home delivery claim,[9] so we use claims as a proxy for prescriptions.[10]

- Employees who had not previously actively indicated that they preferred retail pharmacy pick-up over home delivery. This aspect of the PBM algorithm was only relevant after the initial target sample was selected.

Our algorithm successfully identifies 93% of the employees and 90% of the employee-drug class combinations that were actually targeted in November 2008; of the employees flagged by our algorithm, 95% were actually targeted in November 2008, as were 95% of the employee-drug class combinations.

We make two additional sample restrictions. First, we analyze only 28- and 30-day targeted prescriptions, which make up 95% of the employee-drug class combinations targeted,

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[8] The PBM created its targeting list by identifying employees who, on either November 16 or November 26, 2008, had a retail claim for a prescription maintenance drug in the past 30 days. October 12 is 35 days before November 16, the first targeting date. The additional five days beyond the PBM's 30-day look-back period account for data processing lags and improve the match between the results of our algorithm and the results of the PBM algorithm.
[9] It is possible for a home delivery prescription not to be associated with a home delivery claim, but all home delivery claims are associated with a home delivery prescription.
[10] Our results are similar if we choose a somewhat different look-back period for not having a previous home delivery claim.

since prescriptions of other lengths may differ in important ways. Second, to simplify our analysis and to make it easier to interpret our results, we restrict the bulk of our analysis to employees targeted in November 2008 whom we can observe through October 2009, and employees who would have been targeted in November 2007 whom we can observe through October 2008. This restriction excludes an additional 13% of employee-drug class observations.

After applying the algorithm and sample restrictions, the Before cohort includes 20,486 employees and 38,088 employee-drug class combinations, and the After cohort includes 23,752 employees and 45,600 employee-drug class combinations.[11] We measure outcomes from November of the calendar year in which the cohorts are defined to October of the next calendar year. We are constrained to a one-year period because the active choice program was supplanted by an initiative to more strongly encourage home delivery adoption. Starting on January 1, 2010, employees who had been targeted for the active choice program and had declined home delivery were restricted to two more retail pharmacy claims for each targeted drug at the 2009 copayment; copayments for subsequent retail pharmacy claims doubled. New employees with prescription maintenance medications and existing employees who were starting a newly prescribed maintenance medication were also able to submit two retail pharmacy claims at the 2009 copayment, but they had to pay the full price for subsequent retail pharmacy claims. Details of the 2010 prescription drug plan were announced during Company A's annual open enrollment period, which began on October 26, 2009. Except for a small number of human resources personnel and senior executives, employees did not learn of these changes before that date.

Our primary outcome of interest is the choice between retail pharmacy pick-up and home delivery for each employee-drug class combination. For the After cohort, we additionally

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[11] 13,414 individuals appear in both the 2007 and 2008 cohorts, 7,072 are in the 2007 cohort only, and 10,338 are in the 2008 cohort only.

analyze whether individuals made no active choice. We control for the following variables that are measured as of December 1 of the year in which the cohorts are defined: gender; age; tenure at the company; salary; the number of targeted drugs the employee is taking; and the employee's medication adherence in the drug class[12] over the prior six months as measured by a standard proxy, the proportion of days covered (PDC) calculated according to the approach described in Leslie et al. (2008). The PDC is the fraction of days in a specific time interval in which an individual has a prescribed daily dose of medication available given the individual's refill history and assuming the individual takes the medication as prescribed when it is available.

Because individuals may choose home delivery for its lower copayments, we also control for the potential annual copay savings from switching to home delivery in the case of perfect medication adherence. Appendix A explains how we construct this measure. We calculate an average annual potential savings at the employee-drug class level of $41.51 and $39.69 for the Before and After cohorts, respectively. For generic drugs, potential savings range from $0 (if the retail price is less than the copay) to $12 (the difference between the annual copayments for a generic prescription filled through home delivery rather than retail pharmacy pick-up). About three-quarters of the prescriptions in our data are generics. For other prescriptions, the potential savings may be much higher.

Our final control variables are dummies for the most common indication (MCI) designations, which specify the medical condition that a drug is most commonly used to treat. For example, the MCI for statins is "High Blood Cholesterol," while the MCI for ibuprofen is "Pain and Inflammation." Note that the MCI does *not* necessarily designate the medical condition for which a drug was prescribed for a specific individual. For example, the MCI for

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[12] Calculating PDC at the employee-drug class level ensures that we do not calculate an artificially low PDC if an individual failed to refill one prescription because he switched to another drug within the same drug class.

birth control pills is "Contraceptives," but birth control pills may also be prescribed to treat acne. Each drug has a unique MCI.

Appendix Table A2 presents sample characteristics for the Before and After cohorts as of December 1 of 2007 or 2008, respectively. The two cohorts are very similar in terms of share female (49.9% Before, 49.4% After), average age (46.4 Before, 46.4 After), average salary ($32,191 Before, $32,994 After), and average number of targeted medications (1.86 Before, 1.92 After). The After cohort has higher tenure (5.7 years versus 4.9 years), higher past PDC when it can be calculated (77.4% versus 71.2%), and more cases where PDC cannot be calculated (29.9% versus 19.7%). PDCs cannot be calculated if the prescription is very new or if adherence was extremely low. The last column shows the characteristics of the total employee population at Company A as of December 1, 2008. Relative to the overall employee population, those targeted for the active choice program are more likely to be female, are older, and have been employed at the company longer.

Results

Impact on Home Delivery Take-Up

Figure 1 shows the fraction of individuals in each cohort who filled a prescription through either a retail pharmacy (dashed lines) or home delivery (solid lines) within each calendar month following cohort definition. Less than 2% of the Before cohort uses home delivery in any month during its 12-month observation period.[13] The After cohort starts at a similarly low rate of home delivery utilization, but its utilization rate increases from December

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[13] We had previously noted that 6% of prescriptions were filled through home delivery before the active choice program. The home delivery usage rate is lower in the Before cohort because the targeting criteria exclude prescriptions already being filled through home delivery. In addition, Figure 1 shows the fraction of *individuals* filling a prescription through each channel, rather than the fraction of *prescriptions* filled through each channel.

through March before stabilizing at about 10% of individuals each month for the rest of the year. The increase in home delivery adoption by the After cohort is offset by a corresponding decline in its retail pharmacy fills.

The fraction of individuals with a retail pharmacy fill in any given month declines over time for both cohorts, a pattern commonly observed in the medication adherence literature. For the Before cohort, this decline reflects a combination of medical recovery and imperfect adherence. For the After cohort, an additional source of decline comes from individuals substituting home delivery for retail pharmacy fills. The decline in the retail fill rate is steeper than the increase in the home delivery rate for the After cohort because home deliveries occur less frequently than retail fills due to the 90-day length of home delivery prescriptions versus the one-month length of retail prescriptions. Indeed, the After cohort's home delivery rate increase in excess of the Before cohort's home delivery rate increase is about one-third of the After cohort's retail decline in excess of the Before cohort's retail decline. In untabulated results, we find that PDC is not affected on average by the active choice program.

To understand the correlates of home delivery take-up, we turn to regression analysis. Table 1 presents coefficients from linear probability regressions. In the first column, the dependent variable is a binary indicator for home delivery utilization at any point during the 12-month observation period. The unit of observation is employee-drug class combination. Note that home delivery adoption is a near-absorbing state; less than 3% of individuals who select home delivery subsequently revert back to retail pharmacy pick-up within a given drug class. The only explanatory variable in the first column is an indicator for whether an individual is in the After cohort. Home delivery utilization is 35.6 percentage points higher for employee-drug class combinations in the After cohort relative to the Before cohort ($p < 0.001$).

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The second column adds a rich set of additional covariates to the regression. These covariates have a negligible effect on the estimated active choice effect; the coefficient on the After cohort dummy grows slightly to 36.6% and remains significant ($p < 0.001$). Home delivery utilization is higher for those who are older, have less tenure, and are more highly compensated. It is also higher for individuals who have been more adherent in the past (higher PDC)—hypothetically moving from a 0% PDC to a 100% PDC is associated with a 7 percentage point increase in take-up—and individuals for whom we do not measure a past PDC. Finally, home delivery utilization is higher if the potential savings associated with switching to home delivery are higher, but the magnitude of this effect pales in comparison to the magnitude of the active choice effect; potential savings would have to exceed $4,000 per year to have the same take-up effect as the active choice program.

To examine whether there are heterogeneous effects of the active choice program, the third and fourth columns in Table 1 together show coefficients from a single regression that adds interaction terms between the After cohort dummy and the additional covariates. The interaction terms in the fourth column indicate that active choice has a larger positive impact on those who are older, more recently hired, more highly compensated, taking more targeted drugs, more adherent in the past, and not missing a past PDC.[14]

To obtain regression-adjusted estimates of the timing of home delivery adoption, we expand the regression so that observations are at the employee-drug class-month level. Each month, the dependent variable is whether home delivery had ever been used for this employee-drug class combination in this month or prior. The explanatory variables are the controls in the

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[14] No significance should be attached to the fact that the coefficient on the uninteracted After cohort dummy falls to 0.135, since this represents the estimated effect of active choice on a hypothetical individual who has a zero value for all of the interacted controls.

second column plus calendar month dummies. The last column of Table 1 shows some of the coefficients from this regression, but the main coefficients of interest are those on the calendar month dummies, which are plotted in Figure 2. The coefficients on the dummies for calendar months November 2007 to October 2008 comprise the Before cohort series, and those for calendar months November 2008 to October 2009 comprise the After cohort series.

Home delivery adoption of the Before cohort increases slowly and steadily over time, so that it is about 6 percentage points higher than its November 2007 value at the end of the Before cohort's observation period. The After cohort's home delivery adoption rate increases as well, but most rapidly in February and March of 2009. Figure 3 shows that the number of pharmacy claims rejected for the After cohort peaks at 4,730 in March 2009. Much of the 11 percentage point increase in home delivery adoption in March seems likely to be driven by these rejections, which forced employees to make an active decision about home delivery or pay the full out-of-pocket cost to get their medication. Waiting until rejection at the pharmacy is consistent with both inattention and procrastination. However, there is evidence of forward-looking behavior to preempt rejection in the After cohort as well, since home delivery adoption increased by 5 percentage points in January and an additional 10 percentage points in February, even though only 1 claim was rejected in January and 263 claims were rejected in February.

Up to this point, our analysis has not distinguished between those who actively chose retail pharmacy pickup and those who registered no choice at all with the PBM, thus ending up with the implicit default of retail pharmacy pickup with no subsidy. An important contribution of this paper to the literature on active choice mechanisms is to observe that the act of *not making a choice* is an interesting outcome in its own right.

Figure 4 shows the fraction of individual-drug class observations with an active choice of home delivery in the Before and After cohorts; for the After cohort, we also show the fraction making an active choice for retail pharmacy pickup, and the fraction who retain retail pharmacy pick-up by default because they made no active choice. In the Before cohort, only 6% actively chooses home delivery at some point during the twelve-month observation period; the remainder use retail pharmacy pick-up by default. In contrast, the active choice program is quite successful at encouraging active decision-making in the After cohort, achieving an 81% active decision rate. Active choices are fairly evenly split between home delivery (42%) and retail pharmacy pick-up (39%). The remaining 22% of the After cohort make no choice. This highlights one pitfall of efforts to encourage active choice in settings where it is not technologically feasible to require it: some people may make no choice even when encouraged to do so (and, in this case, even with financial incentives to do so).

How do characteristics of individuals in the After cohort vary with their prescription fill outcomes? In Table 2, we present the average marginal effects from a multinomial logit regression where the unit of observation is the employee-drug class combination. Those who make no choice are more likely to be male, younger, more recently hired, lower-paid, taking fewer medications, and less adherent. Those who choose home delivery are older, have been more recently hired, are more highly paid, take more medications, and are more adherent. Those who actively choose retail are more likely to be female, have longer tenure, are lower paid, take more medications, and are more adherent.

Table 2 also shows that financial incentives matter for making an active choice. Higher annual savings from switching from retail to home delivery significantly decrease the likelihood of making no choice (marginal effect per $100 of savings = -0.0104, $p < 0.05$) and increase the

likelihood of choosing home delivery, although this latter effect is not significant at the 5% level in the multinomial framework (marginal effect = 0.0081, $p = 0.07$). (Recall that in Table 1, where the outcome variable was a binary indicator for whether home delivery was chosen, potential cost savings increased the probability of choosing home delivery in a strongly significant manner.)

Figure 5 further probes the role of financial incentives in encouraging individuals to make an active choice. Recall that if members of the After cohort do not make an active choice, they become liable for the full cost of future prescriptions starting with their third fill of the year. The difference in out-of-pocket costs between having the prescription covered by insurance and not covered is the direct incentive to actively choose. To examine the importance of this incentive, we segment the prescription drug fill choice outcomes for the After cohort by the insurance subsidy given to the last retail claim filled for that employee-drug class combination on or before November 25, 2008. We see in Figure 5 that as the penalty for not choosing actively increases, the frequency of not choosing decreases from 28% (when the insurance subsidy is $0) to 15% (when the insurance subsidy exceeds $68).

Impact on Aggregate Prescription Drug Expenditures

The final outcome that we assess is the impact of the active choice program on aggregate prescription spending by Company A and its employees. The program also likely conferred some financial benefit to the PBM, but we do not have data with which to estimate this effect. Instead of calculating the realized savings over the active choice time period analyzed thus far (November 2008-October 2009), we calculate the annualized cost savings that would accrue to employees and Company A in steady state.

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Following the methodology described in Appendix B, we calculate aggregate annual employee cost-savings of $417,001 and aggregate annual employer cost savings of $283,451, for a total combined cost saving of $700,452. This is a $29.49 savings per capita for Company A plus its employees.

These statistics do not account for any non-pecuniary benefits associated with home delivery (e.g., greater convenience). Nor do they account for the costs of the program to individuals, their physicians, the PBM, or retail pharmacies. These include the cognitive costs to individuals of making an active choice and implementing a change to home delivery for those who decide to switch, the cost to the PBM of operating the active choice program, the cost to physicians of helping patients transfer their prescriptions to home delivery, and the cost to retail pharmacies who lose some of their business to home delivery. For the PBM, the unmeasured benefits presumably outweigh the unmeasured costs, as the PBM has expanded the active choice approach to home delivery enrollment to many of its other clients.

Discussion

This paper adds to the small but growing literature on the effects of active choice mechanisms. Most previous research studies settings in which individuals can be compelled, or at least strongly coerced, to make a choice (e.g., Carroll et al. 2009, Kessler and Roth 2014, Montoy, Dow and Kaplan 2016, Keller et al. 2011). In practice, there are many contexts like the one we study here in which an active choice cannot be required. In these settings, reducing the fraction of individuals who refuse to choose can increase the effectiveness of such interventions at changing outcomes.

We document that in a large-scale implementation of active choice in such a domain that involved nearly 24,000 individuals scattered across the U.S. and required the coordinated action of pharmacists, physicians, and call-center workers, it is possible to achieve a high rate of compliance where 81% of the eligible population makes an active choice. Toft, Schuitema, and Thøgersen (2014) are able to achieve a 75% active choice rate in a small intervention involving 48 individuals presented with the option to adopt energy efficiency upgrades. Josephs et al. (2018) are able to get 71% of 1,808 new hires to click on a "Future Healthcare Planning" link that was presented as mandatory. Putnam-Farr and Riis (2016), who examine click-through rates to e-mail solicitations regarding workplace wellness programs, get much lower active choice rates of 7-13%.

Even though the decision not to actively choose is a relevant outcome, it has received little attention in the literature on active choice interventions. We contribute to this literature by examining the determinants of the no-choice outcome. First, we show that the propensity not to choose is related to observable personal characteristics such as gender, age, salary, and tenure. Second, we show that the probability of making no active choice responds to financial incentives. In our study, individuals with the largest financial benefit from choosing are 13 percentage points more likely to make a choice than individuals with no financial benefit from choosing. Choice architects can thus affect the likelihood of active choosing by selecting the relative costs and benefits of the implicit default that is implemented in the no-choice scenario. A very unattractive implicit default will minimize the probability of no active choice, but at the cost of the ex post welfare of those who still make no active choice. If the penalty associated with the implicit default is especially harsh, it may be difficult to commit to actually implementing it if some individuals fail to choose.

Active choice mechanisms are often implemented in settings where agents are perceived to not be sufficiently taking up a certain option. Consistent with most of the literature, we find that active choice increases take-up of the targeted outcome relative to the opt-in regime, in our case by 36 percentage points. This effect is large compared to the estimated effects of active choice in other contexts. The study most similar to ours is Keller et al. (2011), who find a 10 percentage point increase in take-up of an automatic drug refill option in a setting where active choice can be compelled and there is no financial incentive to choose the automatic drug refill option. Carroll et al. (2009) document that an active choice approach to retirement savings plan enrollment increases participation by 28 percentage points. Stutzer, Goette, and Zehnder (2011) report that active choice increases blood donation rates by 5 to 9 percentage points among those not aware of the importance of donating. Montoy, Dow and Kaplan (2016) find that an active choice approach increases HIV testing rates by 13 percentage points. Josephs et al. (2018) report that active choice increases the rate at which legally binding advance healthcare directives are submitted by 4 percentage points. Toft, Schuitema, and Thøgersen (2014) document that active choice increases adoption of energy efficiency upgrades by 13 percentage points, but this difference is not statistically significant in their small sample. Kessler and Roth (2014) is the one anomaly in this literature, finding that active choice decreases organ donation registration rates by 2-6 percentage points.

The active choice implementation we study was bundled together with an extensive communications campaign. The home delivery take-up effects we observe could in principle be driven by the communications portion of the intervention rather than the active choice portion. Alternatively, it could be that *any* novel policy would have increased home delivery uptake. But the fact that there is relatively little adoption of home delivery before February 2009 despite

multiple communications in Fall 2008 and a sharp increase in adoption as the active choice deadline approached points to active choice being the critical driver of the treatment effect.

Our results suggest that even when home delivery is an option for long-term maintenance medications, many individuals are either unaware that the option exists, have not considered whether they would prefer it to retail pharmacy pick-up, or have procrastinated in implementing their preferences. Stutzer, Goette, and Zehnder (2011), who find that active choice increases blood donation rates only among those who report being not sufficiently informed about the importance of donating blood, argue that active choice facilitates preference formation, which is consistent with the rationale for the active choice intervention that we study in this paper.

Note that any active choice mechanism will impose decision-making costs on individuals who choose to choose (Sunstein, 2015). We do not have any information on the magnitude of those costs in the context that we study here. Ideally, the decision-making costs of active choice would be considered when assessing whether such interventions are appropriate.

Conclusion

We evaluate an active choice program that encourages employees on a long-term prescription to actively choose whether to fill that prescription through home delivery or at a retail pharmacy. Even though compliance with the requirement to actively choose was incomplete, it increased the fraction of employees using home delivery by 36 percentage points relative to a baseline home delivery utilization rate of 6 percent under the pre-existing opt-in regime. Economic incentives matter: individuals with a higher expected level of cost savings from switching to home delivery are more likely to choose home delivery. Making no choice is also an important outcome: 19% of those required to make an active choice to preserve their

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pharmacy insurance subsidies fail to do so. Individuals with little financial incentive to make an active choice are much less likely to do so. The implicit default that is implemented when no active choice is made is an important part of choice architecture in active choice regimes because the implicit default serves as an incentive to choose *and* as the default for those who don't.

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Appendix A: Calculating the Savings to Individuals from Switching to Home Delivery

To calculate the potential cost savings to an individual from filling a claim through home delivery rather than at a retail pharmacy, we have to impute what the copay would have been had the claim been filled through the alternative channel. This is a measure we do not directly observe. Instead, we impute the counterfactual copay from other similar claims. We now describe this procedure.

Some individuals have claims for the same medication via both a retail pharmacy and home delivery. For these individuals, we could use their own claims history to construct a counterfactual copay (we call this "individual imputation"). However, many individuals in our data only fill claims through either retail or home delivery. For these individuals, we do not have the data necessary for individual imputation. As an alternative, we could construct a counterfactual copay from the average of claims filled by everyone in our data who takes the same medication but uses the other delivery channel (we call this "average imputation"). This approach will result in fewer missing values for our "counterfactual copay" variable, but also likely introduces greater measurement error at the individual level.

One approach to imputing the counterfactual copay would be to do the imputation at the highest level of specificity possible—that is, to impute at the individual level if possible, and at the average level otherwise. This approach is problematic because the Before and After cohorts would not be treated symmetrically. Many more individuals in the After cohort have both retail

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and home delivery claims, so there would be a dramatically higher individual imputation rate for the After cohort. To make the imputation comparable across both cohorts, we instead use average imputation to calculate counterfactual copayments for everyone.

A limitation of this second approach is that if an individual is taking an uncommon medication, there may not be anyone else in the data who fills that medication in the same formulation and dosage through the other delivery channel. For these claims, we impute the copay using the nearest similar claim or claims in the data, where we define similarity from either the NDC or GPI code for the medication. An NDC (National Drug Code) is a unique 10-digit code assigned to each medication. Each code is divided into three segments. The first segment identifies the drug labeler (e.g., the manufacturer or distributor). The second segment identifies the strength, dosage form, and formulation of the drug. The third segment identifies the package form and size. The GPI (Generic Product Identifier) is a 14-digit hierarchical drug classification code with increasingly specific information contained in each pair of digits. The first six digits identify the drug's group, class, and subclass. Digits 7-10 identify the drug's generic name and extension, and digits 11-14 identify the drug's dosage and form.

We impute counterfactual copayments using the highest level of specificity available. We are able find a retail match for the vast majority of home delivery claims and vice versa using either an exact NDC (which we look for first) or an exact GPI code. If we lack an exact NDC or GPI match, we turn to drugs in our data that share the same 12-digit GPI code. If we still lack a match, we then turn to drugs that share the same 10-digit GPI code, and so on, until we reach a level of GPI aggregation that allows us to compute a copayment in the counterfactual delivery channel for nearly all observations.

Because individuals in the After cohort are more likely to have claims filled through both retail and home delivery than those in the Before cohort, they are also more likely to have a claim for which we can find an exact NDC match with which to compute a counterfactual copay: we can simply use their own claim. Thus, taking an average of the copayments over all claims at a given level of drug comparability might result in less measurement error in the imputation process for the After cohort than the Before cohort. To make the imputation process more comparable across the two cohorts, we exclude an individual's own claims filled through the other delivery channel when calculating the average counterfactual copayment for a given claim.

To avoid introducing measurement error from the fact that retail drug prices change over time, we only impute copayments for the Before cohort from claims submitted in 2007-2008, and for the After cohort from claims submitted in 2008-09.

Potential savings for a home delivery claim are then derived by first calculating the imputed retail copay minus the actual home delivery copay for each claim. We then average this measure across all the claims submitted during the relevant observation period for each individual-drug class combination. Finally, we take this measure of average savings and annualize it, assuming perfect adherence, to obtain the ultimate measure of potential savings used in our analysis.

Appendix B: Calculating the Aggregate Employee and Employer Savings from the Active Choice Program

We calculate the aggregate reduction in prescription drug expenditures for both employees and their employer as a result of the active choice program. Consumers benefit from lower home delivery copays, while the employer benefits from the lower average prices net of

copays of prescriptions filled through home delivery. For the aggregate savings calculation, we use all home delivery claims submitted from November 1, 2008, through December 31, 2009, for drugs targeted by the active choice program.

The approach we use to calculate employee cost savings is more straightforward than that described in Appendix A because we are calculating cost savings only for the After cohort, for whom we have a complete and accurate copayment schedule. We thus do not need to maintain consistency with calculations for the Before cohort.

For individuals for whom we observe both a retail and a home delivery claim for a prescription with the same NDC, we use the individual-level average of the retail claim prices as the counterfactual retail price for the home delivery claims. We split the total counterfactual cost between the employer and employee using the copay schedule shown in Table A1. When we don't have an individual-specific retail NDC match for a home delivery claim, we use a modification of the imputation procedure described in Appendix A.

Possible imputation methods combine (1) the choice between using only an individual's own retail claims versus retail claims from everyone else in the sample with (2) the level of drug identification specificity that is used (NDC, GPI, or substrings of GPI). For each drug, we rank the imputation methods by accuracy as follows. We first only look at the home delivery claims for which we have an individual-level retail NDC match. For those claims, we can calculate the average counterfactual retail costs on an individual-NDC level, which we will call the "benchmark." We then see what counterfactual retail cost would be calculated under each imputation method if we did not have an individual-level retail NDC match available. We rank the imputation methods, separately for each NDC, by the absolute average deviation between the benchmark retail price and the imputed retail price.

Having ranked the imputation methods, we impute counterfactual retail costs of each home delivery claim for which we do not have an individual-level retail NDC match, using the highest-ranked imputation method that can be used for that claim. Note that this means the imputation method can vary across drugs within an individual.

With the imputed counterfactual retail price and copayment for each home delivery claim, we compute the employer and employee savings for each home delivery claim, convert this to a savings per day, add up the daily savings for the period of time since an employee switched to home delivery for a given drug class, and then annualize this amount to compute a yearly measure of savings attributable to the active choice program.

There are three further adjustments that we make in arriving at our final cost savings measure. The first is an adjustment for prescriptions that are not completely used up by the end of the year and the possibility that some individuals are stockpiling prescriptions. For example, if an employee filled a 90-day home delivery prescription on December 1, 2009 and took it as prescribed, there would be a two-month supply left at the end of the year, although this employee may not be stockpiling. If an employee filled a home delivery claim every two months of the year instead of every three months of the year starting at the beginning of the year, and took the medication as prescribed, there would be a 6-month supply left at the end of the year and this employee would appear to be stockpiling. In either case, it would be inaccurate to assign the entirety of the cost savings associated with the leftover medication at year-end to the period over which we are calculating cost savings. For medications for which there is less than a month's supply left at the end of the year, we make no adjustments to the home delivery cost savings, since filling prescriptions through the retail channel could lead to similar amounts of excess supply. For medications for which there is more than a month's supply left at the end of the year,

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we scale down our cost savings estimate proportionally so that we attribute savings to no more than one month of excess supply.

The second adjustment we make accounts for employees who were already using home delivery before the implementation of the active choice program. The cost savings associated with their home delivery claims should not be attributed to the program. We therefore drop the home delivery claims of individuals who submit only home delivery claims in the six months prior to November 1, 2008, when the initial targeting for the active choice program was done.

The third adjustment accounts for the fact that some individuals would have switched from retail to home delivery even in the absence of the active choice program. The analysis shown in Figure 2 suggest that roughly 15% of the After cohort who switched to home delivery would have done so in the absence of the active choice program. We simply reduce the total cost savings measure that we calculate by 15% to account for the switching that would have occurred in the absence of the program.

Our estimate of the total cost savings after all adjustments are made is $417,001 for employees and $283,451 for the employer, for a total cost savings of $700,452.
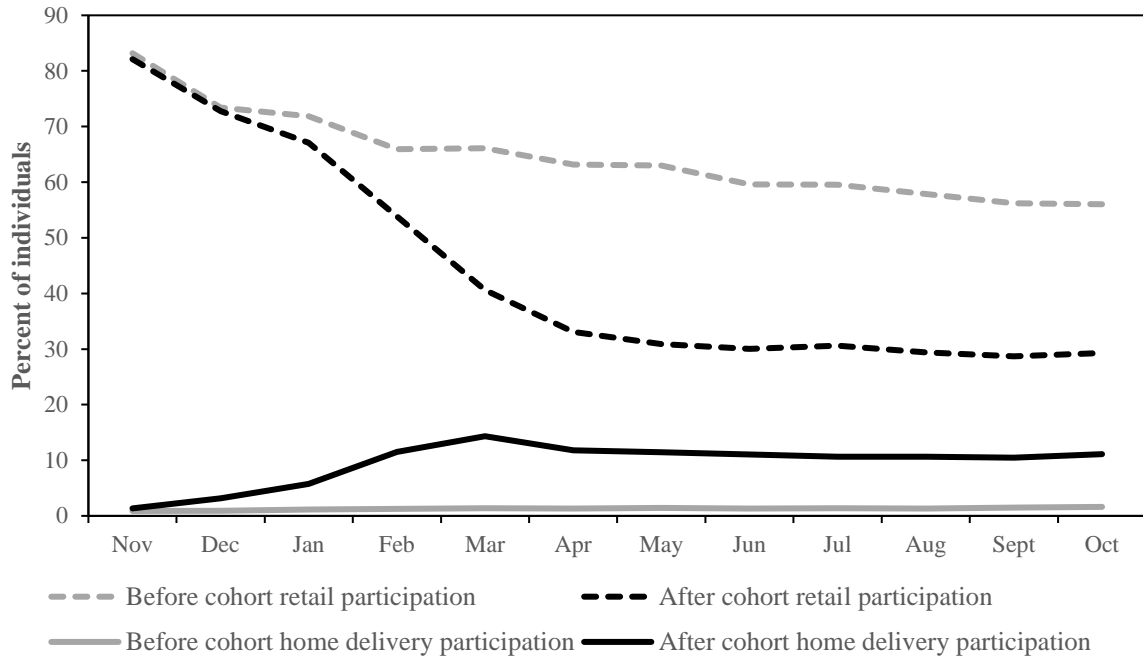
Figure 1. Home delivery and retail pharmacy utilization. The figure plots the percent of individuals with at least one retail pickup (dotted lines) or one home delivery (solid lines) within each month of the 12-month observation period. The unit of observation is the individual. Source: Authors' calculations.
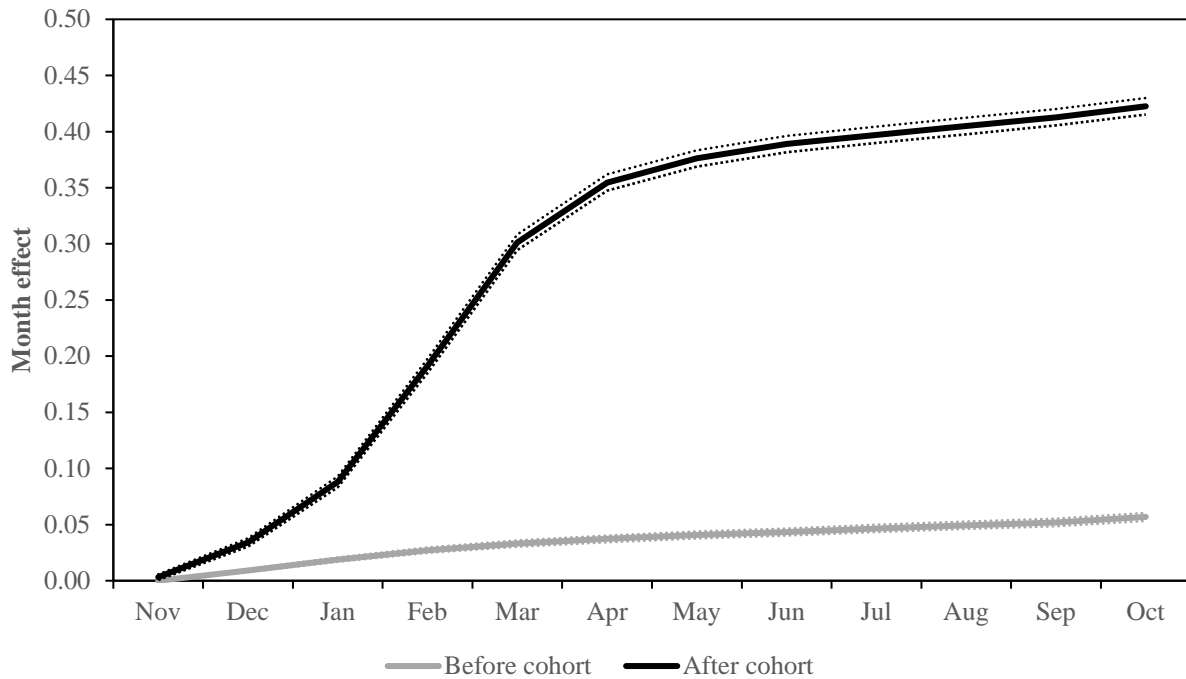
Figure 2. Regression-adjusted adoption of home delivery. The monthly adoption rate comes from the calendar month dummies in the regression specification shown in the last column of Table 1, where the dependent variable is an indicator for whether home delivery has ever been used for the given employee-drug-class in the current month or prior. November 2007 is the omitted category. Dotted lines indicate 95% confidence intervals. Source: Authors' calculations.
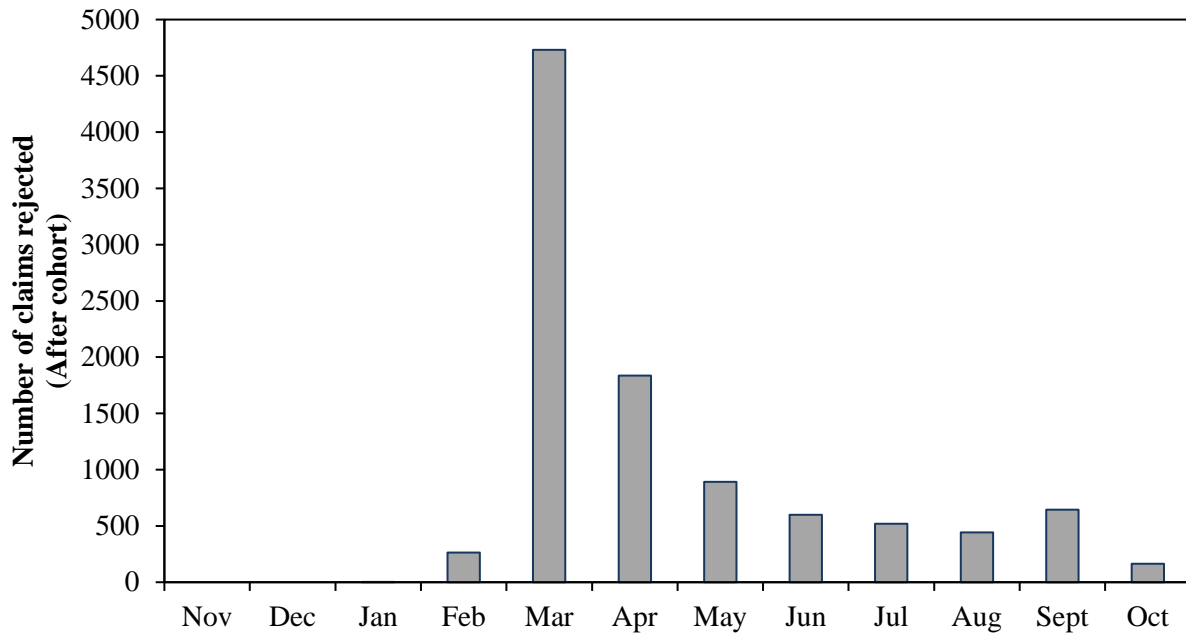
Figure 3. Pharmacy claim rejections. This figure shows the number of prescription claims rejected due to failure to make an active choice for the After cohort in each month from November 2008 to October 2009. Source: Authors' calculations.
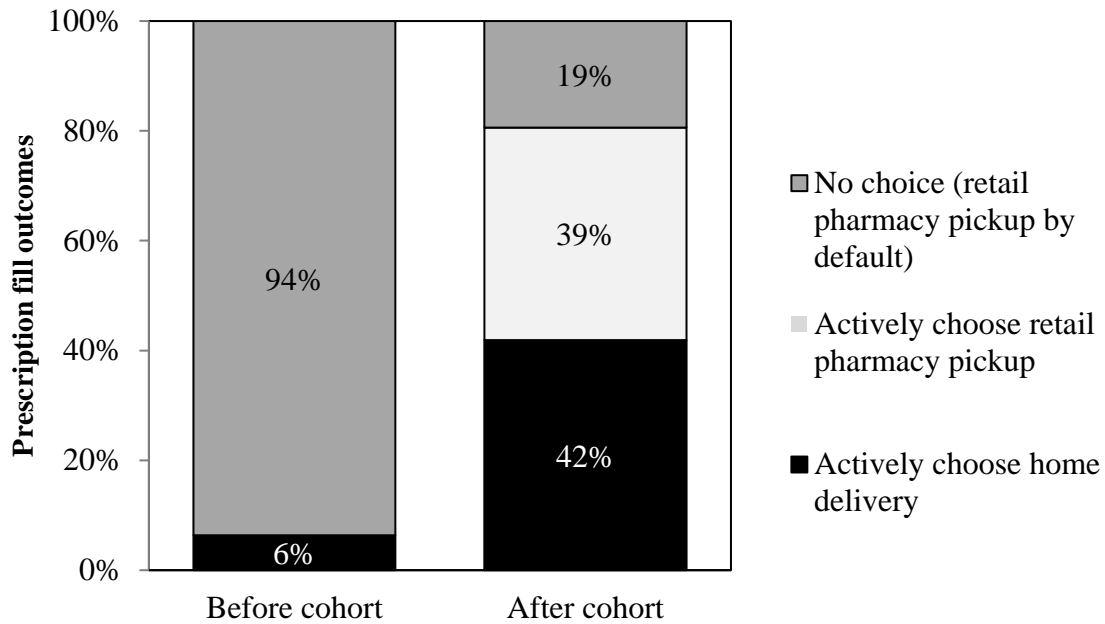
Figure 4. Distribution of prescription fill outcomes for the Before and After cohorts. This figure shows the fraction of employee-drug class combinations in the Before and After cohorts that, during the cohort's 12-month observation period, (1) never makes an active choice for retail pharmacy pickup and never receives a prescription through home delivery, (2) actively chooses retail pharmacy pickup and never receives a home delivery, or (3) receives a prescription through home delivery at some point. Source: Authors' calculations.
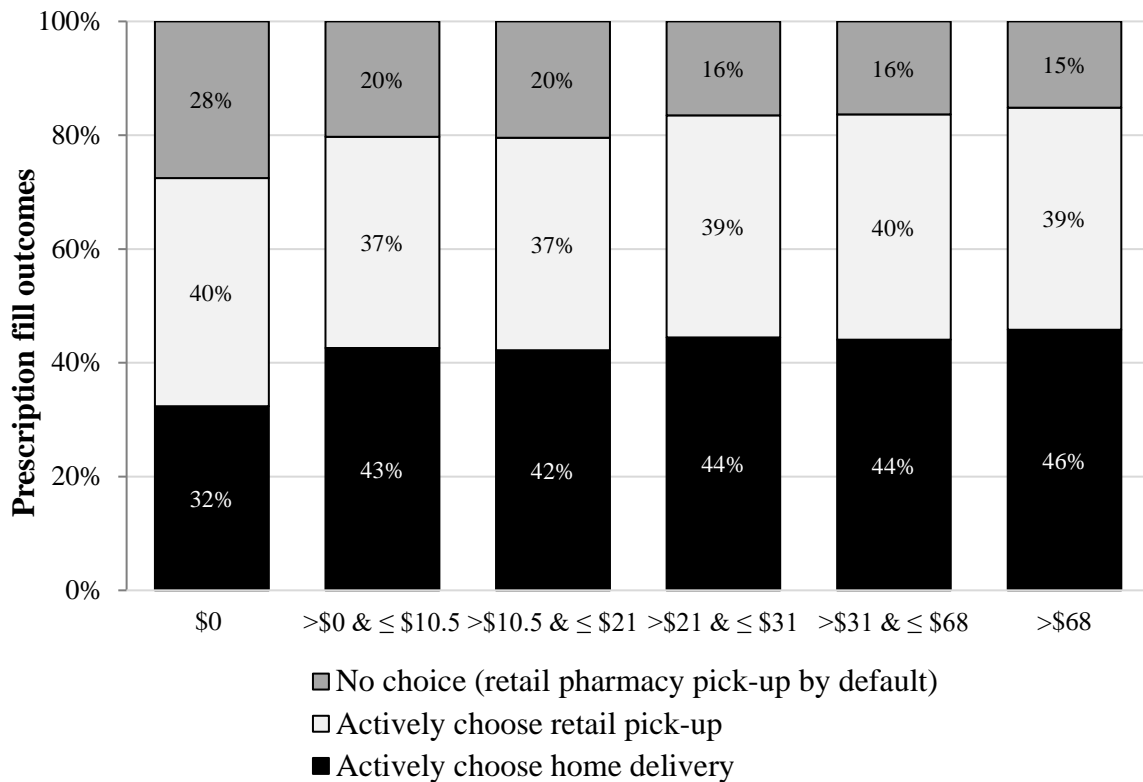
Figure 5. Prescription fill outcomes for the After cohort by prescription savings. This figure shows the fraction of employee-drug class combinations in the After cohort that (1) never makes an active choice for retail pharmacy pickup and never receives a prescription through home delivery, (2) actively chooses retail pharmacy pickup and never receives a home delivery, or (3) receives a prescription through home delivery at some point in the cohort's 12-month observation period. The sorting variable on the horizontal axis is the amount of the insurance subsidy for the last retail claim filed for the employee-drug class combination before November 26, 2008. The bins are chosen so that each contains approximately the same number of observations. Source: Authors' calculations.

Table 1. OLS Regression Results for Home Delivery Adoption

The first four columns of the table report coefficients from OLS regressions where the dependent variable is a binary indicator for home delivery utilization at any point during the 12-month observation periods for the Before and After cohorts. The unit of observation is the individual-drug class combination. The last column reports coefficients from an OLS regression where the unit of observation is the employee-drug class-month combination. The dependent variable is a binary indicator for whether home delivery had ever been used for this employee-drug class combination in the current month or prior. The third and fourth columns are reporting coefficients from a single regression; the fourth column shows the coefficients from interacting the main explanatory variables in the third column with the After cohort dummy. Standard errors are clustered at the individual level.

	No controls	With controls	Main effects	After cohort interactions	Ever filled home delivery
			Interaction specification		**Ever filled home delivery**
After cohort	0.3556**	0.3661**	0.1352**		
	(0.0040)	(0.0042)	(0.0386)		
Female		-0.0067	-0.0046	-0.0044	-0.0047
		(0.0049)	(0.0042)	(0.0090)	(0.0036)
Age		0.0032**	0.0016**	0.0028**	0.0025**
		(0.0002)	(0.0002)	(0.0004)	(0.0002)
Tenure ($<$ 2 yrs. omitted)					
2 to $<$ 5 years		-0.0285**	-0.0129*	-0.0252*	-0.0236**
		(0.0061)	(0.0056)	(0.0120)	(0.0044)
5 to $<$ 10 years		-0.0305**	-0.0202**	-0.0135	-0.0242**
		(0.0065)	(0.0059)	(0.0127)	(0.0047)
10+ years		-0.0479**	-0.0233**	-0.0404**	-0.0343**
		(0.0076)	(0.0067)	(0.0142)	(0.0055)
Salary (1st quart. omitted)					
Salary 2nd quartile		0.0079	0.0063	0.0041	0.0057
		(0.0061)	(0.0050)	(0.0117)	(0.0044)
Salary 3rd quartile		0.0203**	0.0202**	0.0019	0.0153**
		(0.0064)	(0.0056)	(0.0117)	(0.0047)
Salary 4th quartile		0.0579**	0.0339**	0.0420**	0.0430**
		(0.0068)	(0.0059)	(0.0126)	(0.0049)
Number of targeted drugs		0.0011	-0.0046**	0.0099**	0.0017
		(0.0018)	(0.0014)	(0.0031)	(0.0013)
Past PDC		0.0007**	0.0002**	0.0009**	0.0006**
		(0.0001)	(0.0001)	(0.0002)	(0.0001)
Past PDC missing		0.0388**	0.0794**	-0.0473**	0.0353**
		(0.0062)	(0.0059)	(0.0135)	(0.0044)
Potential annual savings ($100s)		0.0079**	0.0098**	-0.0032	0.0065**
		(0.0027)	(0.0022)	(0.0048)	(0.0020)
MCI controls	No	Yes	Yes		Yes
Calendar month fixed effects	No	No	No		Yes
R^2	0.164	0.185	0.193		0.202
Sample size					
Employees	$N = 30,824$	$N = 29,908$	$N = 29,908$		$N = 29,908$
Employee-drug class	$N = 83,688$	$N = 80,988$	$N = 80,988$		$N = 971,856$

* Significant at 5% level. ** Significant at 1% level.

**Table 2. Multinomial Logit Regression Results for Prescription Fill Outcomes
in the After Cohort, Average Marginal Effects**

This table shows average marginal effects from a multinomial logit regression where the sample is the After cohort and the dependent variable is whether the employee-drug class combination receives a prescription through home delivery at some point in the cohort's 12-month observation period, actively chooses retail pharmacy pickup and never receives a home delivery, or never makes an active choice for retail pharmacy pickup and never receives a prescription through home delivery.

	Prescription fill outcome		
	Home delivery	Retail	No choice
Female	-0.0096	0.0230**	-0.0134**
	(0.0082)	(0.0085)	(0.0052)
Age	0.0042**	-0.0007	-0.0036**
	(0.0004)	(0.0004)	(0.0002)
Tenure ($<$ 2 yrs. omitted)			
2 to $<$ 5 years	-0.0413**	0.0334**	0.0079
	(0.0110)	(0.0113)	(0.0067)
5 to $<$ 10 years	-0.0357**	0.0464**	-0.0107
	(0.0115)	(0.0119)	(0.0070)
10+ years	-0.0630**	0.0868**	-0.0238**
	(0.0127)	(0.0133)	(0.0079)
Salary (1st quartile omitted)			
Salary 2nd quartile	0.0093	-0.0084	-0.0009
	(0.0107)	(0.0112)	(0.0069)
Salary 3rd quartile	0.0202	-0.0086	-0.0116
	(0.0108)	(0.0113)	(0.0070)
Salary 4th quartile	0.0738**	-0.0508**	-0.0230**
	(0.0114)	(0.0118)	(0.0071)
Number of targeted drugs	0.0113**	0.0264**	-0.0377**
	(0.0027)	(0.0030)	(0.0023)
Past PDC	0.0011**	0.0010**	-0.0021**
	(0.0001)	(0.0001)	(0.0001)
Past PDC missing	0.0348**	0.0257*	-0.0605**
	(0.0128)	(0.0130)	(0.0069)
Potential annual savings ($100s)	0.0081	0.0023	-0.0104*
	(0.0044)	(0.0044)	(0.0046)
MCI controls		Yes	
Pseudo R^2		0.055	
Sample size			
Employees		$N = 23,126$	
Employee-drug class		$N = 44,139$	

* Significant at 5% level. ** Significant at 1% level.

Appendix Table A1. Prescription Drug Copayment Schedule for Company A in 2009

Type of prescription drug	Retail, 30 day prescription	Home delivery, 90 day prescription	Home delivery savings, 90 days
Generics (some)	$0	$0	$0
Generics (most)	$8	$20	$4
Preferred brand, no generic available	$25	$55	$20
Preferred brand, generic available	35% of full cost $35 min/$70 max	35% of full cost $70 min/$140 max	$35 min/$70 max
Non-preferred brand	35% of full cost $45 min/$105 max	35% of full cost $90 min/$210 max	$45 min/$105 max
Non-preferred brand, excl. lifestyle drugs	35% of full cost $70 min/$140 max	35% of full cost $140 min/$280 max	$70 min/$140 max
Non-preferred brand, lifestyle drugs	85% of full cost	80% of full cost	5% of full cost
Smoking cessation products	20% of full cost	20% of full cost	$0
Fertility regulation products	50% of full cost	50% of full cost	$0

The savings listed are those due solely to differences in the copay formulas. There could also be differences in the full price of retail or home delivery prescriptions that cause the actual savings to be higher or lower than the number listed. Source: Pharmacy Benefits Manager.

Appendix Table A2. Sample Characteristics for Company A

	Before cohort Dec 1, 2007	After cohort Dec 1, 2008	All employees Dec 1, 2008
A. Employee characteristics			
Female	49.9%	49.4%	37.0%
Race			
White	58.9%	61.4%	56.8%
Black	5.2%	5.6%	8.3%
Hispanic	2.1%	2.5%	6.2%
Missing	32.9%	29.4%	26.9%
Age (years)	46.4	46.4	39.7
Tenure (years)	4.9	5.7	3.9
Mean salary	$32,191	$32,994	$31,227
Number of targeted drug classes	1.86	1.92	Not available
Sample size	$N = 20,486$	$N = 23,752$	$N = 166,221$
B. Employee-drug class characteristics			
Potential annual savings	$41.51	$39.69	Not available
Past Proportion of Days Covered (PDC)	71.2%	77.4%	Not available
Past PDC missing	19.7%	29.9%	Not available
Sample size	$N = 38,088$	$N = 45,600$	Not available

Source: Authors' calculations.