Online Appendix for:

# The Behavioral Foundations of Default Effects: Theory and Evidence from Medicare Part D

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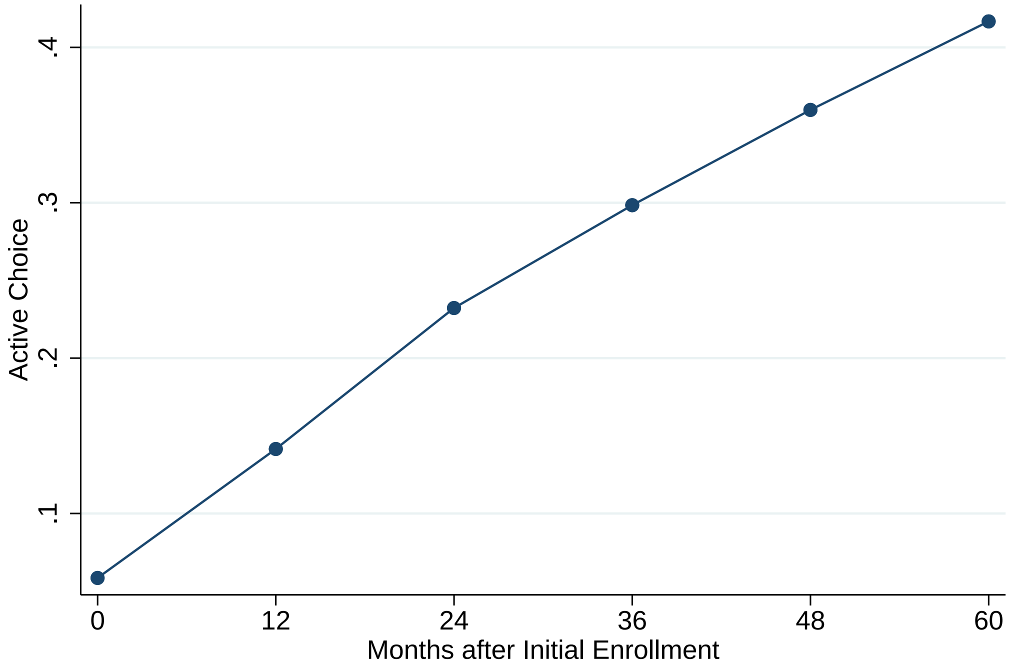
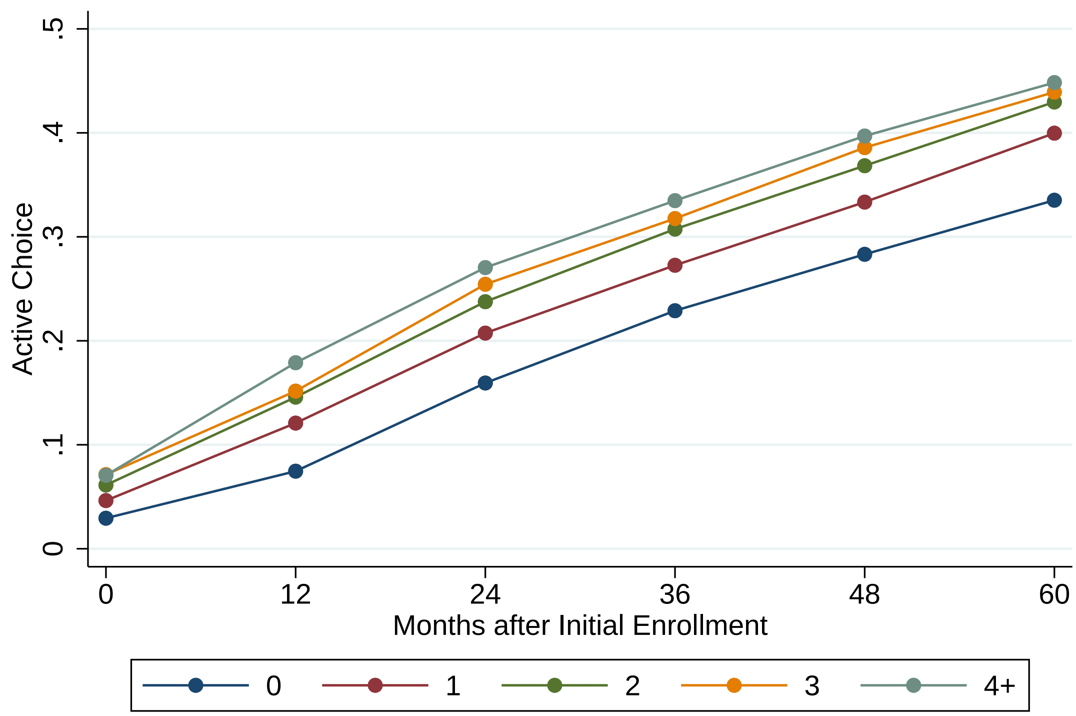
# A Appendix: Additional Figures and Tables

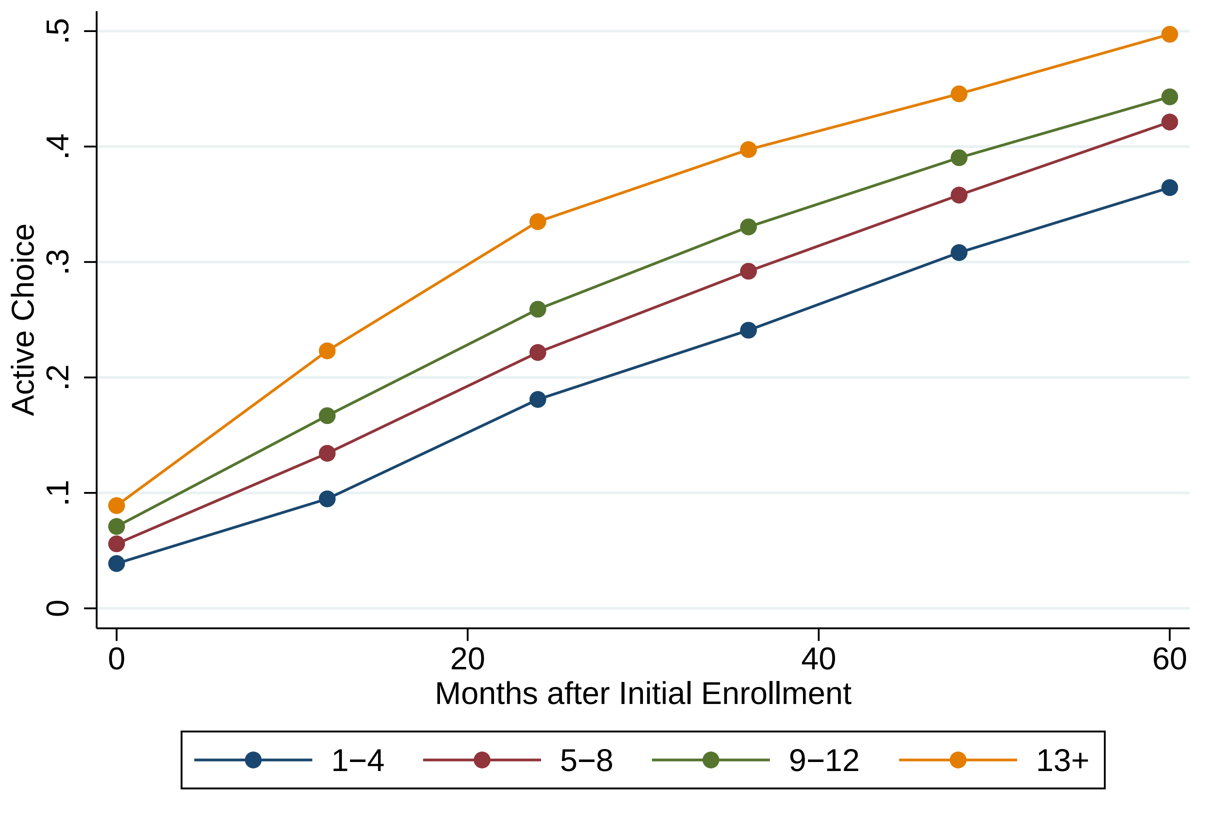
**Appendix Figure A1:** NY and TX Medicaid-Linked Sample



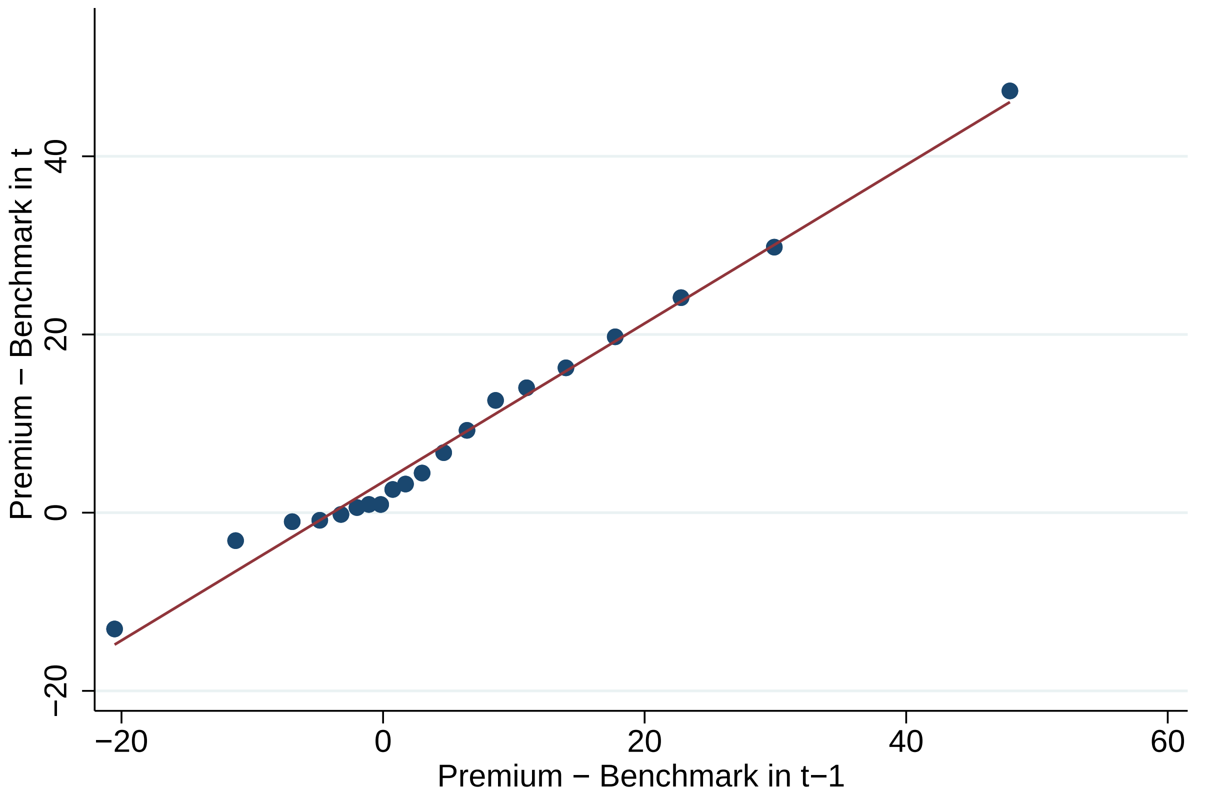

*Notes*: Figure plots the cumulative active choice propensity for LIS beneficiaries following their initial Medicare enrollment (at age 65). Panel (a) is based on the sample of all LIS beneficiaries in the U.S. who turned 65 in 2007 - 2010. Panel (b) is based on a subsample of those living in New York and Texas, and who were enrolled in Medicaid at age 64 due to disability before enrolling in Medicare.

**Appendix Figure A2:** Cumulative Active Choice Propensity among New Age-65 LIS Beneficiaries By Elixhauser Comorbidity Index, Measured at Age 64

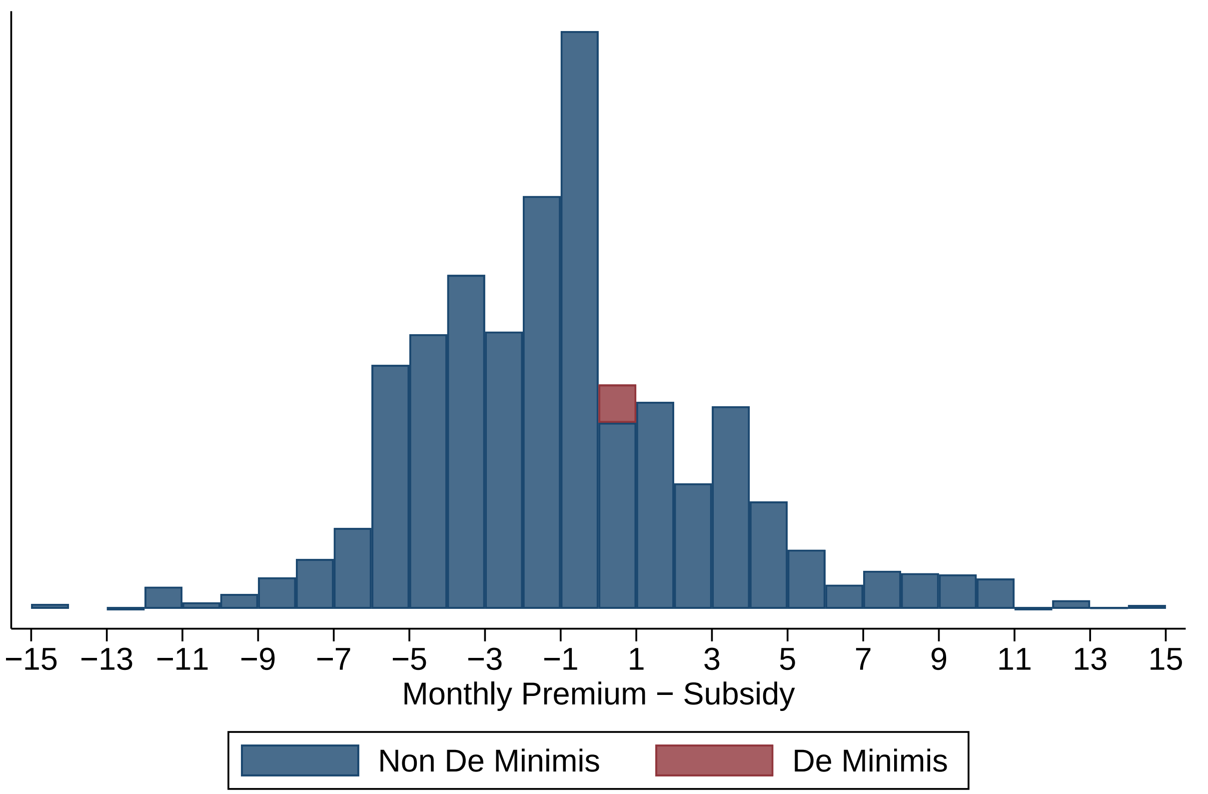




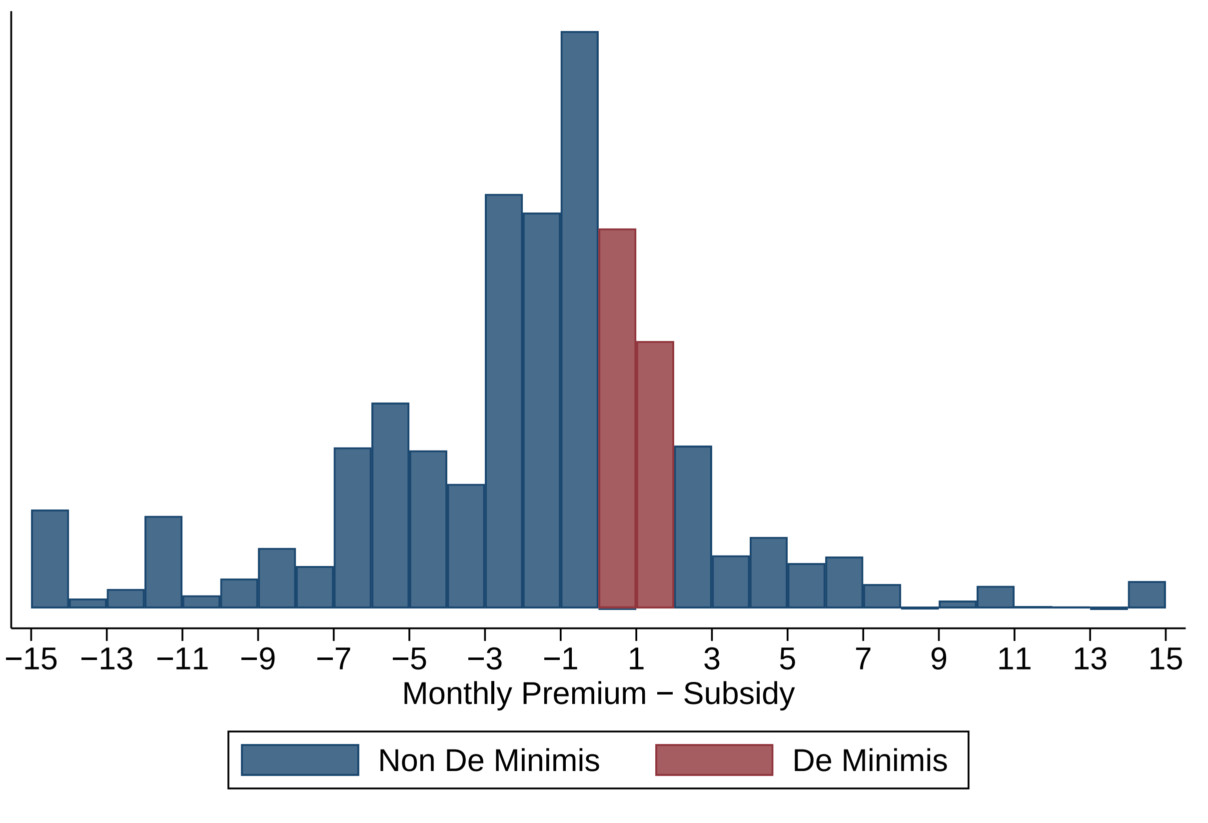

**Appendix Figure A3:** Cumulative active choice propensity for our Medicaid-linked sample by number of unique drugs taken at 64.




**Appendix Figure A4:** This figure plots premiums in $t-1$ against premiums in $t$ for all plans in our data that existed in consecutive year pairs.



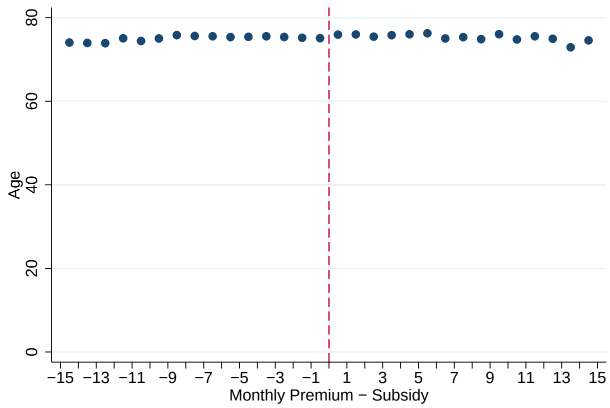

**Appendix Figure A5:** This figure replicates Figure 2, restricting only to 2007-2010, before the de minimis rules were implemented.



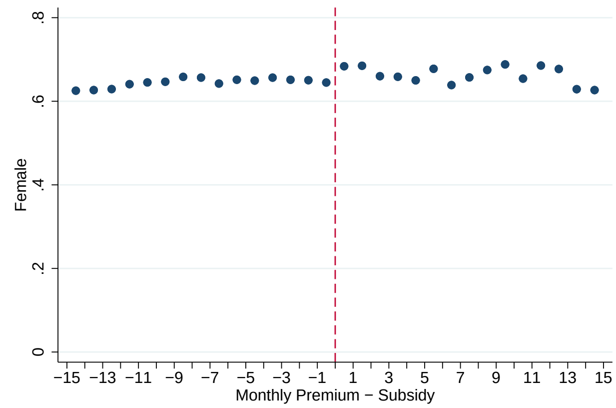

**Appendix Figure A6:** This figure replicates Figure 2, restricting only to 2011 onwards, after the de minimis rules were implemented.

**Appendix Figure A7:** Regression Discontinuity Balance Tests for Observable Beneficiary Characteristics

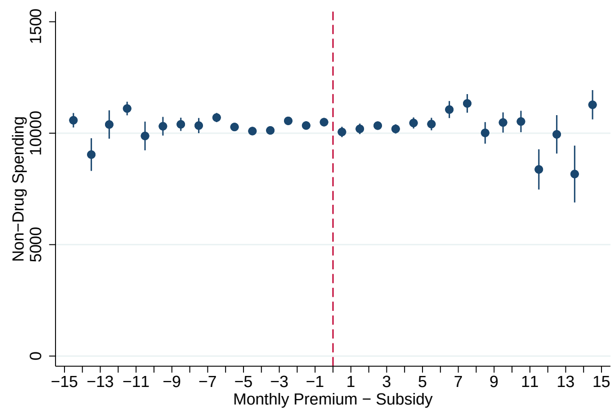
**(a)** Beneficiary Age



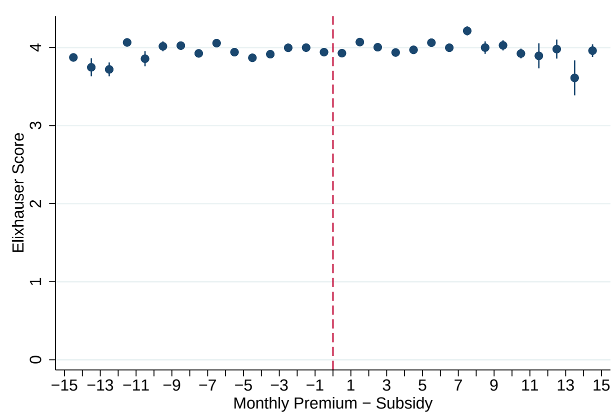

**(b)** % Female



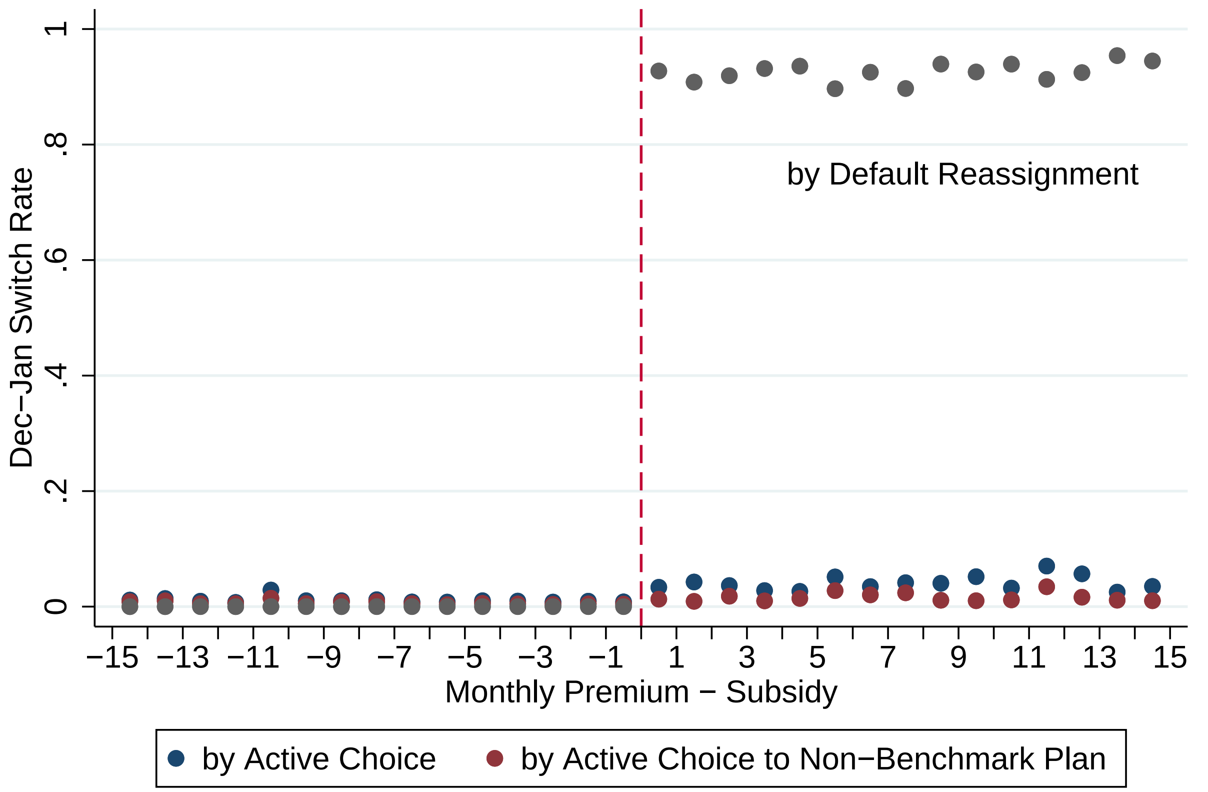

**(c)** Prior Non-Drug Medical Spending



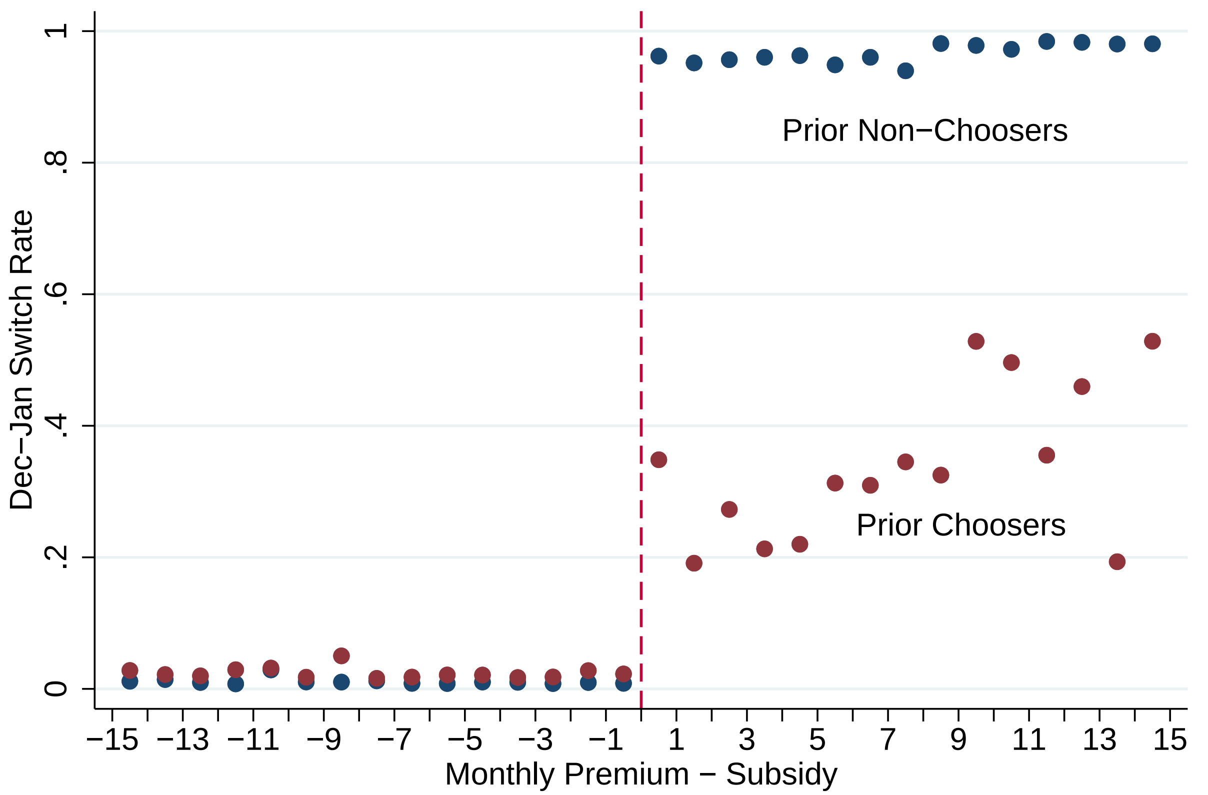

**(d)** Elixhauser Comorbidities



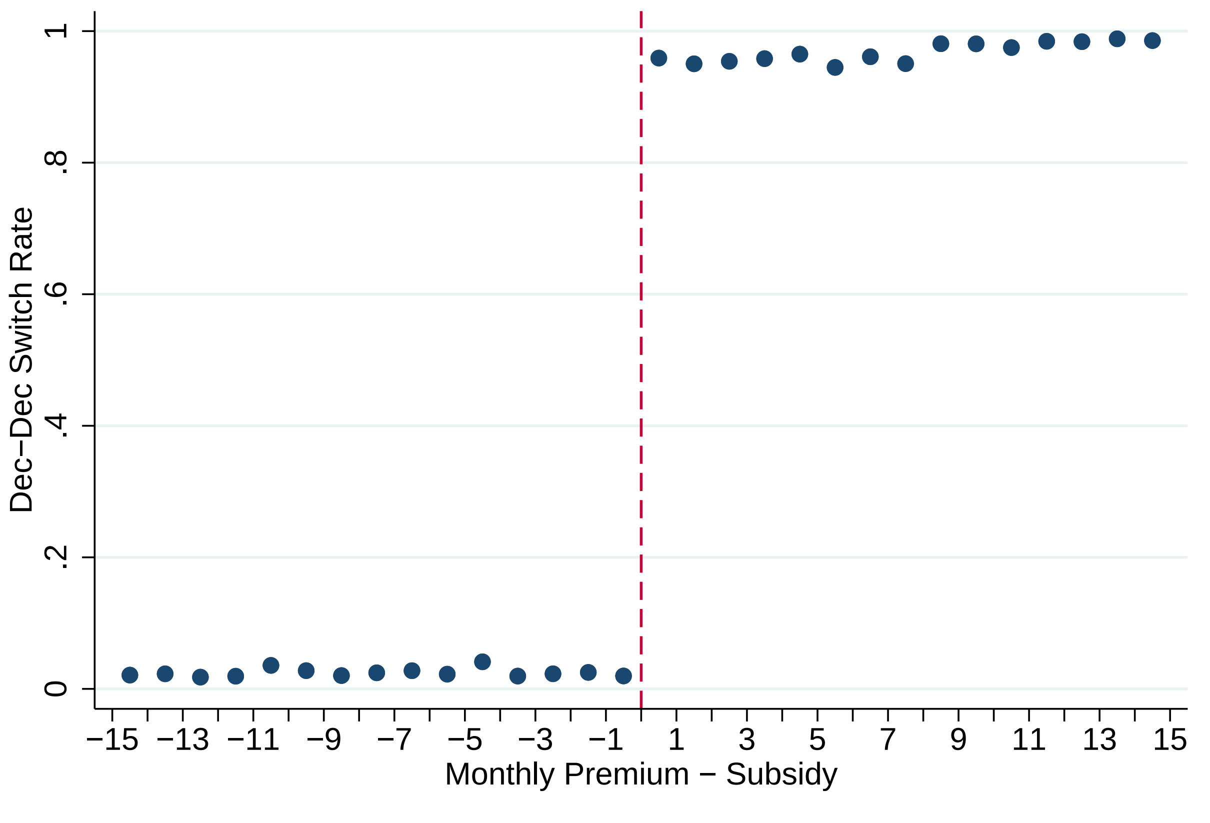

*Notes*: This figure plots average beneficiary characteristics in the prior year, for whole-dollar bins of the difference between their incumbent plans' monthly premium bid in the current year and the regional LIS premium subsidy in the current year. We use the 'RD Analysis Sample' described in Table 1. Panel (a) reports average beneficiary age, Panel (b) reports the share of female beneficiaries, Panel (c) reports average non-drug spending on medical services in the prior year, and panel (d) reports the average Elixhauser Comorbidity Index.




**Appendix Figure A8:** We break down the outcome in Figure 4 by whether the plan switch was an active choice, an active choice specifically to a non-benchmark plan, or was a reassignment through the default mechanism.



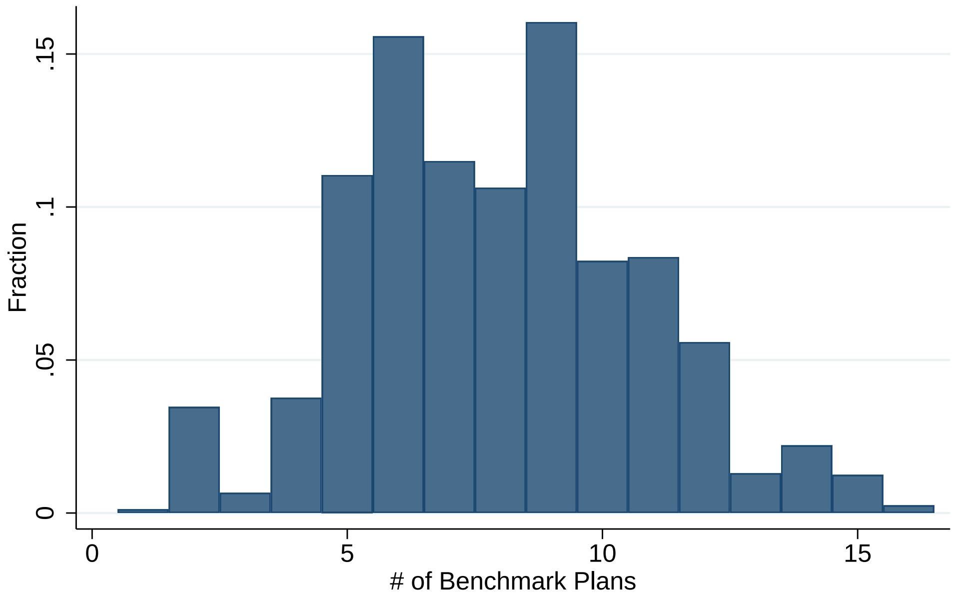

**Appendix Figure A9:** This figure replicates Figure 4, but additionally plots switching rates among those who had actively chosen their incumbent plan.



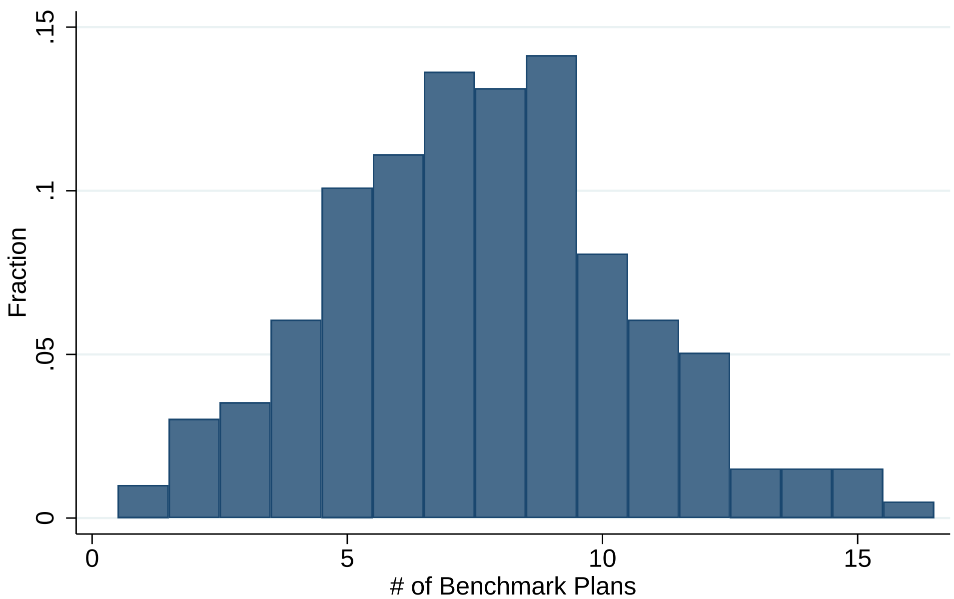

**Appendix Figure A10:** Probability of switching plans between December of $t-1$ and December of $t$, by incumbent plan premium in $t$.

**Appendix Figure A11:** Distribution of Number of Benchmark Plans in Market-Year

**(a)** Weighted by Number of Beneficiaries



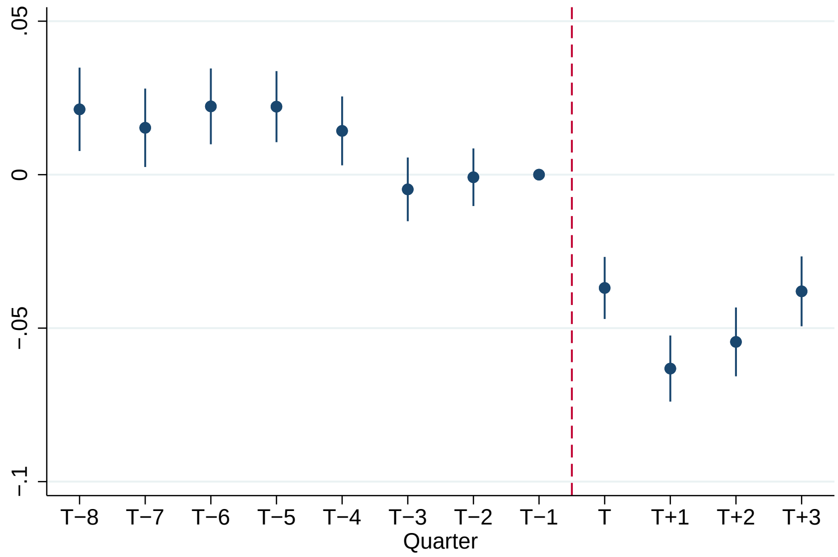

**(b)** Unweighted



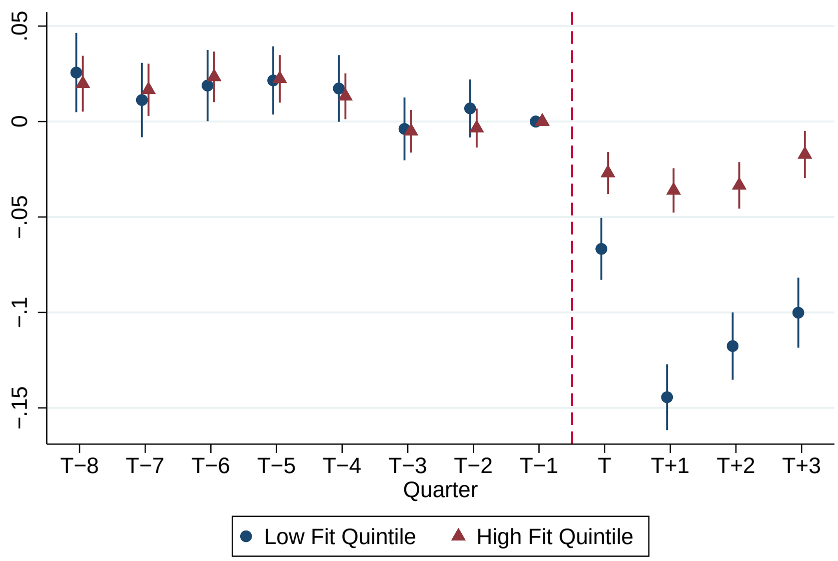

*Notes*: This set of figures plots the distribution in the number of benchmark plans across the combination of Part D market region-years. The top figure presents this distribution weighing all Part D market region-years equally, while the bottom weighs Part D market region-years by the number of beneficiaries in our sample enrolled under each.

**Appendix Figure A12:** Event Study – Effect of Change in Default from Remaining in Incumbent Plan to Auto-assignment to Randomly-Chosen Default Plan on Log Prescription Drug Spending, with Prices Normalized by Drug

**(a)** Overall

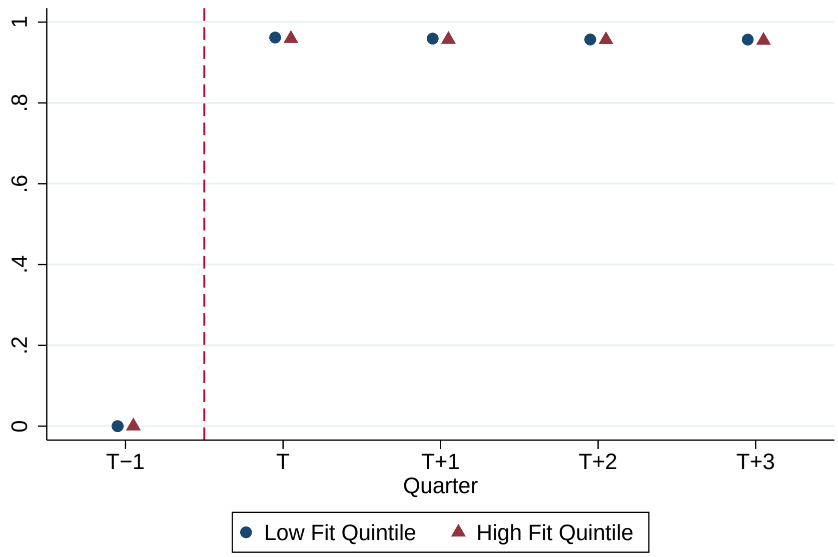



**(b)** By Fit of Default Plan



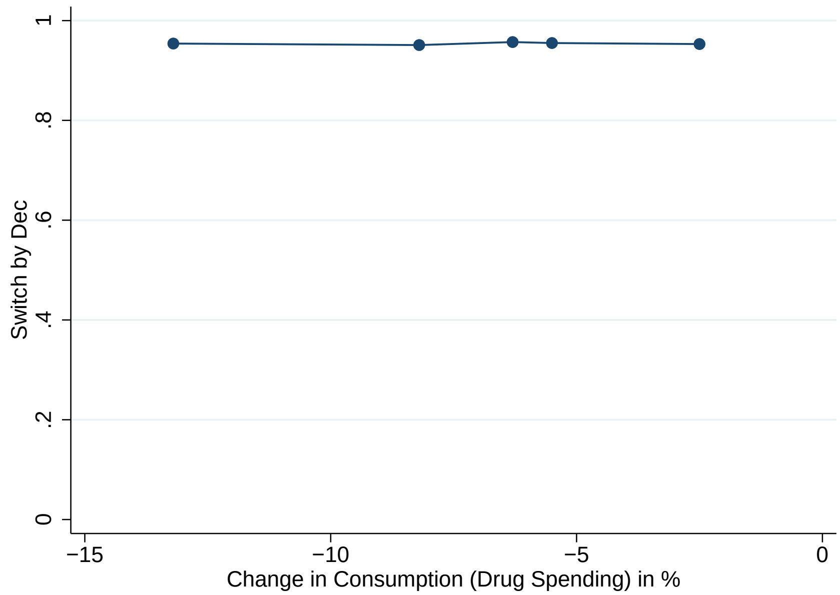

*Notes*: This figure plots event-study regression estimates of the effect of facing a default of reassignment to a randomly-chosen plan on quarterly drug spending outcomes, relative to facing a default of remaining in the incumbent plan. We use the 'RD Analysis Sample' described in Table 1 and estimate a stacked difference-in-differences design as described in Section 4. $T$ represents the first quarter following potential reassignment through the default mechanism. The outcome variable is quarterly logged normalized allowed prescription drug spending. We normalize the prices for all drugs by the average price for their NDC across all plans and years before computing spending. In the third column, we normalize prices by the average price of all drugs in the same therapeutic class across all plans and years. In Panel (a), we estimate a single effect for all beneficiaries. For Panel (b), we allow for separate effects based on whether beneficiaries who faced a default of reassignment would have been assigned to a plan in the bottom quintile of plans they could have been assigned to, by the 'fit' of that plan's formulary for their own drug consumption. Red triangles represent treated beneficiaries assigned to a bottom-quintile plan, whereas blue triangles represent all other treated beneficiaries. Lines represent 95% confidence intervals.

**Appendix Figure A13:** Event Study Estimates of the Effect of Change in Default from Remaining in Incumbent Plan to Auto-Assignment to Randomly-Chosen Default Plan on Plan Switching Propensity, by Fit of New Default Plan




*Notes*: This figure plots event-study regression estimates of the effect of facing a default of reassignment to a randomly-chosen plan on plan switching, relative to facing a default of remaining in the incumbent plan. We use the 'RD Analysis Sample' described in Table 1, and the model estimated is described in Section 6. The outcome is an indicator for whether the beneficiary was enrolled in a different plan at the end of the quarter than they were enrolled in during December of the prior year. We allow for separate effects based on whether beneficiaries who faced a default of reassignment would have been assigned to a plan in the bottom quintile of plans they could have been assigned to, by the fit of that plans formulary for their own drug consumption. Red triangles represent treated beneficiaries assigned to a bottom-quintile plan, whereas blue triangles represent all other treated beneficiaries. Lines represent 95% confidence intervals.

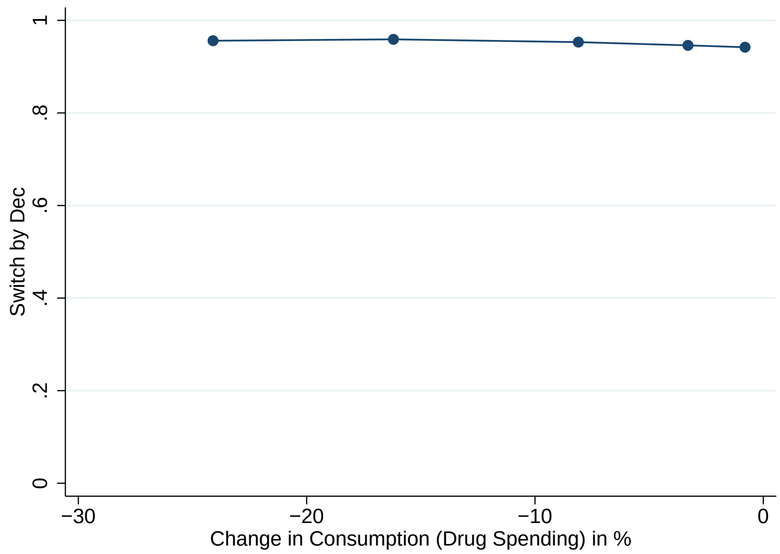
**Appendix Figure A14:** Comparison of Default Effects on Drug Consumption Against Default Effects on Plan Switching Propensity



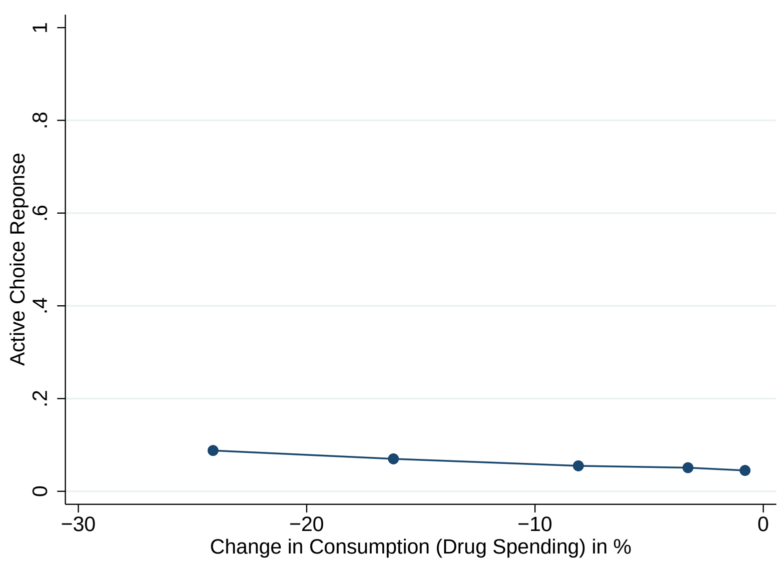

*Notes*: This figure plots the relationship between the effect of default plan assignment on drug spending and the effect of default plan assignment on propensity to switch plans between December of the prior year and December of the current year. To construct this figure, we estimate two difference-in-differences regressions of log drug spending and plan switching propensity on whether the beneficiary faced a default of auto-assignment to a randomly-chosen plan, interacted with the 'fit' of that plan's formulary for the beneficiary's drug consumption, binned into quintiles. Each dot represents a pair of quintile-specific spending (measured in percent changed) and plan switching effects. The regressions underlying this figure are described in Sections 4 and 6, and the specific estimates are given in Appendix Table A5.

**Appendix Figure A15:** Rational Inattention, Top Half in Spread of Fit

**(a)** Plan Switch



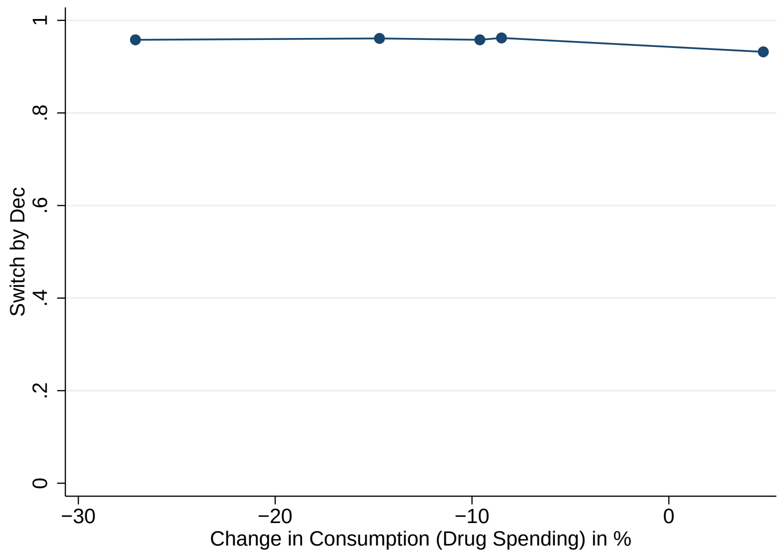

**(b)** Active Choice



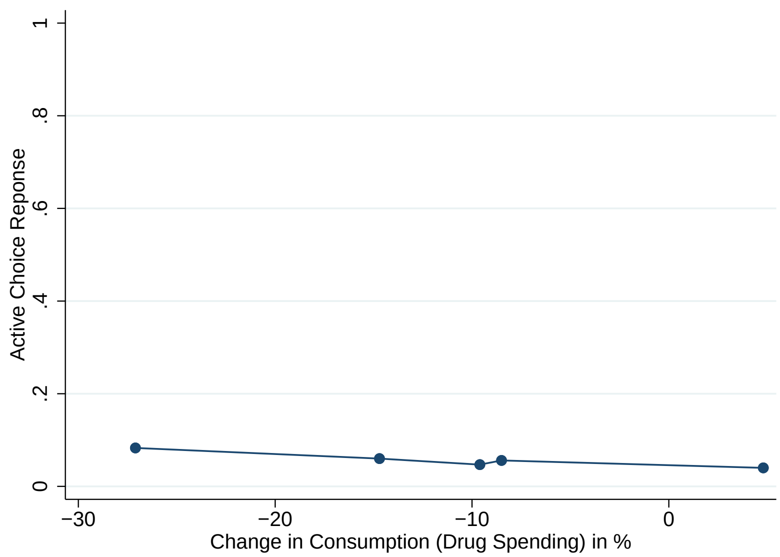

*Notes*: This figure replicates Figure A14 and 7, restricting to beneficiaries for whom the gap between the fit of their best plan and their worst plan is above the median gap in the sample. The top panel plots the propensity of beneficiaries to switch from the plan they were enrolled in during December of year $t-1$, as a function of the potential consumption loss they face by remaining in their default plan assignment over year $t$, in terms of log drug spending. The bottom panel plots the propensity of beneficiaries to make an active choice in December of year $t$, as a function of the potential consumption loss they face by remaining in their default plan assignment. In both panels, estimates of consumption loss come from a regression of log drug spending on whether a beneficiary's plan loses benchmark status in year $t$, and includes beneficiary-experiment level fixed effects, Part D region-experiment year fixed effects, and experiment pre/post year indicators. Estimates of propensity to switch and propensity to make an active choice come from regressions also including whether a beneficiary's plan lost benchmark status in year $t$, as well as region-experiment year fixed effects. Each regression is run for five different treatment groups, based on quintile of assigned plan fit, while full control group in all regressions. Matching coefficients from each of these five regression specifications are then plotted in the figures.

**Appendix Figure A16:** Rational Inattention, Top Quarter in Spread of Fit

**(a)** Plan Switch




**(b)** Active Choice




*Notes*: This figure replicates Figure A14 and 7, restricting to beneficiaries for whom the gap between the fit of their best plan and their worst plan is above the 75th percentile gap in the sample. The top panel plots the propensity of beneficiaries to switch from the plan they were enrolled in during December of year $t-1$, as a function of the potential consumption loss they face by remaining in their default plan assignment over year $t$, in terms of log drug spending. The bottom panel plots the propensity of beneficiaries to make an active choice in December of year $t$, as a function of the potential consumption loss they face by remaining in their default plan assignment. In both panels, estimates of consumption loss come from a regression of log drug spending on whether a beneficiary's plan loses benchmark status in year $t$, and includes beneficiary-experiment level fixed effects, Part D region-experiment year fixed effects, and experiment pre/post year indicators. Estimates of propensity to switch and propensity to make an active choice come from regressions also including whether a beneficiary's plan lost benchmark status in year $t$, as well as region-experiment year fixed effects. Each regression is run for five different treatment groups, based on quintile of assigned plan fit, while full control group in all regressions. Matching coefficients from each of these five regression specifications are then plotted in the figures.

**Appendix Figure A17:** Letter Sent to Beneficiaries Whose Plan Lost Benchmark Status

DEPARTMENT OF HEALTH & HUMAN SERVICES

Centers for Medicare & Medicaid Services

7500 Security Boulevard
Baltimore, MD 21244-1850

<BENEFICIARY FULL NAME>
<ADDRESS>
<CITY STATE ZIP>

<file creation date>

## Medicare is moving you to a new Part D drug plan for 2022

You're getting this notice because your premium costs in <Old Organization Marketing Name>'s <Old Plan Name> (<old contract>/<old PBP>) will go up starting January 1, 2022. Medicare is moving you to a new Medicare Part D drug plan to make sure you pay $0 for your monthly premium.

**Medicare will enroll you in <New Organization's Name>'s <New Name of Plan> (<new contract>/<new PBP>) starting January 1, 2022.** You'll pay:

- $0 of the monthly drug plan premium
- <$0 or up to $99> of the yearly drug plan deductible
- <insert LIS copayment amounts > for each prescription covered by the plan

## Do you want this new Medicare Part D drug plan?

▪ **NO, I don't want this new Medicare Part D drug plan.**

**You can stay in your current plan, but you must call your plan right away to let them know**. To stay in <Old Organization Marketing Name>'s <Old Plan Name >, call them at <Old Plan Phone Number> and tell them you want to stay a member. You'll pay <cost> each month for your premium in 2022.

Or, you can join a different Medicare Part D drug plan. To switch to a different Medicare drug plan, see the list of plans included with this notice. You can join any plan in this list and pay $0 premium and <insert LIS copayment amounts> for each prescription.

▪ **YES, I want to be in <New Organization's Name>'s <New Name of Plan> and pay $0 premium in 2022.**

**You don't have to do anything to stay in this new Medicare drug plan.** Visit <Plan Website> or call <New Plan Name> at <New Plan Phone> for more information about this plan. In December, Medicare will send you another blue letter letting you know which of the drugs you take will be covered in this plan.

This plan serves <States>. If this isn't where you live, call <New Plan Name> to make sure it serves where you live now. If it doesn't, call 1-800-MEDICARE (1-800-633-4227) to choose and join a plan that serves the state where you live. TTY users can call 1-877-486-2048.

## Get help & more information

For help understanding this notice, call your State Health Insurance Assistance Program at <SHIP Phone Number> for free, personalized health insurance counseling. Or, call 1-800-MEDICARE.

CMS
CENTERS FOR MEDICARE & MEDICAID SERVICES

CMS Product No. 11209 –
BLUE November 2021

*Notes*: This figure displays a template for the official letter sent by CMS to LIS beneficiaries who were previously auto-enrolled in a plan which lost benchmark status in 2022. We display only the first page here. The following page gives a list of potential plan options, with their contact information.

**Appendix Figure A18:** Letter Sent to Beneficiaries Whose Plan Exited

DEPARTMENT OF HEALTH & HUMAN SERVICES Centers for Medicare & Medicaid Services

7500 Security Boulevard
Baltimore, MD 21244-1850

<BENEFICIARY FULL NAME>
<ADDRESS>
<CITY STATE ZIP>

<file creation date>

**Medicare is moving you to a new Part D drug plan for 2022**

You're getting this notice because <Old Organization Marketing Name>'s <Old Plan Name> (<old contract>/<old PBP>) is leaving the Medicare Program on December 31, 2021. Medicare is moving you to a new Part D drug plan to make sure you have drug coverage in 2022.

**Medicare will enroll you in <New Organization's Name>'s <New Name of Plan> (<new contract>/<new PBP>).** Your coverage starts on January 1, 2022. With this new Medicare drug plan, you'll pay:

- <subsidy % or $0> of the monthly drug plan premium
- <$0 or up to $99> of the yearly drug plan deductible
- <insert LIS copayment amounts or % of the cost of each prescription> for each prescription covered by the plan filled at one of the plan's pharmacies

This plan serves <States>. If this isn't where you live, call <New Plan Name> to make sure it serves where you live now. If it doesn't, call 1-800-MEDICARE (1-800-633-4227) to choose and join a plan that serves the state where you live. TTY users can call 1-877-486-2048.

**Do you want to stay in this new plan for 2022?**

- **YES, I want to stay in <New Name of Plan>.**
  You don't need to do anything to stay in this Medicare Part D drug plan for 2022. You can visit <Plan Website> or call <New Plan Name> at <New Plan Phone> for more information about the plan. In December, Medicare will send you another blue letter letting you know which of the drugs you take will be covered in this plan.

- **NO, I want to switch to a different Part D drug plan for 2022.**
  To switch to a different Medicare drug plan, see the list of plans included with this notice. You can join any plan in this list and pay <subsidy % or $0> premium and <insert LIS copayment amounts > for each prescription. Your new coverage would start the next month. **You qualify for Extra Help, so you may have other chances to switch your Medicare Part D drug plan during the year**. A different Medicare drug plan may cover more of your drugs.

**Get help & more information**

For help understanding this notice, call your State Health Insurance Assistance Program at <SHIP Phone Number> for free, personalized health insurance counseling. Or, call 1-800-MEDICARE (1-800-633-4227) for help. TTY users can call 1-877-486-2048.

CMS Product No. 11208 -
BLUE November 2021

*Notes*: This figure displays a template for the official letter sent by CMS to LIS beneficiaries who were previously auto-enrolled in a plan which exited in 2022. We display only the first page here. The following page gives a list of potential plan options, with their contact information.

**Appendix Table A1:** Regression Discontinuity Balance Tests for Observable Beneficiary Characteristics

| | Age | % Female | Prior Drug Spending | Prior Non-Drug Med. Spending | Elixhauser Index | Incumbent Plan 'Fit' |
|---|---|---|---|---|---|---|
| Beneficiary Faced Plan | 0.021 | 0 | -23.484 | -23.102 | -0.001 | -0.015 |
| Reassignment Default | (0.034) | (0.002) | (19.283) | (85.557) | (0.013) | (0.001) |
| Average Value for Control Beneficiaries | 76.3 | 0.682 | 3,328 | 9,528 | 3.88 | 0.632 |
| Observations | | | 464,341 | | | |

*Notes*: This table reports regression discontinuity balance tests for observable beneficiary characteristics, illustrated by coefficients on a dummy variable for the beneficiary's year $t-1$ plan having a year $t$ bid that exceeds the year $t$ benchmark. For this, the RD analytic sample is employed, which is described in further detail in Table 1. RD regressions include Part D region-experiment year fixed effects. All regressions restrict to beneficiaries whose year $t-1$ plans bid within $6 of the year $t$ benchmark. Regression coefficients summarize the results in Figure 3.

**Appendix Table A2:** Difference-in-Difference Regression Estimates of the Effects of a Change in Default from Remaining in Incumbent Plan to Auto-assignment to Randomly-Selected Default Plan on Drug and Non-Drug Outcomes

| | Log # Fills | Log Days Supply | Log Non-Drug Spending |
|---|---|---|---|
| **Panel A** | | | |
| Faced Plan Reassignment Default | -0.005 | -0.015 | 0.006 |
| × Post | (0.002) | (0.003) | (0.006) |
| **Panel B** | | | |
| Faced Plan Reassignment Default | -0.002 | -0.007 | 0.010 |
| × Post | (0.002) | (0.004) | (0.007) |
| Faced Plan Reassignment Default | -0.011 | -0.030 | -0.017 |
| × Assigned Plan in Worst Quintile by Fit | (0.003) | (0.006) | (0.010) |
| × Post | | | |
| Average Value for Control Beneficiaries in Prior Year (levels) | 53.3 | 1649 | $9535 |
| Observations | | 5,574,684 | |

*Notes*: This table reports difference-in-difference regression estimates of the change in a beneficiary's default from remaining in their incumbent plan to auto-assignment to a randomly-selected default plan on quarterly drug utilization and non-drug spend outcomes. The treatment group consists of beneficiaries whose year $t-1$ plans bid above the year $t$ benchmark, and the control group consists of beneficiaries whose year $t-1$ plans bid below the year $t$ benchmark. For this, the RD analytic sample is employed, which is described in further detail elsewhere. For both groups, we restrict to beneficiaries whose year $t-1$ plans bid within $6 of the year $t$ benchmark. Regressions include beneficiary-experiment level fixed effects, Part D region-experiment year fixed effects, and experiment-pre/post year indicators. In Panel A, we estimate the overall effect of the change in default. In Panel B, we estimate heterogeneous effects by the fit of the plan (randomly) selected to be the beneficiary's year $t$ default by interacting an indicator for being assigned a default plan in the bottom quintile of fit among the benchmark plans in the beneficiary's choice set with dummies for "Post" and $1(Bid_t > Benchmark_t)$. As such, the coefficients in the first row of Panel B can be interpreted as the effects of the change in default for beneficiaries whose randomly-selected default plan is in the top four quintiles of fit, and the sum of the coefficients in the first and second rows of Panel B can be interpreted as the effects of the change in default for beneficiaries whose randomly-selected default plan is in the bottom quintile of fit. In column (1), the outcome is number of prescription fills, while the outcome in column (2) is total days supply associated with those fills. In column (3) meanwhile, the outcome is non-drug medical spending, defined as spending under Medicare Parts A and B.

**Appendix Table A3:** Event Study — Effect of Change in Default from Remaining in Incumbent Plan to Auto- assignment to Randomly-Chosen Default Plan

| | Switch from $t$ - 1 Plan by December of $t$ | Active Choice by December of $t$ | Log (Drug Spending) | Log (Price-Normalized Drug Spending) |
|---|---|---|---|---|
| T - 8 | | | 0.013 | 0.021 |
| | | | (0.007) | (0.007) |
| T - 7 | | | 0.001 | 0.015 |
| | | | (0.007) | (0.007) |
| T - 6 | | | 0.009 | 0.022 |
| | | | (0.006) | (0.006) |
| T - 5 | | | 0.009 | 0.022 |
| | | | (0.006) | (0.006) |
| T - 4 | | | 0.011 | 0.014 |
| | | | (0.006) | (0.006) |
| T - 3 | | | -0.003 | -0.005 |
| | | | (0.005) | (0.005) |
| T - 2 | | | 0.004 | -0.001 |
| | | | (0.005) | (0.005) |
| T | 0.960 | 0.046 | -0.041 | -0.037 |
| | (0.001) | (0.001) | (0.005) | (0.005) |
| T + 1 | 0.957 | 0.049 | -0.077 | -0.063 |
| | (0.001) | (0.001) | (0.006) | (0.005) |
| T + 2 | 0.956 | 0.050 | -0.075 | -0.054 |
| | (0.001) | (0.001) | (0.006) | (0.006) |
| T + 3 | 0.955 | 0.050 | -0.050 | -0.038 |
| | (0.001) | (0.001) | (0.006) | (0.006) |
| Mean of Dep Var | 0.170 | 0.016 | 5.390 | 5.451 |
| Observations | 2,307,468 | 2,307,441 | 5,574,672 | 5,574,672 |

*Notes*: Regression results underlying Figure 5

**Appendix Table A4:** Event Study — Effect of Change in Default from Remaining in Incumbent Plan to Auto- assignment to Randomly-Chosen Default Plan

| | Switch from $t$ - 1 Plan by December of $t$ | Active Choice by December of $t$ | Log (Drug Spending) | Log (Price-Normalized Drug Spending) |
|---|---|---|---|---|
| T - 8 (low fit) | | | 0.018 | 0.026 |
| | | | (0.011) | (0.011) |
| T - 8 (high fit) | | | 0.011 | 0.020 |
| | | | (0.007) | (0.007) |
| T - 7 (low fit) | | | -0.003 | 0.011 |
| | | | (0.010) | (0.010) |
| T - 7 (high fit) | | | 0.002 | 0.017 |
| | | | (0.007) | (0.007) |
| T - 6 (low fit) | | | 0.006 | 0.019 |
| | | | (0.010) | (0.010) |
| T - 6 (high fit) | | | 0.009 | 0.023 |
| | | | (0.007) | (0.007) |
| T - 5 (low fit) | | | 0.011 | 0.022 |
| | | | (0.009) | (0.009) |
| T - 5 (high fit) | | | 0.009 | 0.022 |
| | | | (0.006) | (0.006) |
| T - 4 (low fit) | | | 0.015 | 0.017 |
| | | | (0.009) | (0.009) |
| T - 4 (high fit) | | | 0.009 | 0.013 |
| | | | (0.006) | (0.006) |
| T - 3 (low fit) | | | -0.002 | -0.004 |
| | | | (0.008) | (0.008) |
| T - 3 (high fit) | | | -0.004 | -0.005 |
| | | | (0.006) | (0.006) |
| T - 2 (low fit) | | | 0.012 | 0.007 |
| | | | (0.008) | (0.008) |
| T - 2 (high fit) | | | 0.001 | -0.003 |
| | | | (0.005) | (0.005) |
| T (low fit) | 0.962 | 0.055 | -0.071 | -0.067 |
| | (0.001) | (0.002) | (0.008) | (0.008) |
| T (high fit) | 0.959 | 0.043 | -0.031 | -0.027 |
| | (0.001) | (0.001) | (0.006) | (0.006) |
| T + 1 (low fit) | 0.959 | 0.059 | -0.159 | -0.144 |
| | (0.001) | (0.002) | (0.009) | (0.009) |
| T + 1 (high fit) | 0.957 | 0.046 | -0.050 | -0.036 |
| | (0.001) | (0.001) | (0.006) | (0.006) |
| T + 2 (low fit) | 0.957 | 0.061 | -0.142 | -0.118 |
| | (0.001) | (0.002) | (0.009) | (0.009) |
| T + 2 (high fit) | 0.956 | 0.047 | -0.053 | -0.033 |
| | (0.001) | (0.001) | (0.006) | (0.006) |
| T + 3 (low fit) | 0.957 | 0.062 | -0.109 | -0.100 |
| | (0.001) | (0.002) | (0.009) | (0.009) |
| T + 3 (high fit) | 0.955 | 0.047 | -0.030 | -0.017 |
| | (0.001) | (0.001) | (0.006) | (0.006) |
| Mean of Dep Var | 0.170 | 0.016 | 5.390 | 5.451 |
| Observations | 2,307,468 | 2,307,441 | 5,574,672 | 5,574,672 |

*Notes*: Regression results underlying Figures 5b, A12b, A14, 6.

**Appendix Table A5:** Comparison of the Reduction in Consumption Due to the Change in Default Versus Effects of the Change in Default in Switching and Active Choice

| | Fit Quintile | | | | | Difference Between 1 & 5 | |
|---|---|---|---|---|---|---|---|
| | 1 | 2 | 3 | 4 | 5 | Effect | $ |
| **Primary Sample** | | | | | | | |
| Active Choice Response | 0.073 | 0.063 | 0.054 | 0.051 | 0.050 | 0.023 | |
| | (0.002) | (0.002) | (0.002) | (0.002) | (0.002) | | |
| Utilization Response | -0.132 | -0.082 | -0.063 | -0.055 | -0.025 | -0.107 | $396 |
| | (0.005) | (0.006) | (0.006) | (0.006) | (0.006) | | |
| **Top 50% Spread Sample** | | | | | | | |
| Active Choice Response | 0.088 | 0.070 | 0.055 | 0.051 | 0.045 | 0.043 | |
| | (0.003) | (0.003) | (0.003) | (0.002) | (0.003) | | |
| Utilization Response | -0.241 | -0.162 | -0.081 | -0.033 | -0.008 | -0.233 | $863 |
| | (0.009) | (0.010) | (0.010) | (0.010) | (0.011) | | |
| **Top 25% Spread Sample** | | | | | | | |
| Active Choice Response | 0.083 | 0.060 | 0.056 | 0.047 | 0.040 | 0.043 | |
| | (0.004) | (0.004) | (0.004) | (0.003) | (0.003) | | |
| Utilization Response | -0.271 | -0.147 | -0.085 | -0.096 | 0.048 | -0.271† | $1,004 |
| | (0.014) | (0.015) | (0.015) | (0.016) | (0.015) | | |
| **Top 10% Spread Sample** | | | | | | | |
| Active Choice Response | 0.076 | 0.053 | 0.043 | 0.044 | 0.034 | 0.042 | |
| | (0.006) | (0.006) | (0.005) | (0.006) | (0.004) | | |
| Utilization Response | -0.300 | -0.131 | -0.085 | -0.140 | 0.011 | -0.300† | $1,112 |
| | (0.023) | (0.028) | (0.026) | (0.028) | (0.026) | | |

*Notes*: This table reports results from an alternative specification to that of Equation 3 where we interact the indicator for facing default reassignment with an indicator for every fit quintile separately rather than comparing the lowest quintile (1) to the other four. Effects are measured relative to control beneficiaries whose default was to remain in their incumbent plan, and are reported in the first five columns. The sixth column displays the difference in treatment effects between the bottom (1st) and top (5th) quintiles; e.g., under our original sample, beneficiaries assigned to plans in the bottom quintile of fit are 2.3pp more likely to make an active choice than beneficiaries assigned to plans in the top quintile of fit. The seventh column converts utilization responses into equivalent dollar effects. Each panel reflects regression results in a different subsample; the top panel reflects our primary sample, while the others reflect subsamples limiting to beneficiaries with the most variation in fit across benchmark plans.
† In these specifications, we treat the effect of being assigned to a plan in the top quintile of fit as 0 rather than the estimated effect.

**Appendix Table A6:** Effects of Plan Assignment on Spending for Beneficiaries With Large Variance in Fit Across Plans, Price Normalization

| | All | Subsample (Fit Variance Quantile) | | |
|---|---|---|---|---|
| | | Top 50th | Top 25th | Top 10th |
| | **Outcome: Spending with Drug-Level Price Normalization** | | | |
| Faced Plan Reassignment Default | -0.030 | -0.054 | -0.046 | -0.026 |
| × Post | (0.004) | (0.010) | (0.015) | (0.025) |
| Faced Plan Reassignment Default | -0.082 | -0.196 | -0.218 | -0.216 |
| × Assigned Plan in Worst Quintile by Fit | (0.007) | (0.010) | (0.015) | (0.026) |
| × Post | | | | |
| | **Outcome: Spending with Class-Level Price Normalization** | | | |
| Faced Plan Reassignment Default | -0.012 | -0.049 | -0.042 | -0.014 |
| × Post | (0.004) | (0.009) | (0.014) | (0.024) |
| Faced Plan Reassignment Default | -0.036 | -0.111 | -0.110 | -0.085 |
| × Assigned Plan in Worst Quintile by Fit | (0.006) | (0.009) | (0.014) | (0.025) |
| × Post | | | | |

*Notes*: This table reports results from regressions in which we replicate the specifications from the second and third columns of Table 3, Panel B, restricted to subsamples. We define these subsamples based on the variance of 'fit' across benchmark plans in that beneficiary's region and year. The first column directly replicates the results from the prior tables. In the second through fourth columns, we restrict to the top 50%, 25%, and 10% of beneficiaries, ranked by this variance measure, respectively.

**Appendix Table A7:** Estimates of Effects of Prior Active Choice and Other Observed and Unobserved Individual Characteristics on Current Active Choice

| | Made Active Choice Following Plan Exit | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Actively Enrolled in Exiting Plan | 0.179 | 0.175 | 0.119 | 0.237 |
| | (0.005) | (0.005) | (0.006) | (0.011) |
| Years Enrolled in Exiting Plan | | | | 0.011 |
| | | | | (0.003) |
| Actively Enrolled $\times$ Years Enrolled | | | | $-0.038$ |
| | | | | (0.002) |
| Demographic and Health Controls | | X | X | X |
| Year Fixed Effects | | X | X | X |
| Exiting Plan Fixed Effects | | | X | X |
| $R^2$ | 0.058 | 0.092 | 0.181 | 0.190 |
| Share Active Choice | 0.128 | | | |
| Share Active Choice For Previously Non-Actively Enrolled | 0.077 | | | |
| Share Active Choice For Previously Actively Enroled | 0.256 | | | |
| Share Previously Actively Enrolled | 0.283 | | | |
| Number of Exiting Plans | 84 | | | |
| Observations | 32,852 | | | |

*Notes*: This table reports regression estimates of the effect of whether a beneficiary had enrolled in their incumbent plan by actively choosing it on their propensity to make an active choice of plan following their incumbent plan's exit from the market. In the first column we present the results from a regression with no additional controls. In column (2), we add in a battery of controls for demographics, including age, gender, and race, as well as controls for health status, i.e. the presence of comorbidities used in the Elixhauser Comorbidity Index. In column (3), we add fixed effects for the beneficiary's exiting plan. Lastly, in column (4), we interact the beneficiary's active choice indicator with a measure of the years enrolled in the exiting plan, i.e., the years since their last active choice. The average prior chooser had been enrolled for 2.9 years in their exiting plan, while the average prior non-chooser had been enrolled for 2.4.

# B Appendix: Measurement of Plan 'Fit'

In this appendix, we describe our measure of how well a plan's formulary "fits" the basket of drugs demanded by a beneficiary. For each beneficiary, we identify the basket of drugs taken by the beneficiary in year $t-1$. For new beneficiaries, $t-1$ is the year prior to the year the beneficiary entered Medicare, and the basket of drugs is constructed using data from the Medicaid programs in New York and Texas in the year prior to entering Medicare at age 65. For beneficiaries experiencing a change in their default from staying in their incumbent plan to auto-assignment to a randomly-selected default plan, $t-1$ is the year prior to the change in the default and the basket of drugs is constructed using data from that year (when beneficiaries were in their prior plan). The measure ranges from 0 (no drugs in the beneficiary's basket covered by the plan) to 1 (all drugs in the beneficiary's basket covered by the plan).

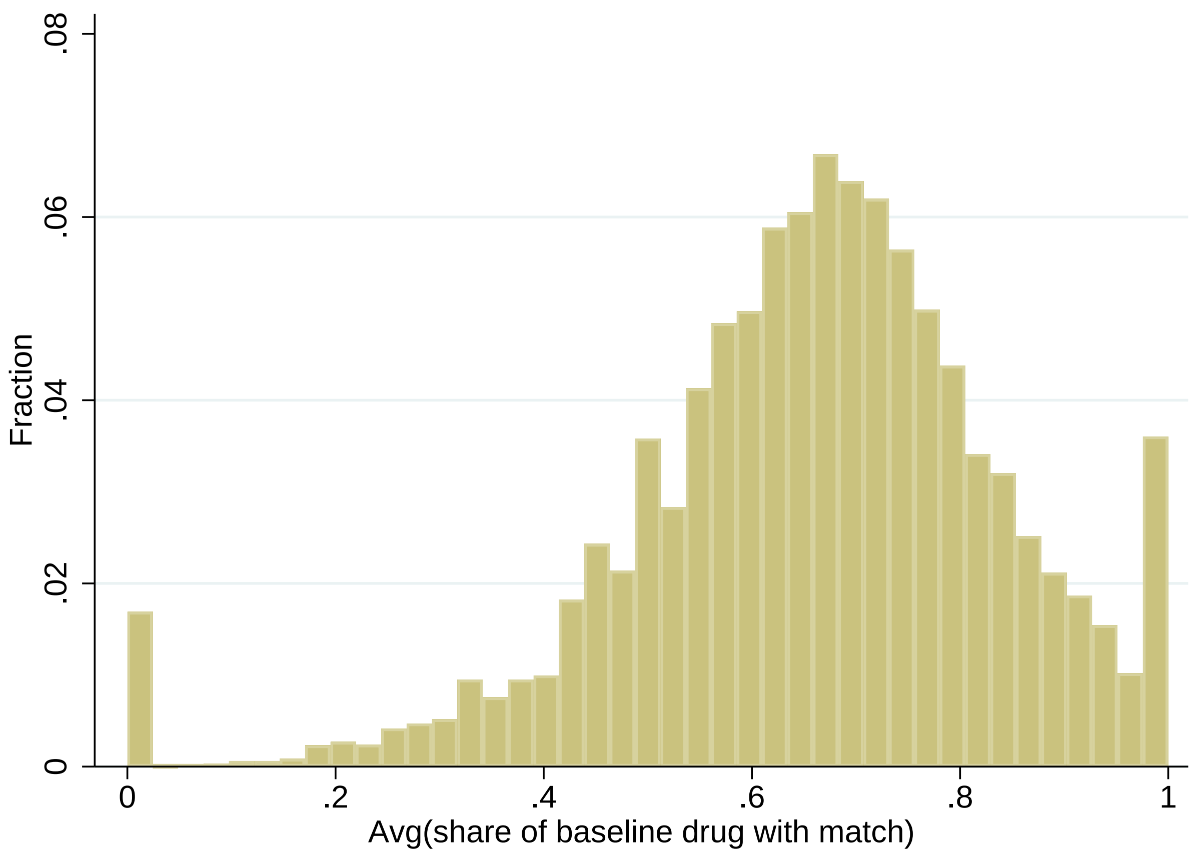
We plot the distribution of these beneficiary-plan fit measures for benchmark plans in Figure A19. Plan-beneficiary fit varies significantly for beneficiaries in the LIS program – while some plans cover 100% of a beneficiary's drugs, others cover none at all, with nearly full support across the distribution between 0 and 1. The average benchmark plan covers 60% of a beneficiary's prescriptions with a standard deviation of 23 percentage points.

Ultimately, we use the measure of plan-beneficiary fit to (1) measure the value of active choice and (2) assess the consequences of being assigned to a low-fitting plan versus a high-fitting plan. Both of these uses depend more on a beneficiary facing *differences* in fit across plans rather than lower or higher average fit across plans. Because of this, we present information on the *within-beneficiary* variation in fit across plans in the beneficiary's choice set. Figure A20 plots, for each beneficiary, the difference between the fit of the best fitting plan and the average fit across plans in the beneficiary's choice set.[33] This distribution is more compressed than the distribution of fit, owing to the correlation between fit values across plans for a given beneficiary. Despite this, we still see a great deal of heterogeneity, with a significant portion of beneficiaries facing relative coverage gaps of over 10 percentage points and some beneficiaries facing relative coverage gaps nearing half of their drugs.

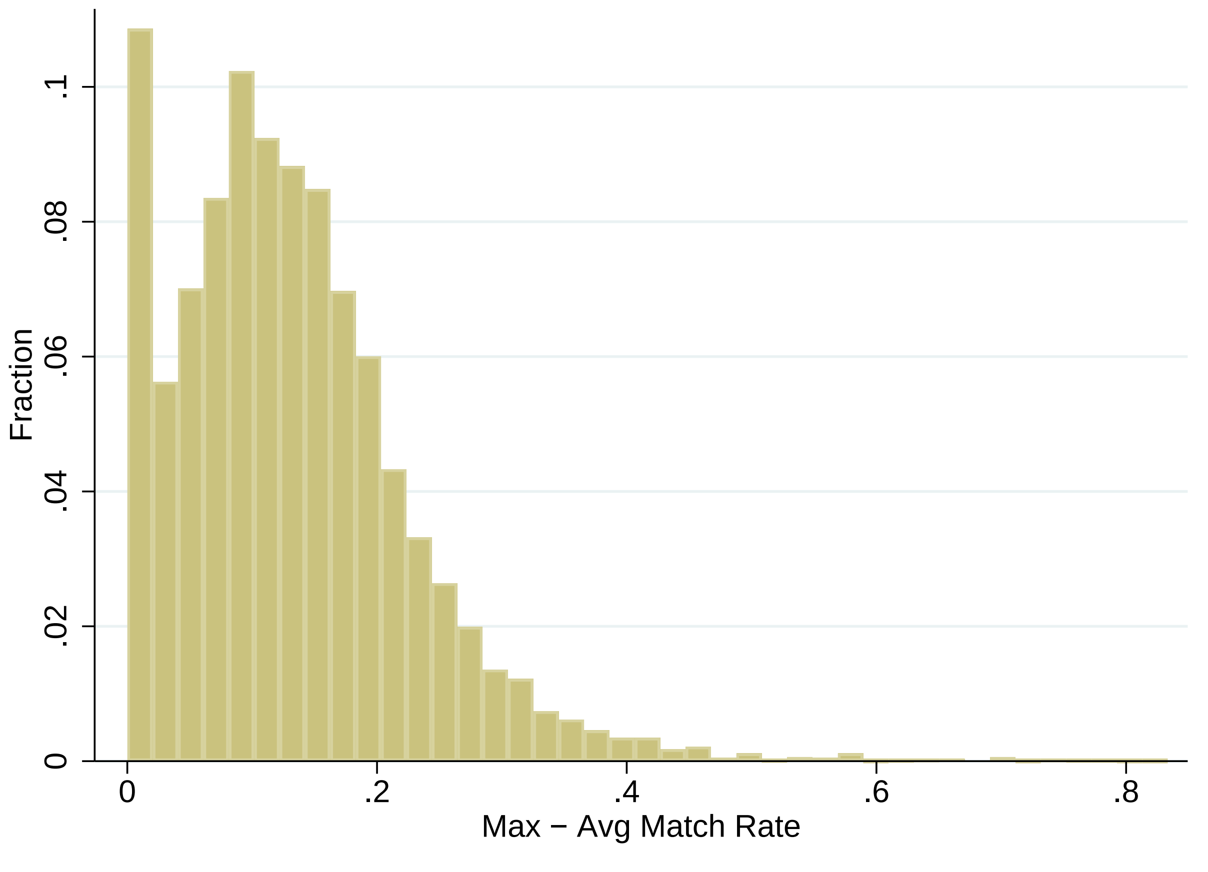
In Figure A21, we create a plot similar to Figure 1b, but using quartiles of the raw difference between the best-fitting plan a beneficiary has access to and the average, another representation of the "value of choice." In Section 6 we explain why using absolute differences may not be ideal. However, it produces results similar to that of Figure 1b.

Additionally, our measure of fit weights each drug in the beneficiary's basket as equivalent. In fact, some drugs may be of much higher value to the beneficiary than others. Naturally, we have no way of assessing beneficiary drug-specific value. As a proxy, we construct an alternative 'fit' measure that weights each drug by its average transacted price, so that our 'fit' measure is the share of *spending* covered by a plan, rather than the share of drugs. We replicate Figures A19, A20, A21, 1b, and 5b with this measure in, respectively, Appendix Figures A22 through A26.

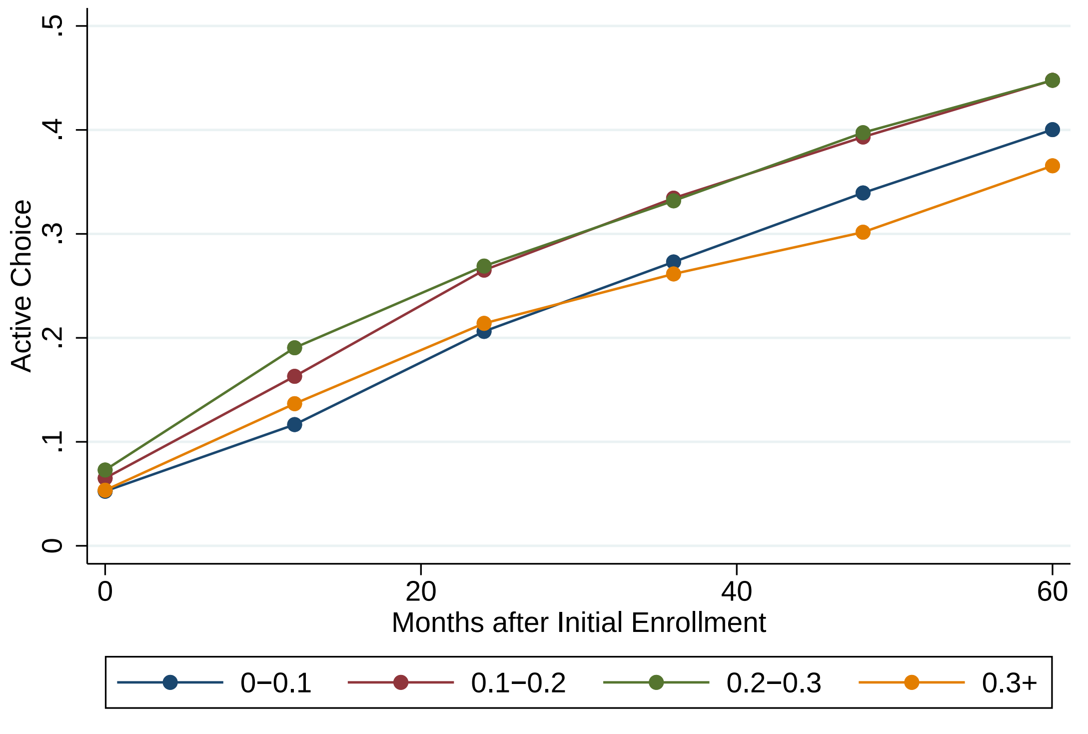
[33] One important note is that we take the maximum fit value from only the set of benchmark plans. Some beneficiaries might be able to improve their coverage by choosing a non-free insurance option. In practice, few LIS beneficiaries end up in such plans. Our approach is agnostic about the beneficiary's preferred trade-off between formulary coverage and premium payments and therefore serves as a lower bound on the true value of choice.



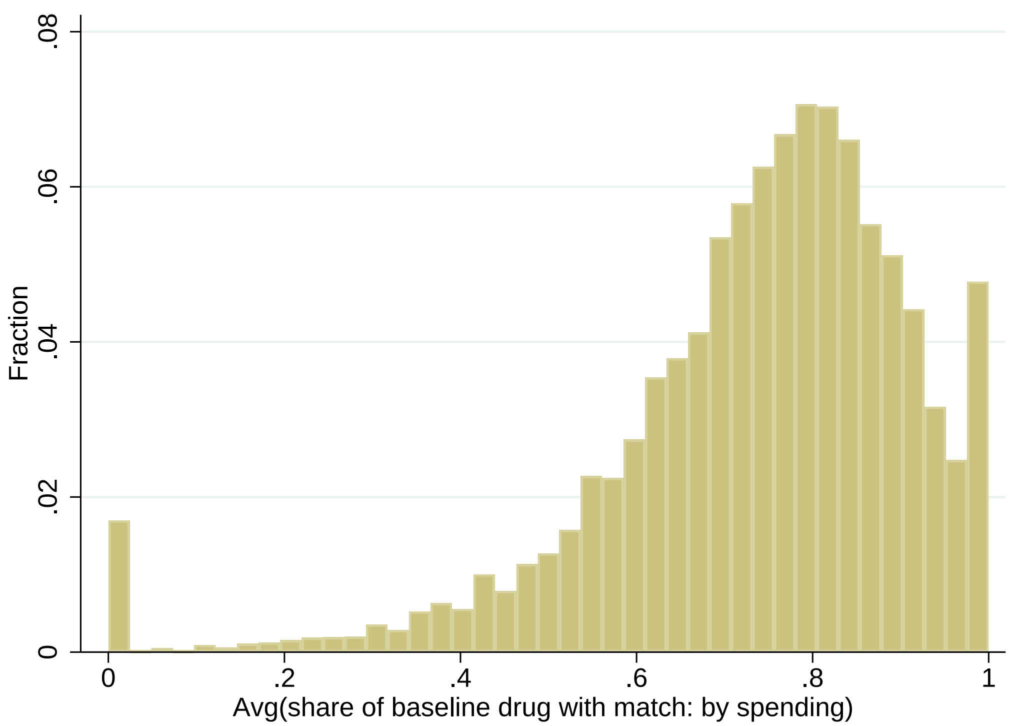

**Appendix Figure A19:** Distribution of beneficiary-plan pairwise fit measures for our Medicaid-linked sample of 65-year-olds, where fit is defined as the share of drugs taken at 64 covered by the plan. Fit is only computed for benchmark plans available when they turned 65.



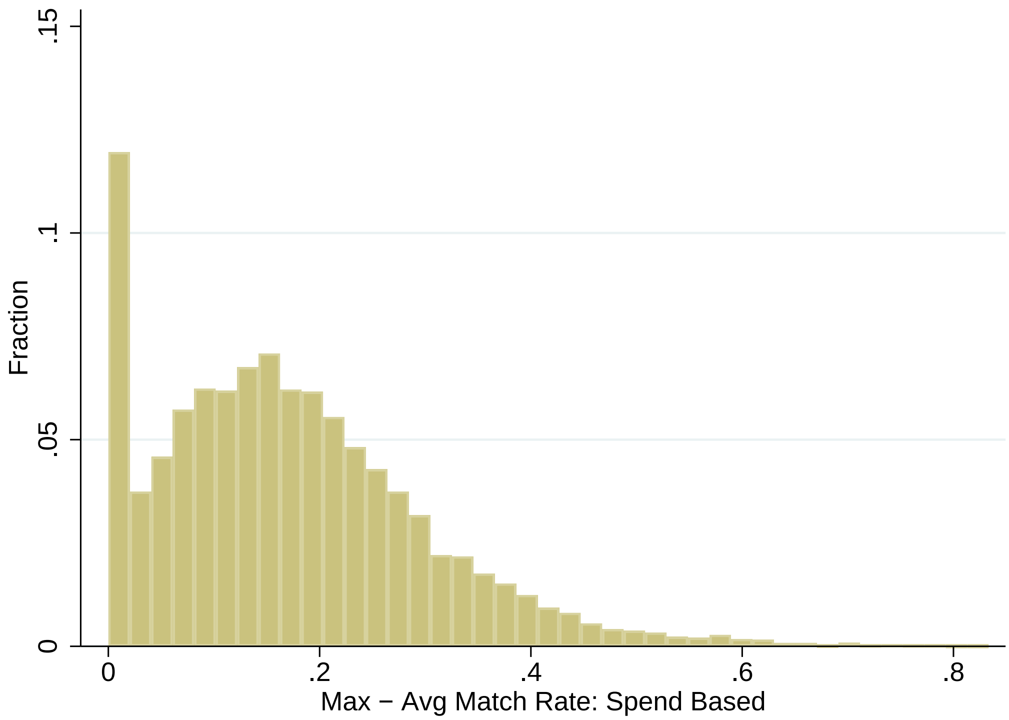

**Appendix Figure A20:** Distribution of beneficiary-specific measures of the difference between the fit of the best-fitting plan, and the average across available benchmark plans when they turned 65, for our Medicaid-linked sample of 65-year olds.



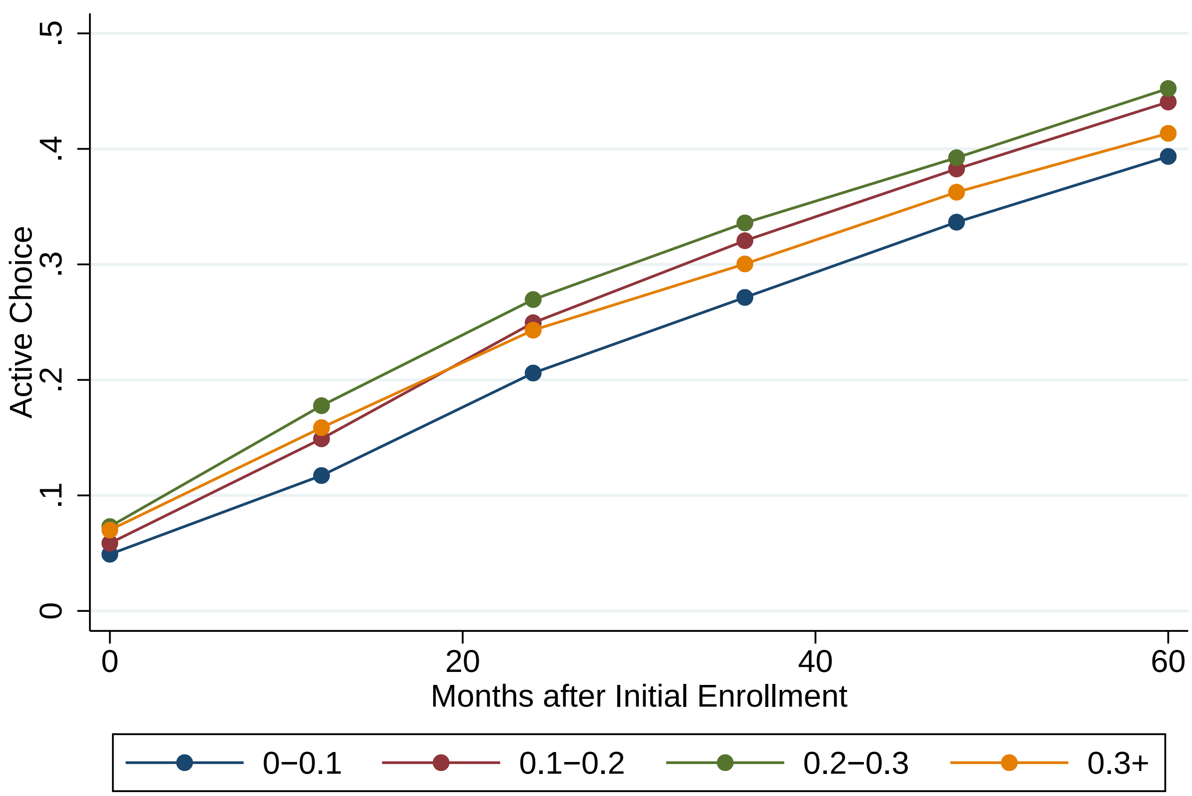

**Appendix Figure A21:** Cumulative active choice propensity for our Medicaid-linked sample by quartile of our 'value of choice' measure.



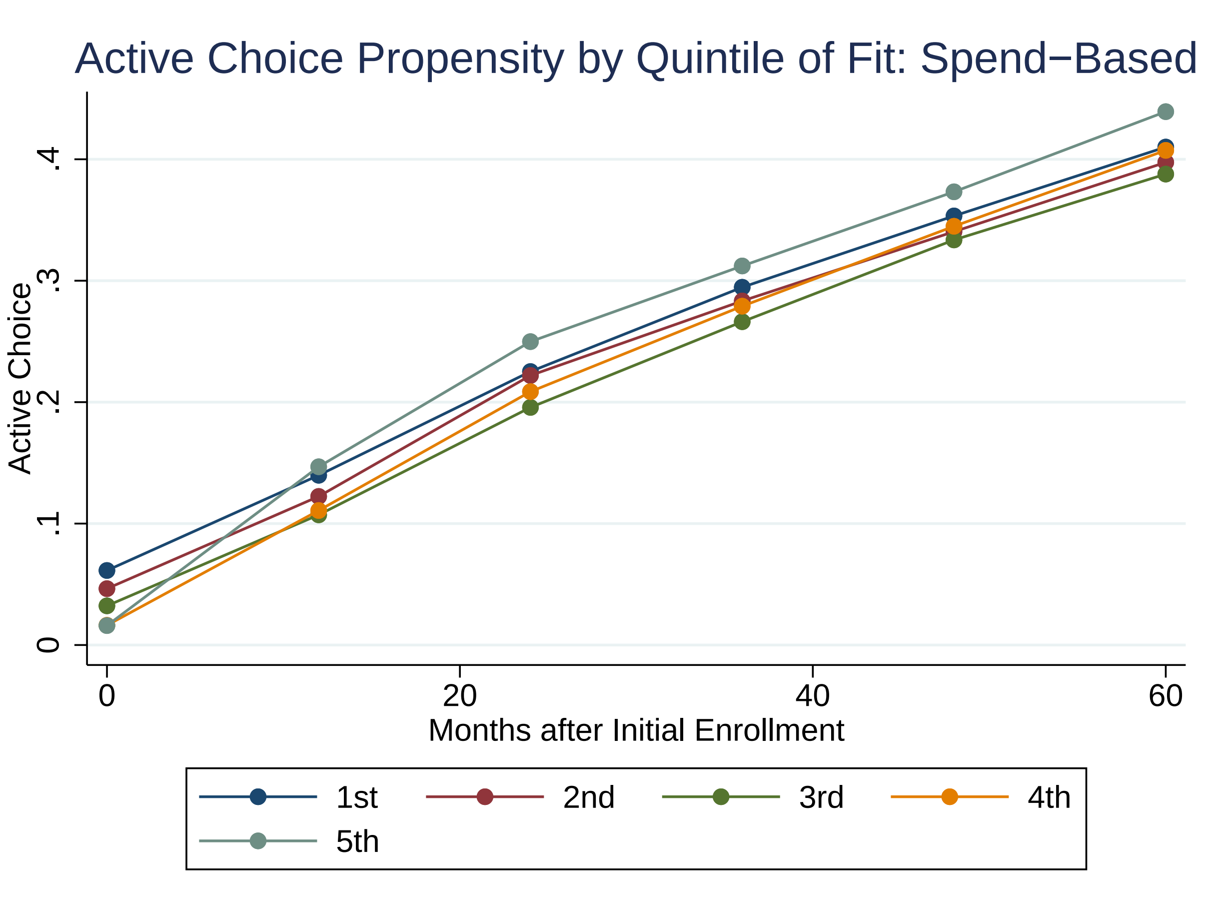

**Appendix Figure A22:** This figure replicates Figure A19 using our price-weighted measure of fit.



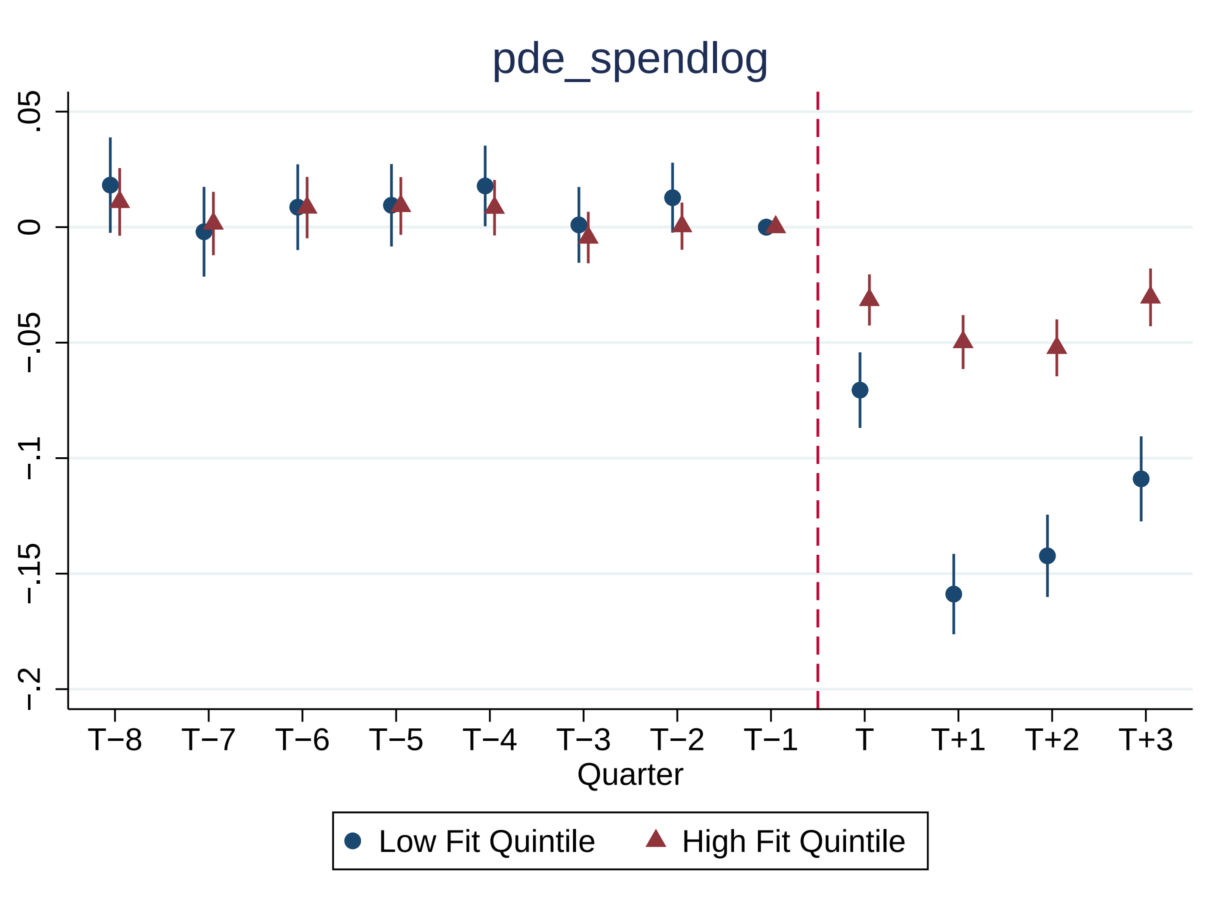

**Appendix Figure A23:** This figure replicates Figure A20 using our price-weighted measure of fit.




**Appendix Figure A24:** This figure replicates Figure A21 using our price-weighted measure of fit.




**Appendix Figure A25:** This figure replicates Figure 1b using our price-weighted measure of fit.




**Appendix Figure A26:** This figure replicates Figure 5b using our price-weighted measure of fit.

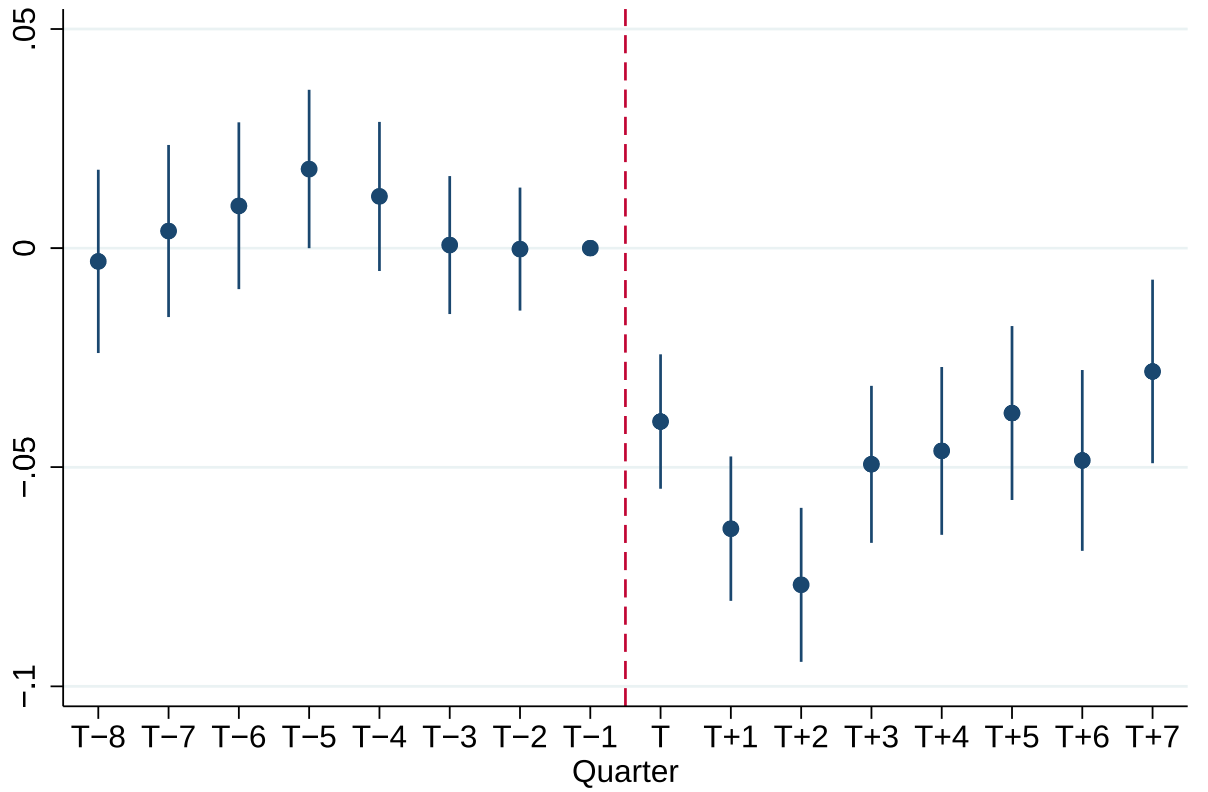
# C Appendix: Long-Run Effects

In Sections 4 and 6, we use a regression discontinuity design leveraging the fact that when a $t-1$ benchmark plan submits a year $t$ bid exceeding the year $t$ subsidy, the beneficiaries in that plan face an exogenous change in their default from remaining in their year $t$ plan to auto-assignment to a randomly-chosen default plan. We use that RD design to estimate the effects of the change in default on the probability of switching plans and the probability of making an active choice. For those analyses, we study switching and active choice within 12 months of the change in default. We limit to only 12 months because, following that year, many plans that were benchmark plans in year $t$ (and thus enroll both control beneficiaries and reassigned treated beneficiaries) may themselves lose benchmark status in year $t+1$. Because of this, interpreting treatment effects in future years is tricky, as we must disentangle the treatment effect of the benchmark loss in $t$ *and* the potential loss in $t+1$.

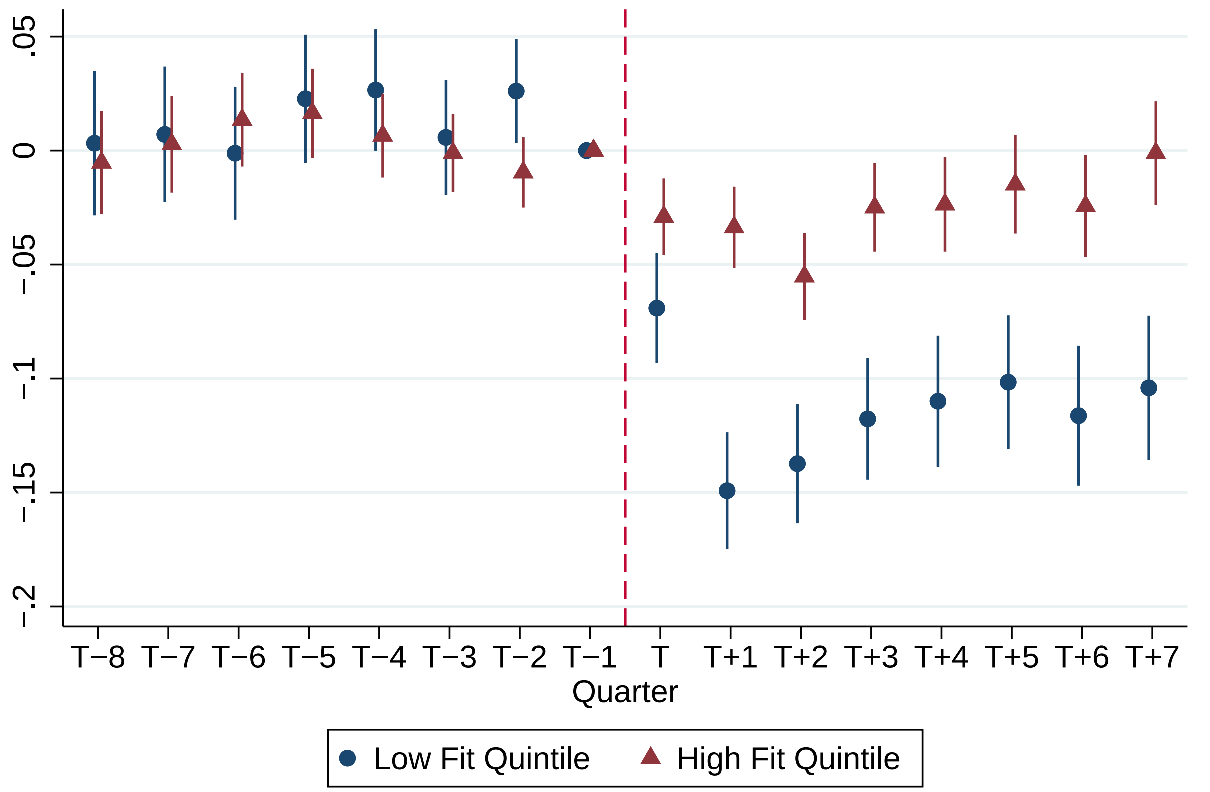
In this section, we study the effects of the change in default on switching and active choice over 24 months following the implementation of the new default. To do so, we restrict the set of default plans we allow beneficiaries to be assigned to in year $t$ to only those that do not lose benchmark status in $t+1$. Specifically, for beneficiaries in plans losing benchmark status from year $t-1$ to $t$, we restrict to those assigned to plans that retained benchmark status in $t+1$, We implement this restriction based on the beneficiary's assigned plan rather than actual enrolled plan, as the assigned plan was randomly selected. Likewise, for the control group of beneficiaries in plans not losing benchmark status from year $t-1$ to $t$, we restrict to those whose plan of enrollment as of December of year $t-1$ retains benchmark status in both $t$ and $t+1$. After these restrictions, we are left with 304,602 beneficiary-experiment records in our restricted sample.

We replicate our difference-in-differences analyses with this new sample. Figures A27 and A28 present event study estimates of the effects of the change in default on drug spending, with Figure A27 presenting overall effects and Figure A28 presenting effects stratified by plan fit. These results indicate that the spending reductions from the change in the default persist through the end of the second year after the new default was implemented. Further, they show that the difference in the consequences of being assigned to a high-fitting versus a low-fitting plan are also persistent, implying that the consumption losses are permanent and beneficiaries do not offset them by eventually taking up different drugs that do appear on the new plan's formulary or by switching to a plan with a smaller effect on drug spending.

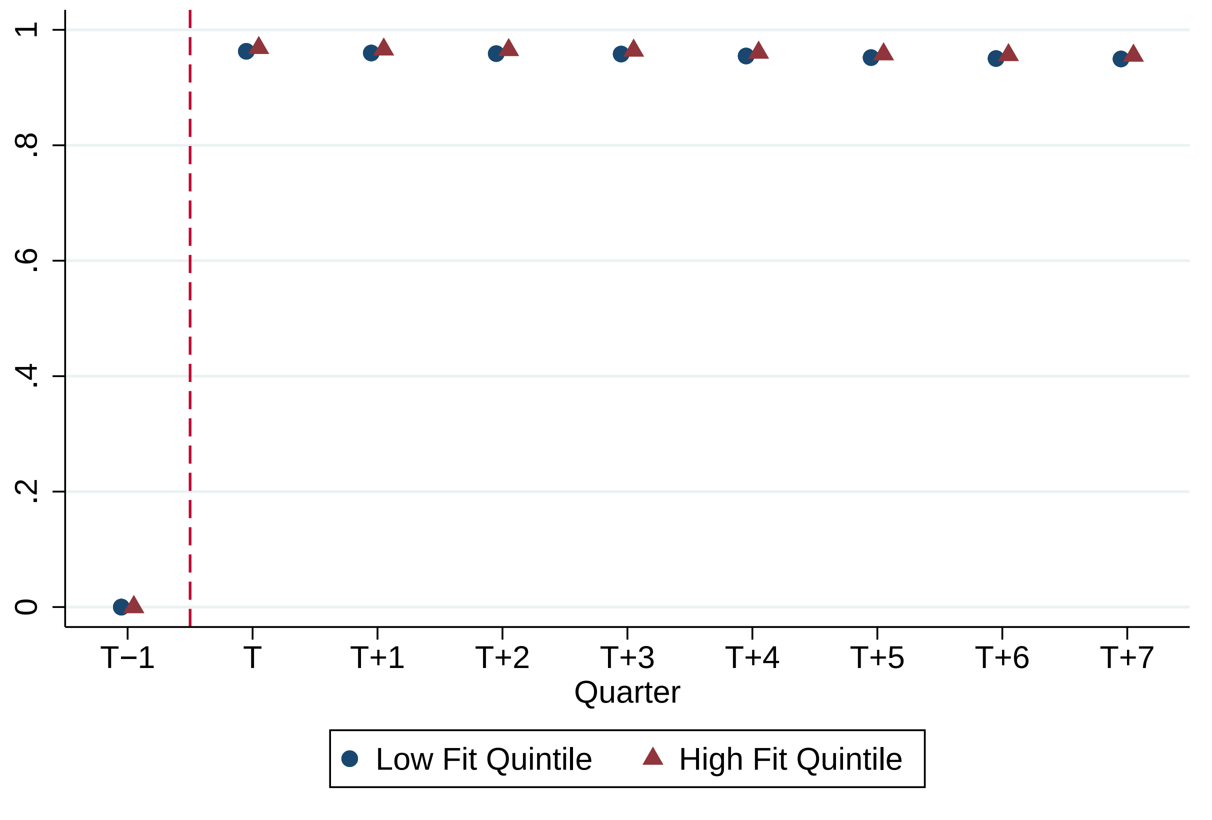
Figures A29 and A30 present event study estimates of the effects of the change in default on the probability of switching plans and the probability of making an active choice, respectively. Again, the effects are persistent. Beneficiaries who faced a change in default switched out of their incumbent plans at extremely high rates and did not switch back within 24 months of the implementation of the new default. They also were not induced by the change in default to make an active choice. The rate of active choice was extremely low after the change in default and did not increase over time. Further, there is effectively no difference in effects on switching or active choice for beneficiaries assigned to higher- versus lower-fitting plans. Again, these results imply that beneficiaries did not respond to the negative effects of the new default by switching plans or making active choices later. Instead, they just passively followed the new defaults and absorbed the persistent reductions in drug spending.



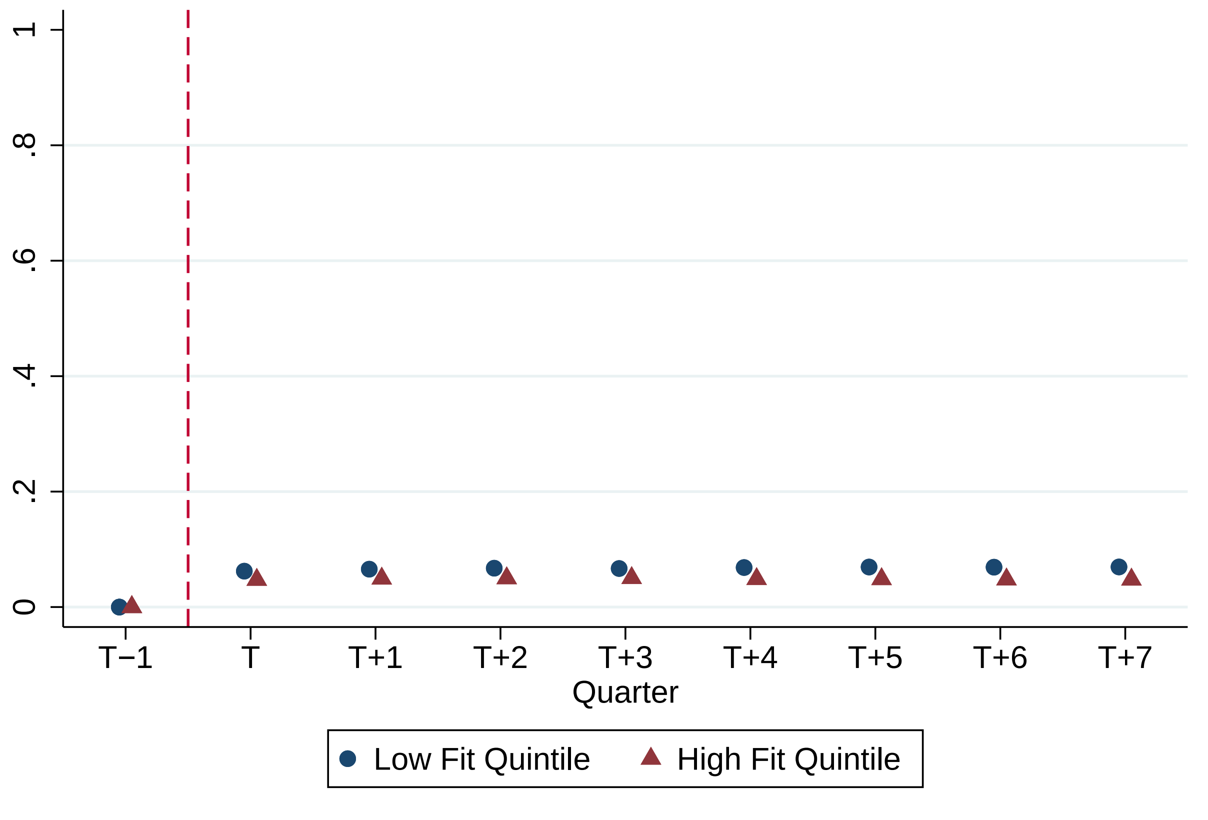

**Appendix Figure A27:** This figure plots event-study regression estimates of the effect of reassignment to a new plan on quarterly drug spending. This is a replication of Figure 5a with our two-year follow-up sample.




**Appendix Figure A28:** This figure plots event-study regression estimates of the effect of reassignment to a new plan on quarterly drug spending, for beneficiaries assigned to plans of differing 'fit.' This is a replication of Figure 5b with our two-year follow-up sample.




**Appendix Figure A29:** This figure plots the propensity of beneficiaries whose incumbent plan lost benchmark status to switch from the plan they were enrolled in during December of year $t-1$, stratified by the fit of the plan they were randomly assigned as a default for year $t$, relative to beneficiaries whose plan did not lose benchmark status. This is a replication of Figure A13 with our two-year follow-up sample.




**Appendix Figure A30:** This figure plots the propensity of beneficiaries whose incumbent plan lost benchmark status to make an active plan choice (not entering the default mechanism *or* switching plans after reassignment), stratified by the fit of the plan they were randomly assigned as a default for year $t$, relative to beneficiaries whose plan did not lose benchmark status. This is a replication of Figure 6 with our two-year follow-up sample.

# D Appendix: Further Model Results

In Section 5, we describe a general framework for thinking about default effects manifested through a model of inattentive choice. In this Appendix, we expand on some implications of that framework: How to think about different nested models, as well as welfare and policy implications.

## D.1 Example Parameterizations of $A(\cdot)$

First, we can build intuition by considering some common parameterizations of $A(\cdot)$, the latent attention function. We consider a set of parameterizations of the form $A(v^*, v^d, c; \theta) = \beta(v^* - v^d) - c + k$. Under such parameterizations, the agent trades off the benefit of making an active choice, $(v^* - v^d)$, against the cost of doing so, $c$, putting some relative weight $\beta$ on the benefits versus the costs. They also face random welfare-irrelevant (in the sense of Bernheim and Rangel (2009)) transitory shocks to attention, $k$, drawn from a distribution $F(\theta)$. We assume that $v$, $c$, and $k$ are denominated in the same units.

In Figure A31 we plot the probability of making an active choice (which we refer to as $a(\cdot)$), as a function of the value of the default plan option $v^d$. This can be thought of as the 'stakes' at play in making an active choice. When the default is better for the agent, they have less to lose by not making a choice. We consider three specific parameterizations that are exemplary of common models used in the literature and discuss each in turn. While we refer to these models as representing 'attention,' what we describe as 'paying attention' can also be thought of as consideration, or overcoming hassle costs, or any other costly decision effort.

**Rational Inattention.** First, in dark blue, we plot choice as a function of the value gap for a fully rational agent for whom $\beta = 1$, $k = 0$ always, and $c > 0$ is a constant. In such a model, the agent *never* makes an active choice if $v^* - v^d - c < 0$, and *always* does so if $v^* - v^d - c \geq 0$. Since the welfare gains to making an active choice are $v^* - v^d - c$, we describe this parameterization as representing a 'rationally attentive' chooser, who only makes an active choice when the expected benefit is greater than the cognitive cost. This is the model used by Handel (2013), and is the general workhorse model in the literature on active choice in health insurance and other markets for contracts. Models of rational search (Honka 2014) and rational inattention (Gabaix 2014, Matějka and McKay 2015, Brown and Jeon 2022) follow a similar active choice rule, in which agents rationally trade off costs and benefits, albeit replacing $v^*$ with the expected $v^j$ received as a result of searching or being attentive given the agent's prior information and $c$ with the expected total cognitive cost they will incur.

**Boundedly-Rational Inattention.** Second, in green, we plot the active choice probability as a function of the value gap for an agent for whom, again, $c$ is constant and $k = 0$, but now, $0 < \beta < 1$. The difference between this model and the rational inattention model is obvious: The curves have the same shape, but the new agent always makes an active choice if $\beta(v^* - v^d) - c \geq 0$, i.e. if $v^* - \frac{c}{\beta} \geq v^d$. This agent is 'boundedly-rationally attentive' in the sense that she understands the incentives facing her and responds correctly in a qualitative sense, but still sometimes makes mistakes in deciding when to pay attention and when not to. Here, the specific mistake is excessively weighting the decision cost over the expected benefit of active choice. This differential weighting causes agents to fail to make an active choice when $v^d \in \left(v^* - \frac{c}{\beta}, v^* - c\right)$, situations in which a rationally-attentive agent would make an active choice. This is a mistake because the agent would receive a positive net welfare improvement from making a choice in this range. A special case of this parameterization is the beta-delta model of present-bias most commonly used in the literature on default effects in retirement saving (Choi et al. 2003, Carroll et al. 2009, DellaVigna 2018), if we think of agents as discounting their *future* value of insurance coverage relative to their *current* cost of making an active choice. The fact that mistakes are made on an intermediate interval is consistent with the logic in these models that agents procrastinate when the stakes are sufficiently low, but do not do so when stakes are very high. This specific parameterization does not reflect the various ways in which agents can be boundedly-rationally inattentive, but common alternatives have similar features, typically involving the agent shading or being unaware of some part of the benefit of their choices (Ho et al. 2017, Heiss et al. 2021).

**Random/Exogenous Attention.** Finally, in brown, we plot the active choice probability for an agent for whom $c$ is once again a constant, but for whom $\beta = 0$ and $k \sim U[c - (1-\theta), c + \theta]$ for $\theta \in (0, 1)$. That is, $(100 \times \theta)\%$ of the time, the agent receives a transitory positive attention shock that is big enough to induce them to overcome their decision costs. For such agents, attention is entirely driven by transitory shocks, and not by any fundamentals in the value of their available options. We think of these types of agents as being irrationally or exogenously inattentive in the sense that their active decision-making is completely orthogonal to the benefit of making an active choice. The literature on inattention and salience in taxation (Chetty et al. 2009) has implicitly relied on models similar to this parameterization, in that they model agents as having some fixed probability of, e.g. ignoring sales taxes.[34]

The decision rules embedded in these models are simple, but they highlight the similarities and differences between these views of active choice. While there are important distinctions between rational and boundedly-rational models in terms of welfare considerations, Figure A31 shows that typical boundedly-rational models, in terms of unequal weights on decision criteria, still are very 'rational' in the sense that paying attention is still increasing in the benefit of doing so.

## D.2 Welfare

In Section 5.2, we discuss the idea that the attention elasticity governs the extent to which welfare losses from behavioral biases are bounded. In Figure A32 we illustrate this graphically, by plotting the relationship between value of the default, $v^d$, and expected welfare, under the welfare measure given in Section 5.2, for the three parameterizations we describe above. The dark blue line once again represents the rational inattention case, with $\beta = 1$ and $k = 0$. As shown in A31, this agent does not make an active choice when the potential welfare loss is less than $c$, $v^* - c < v^d$. In this range, the agent's expected welfare is equal to $v^d$. When the gap between $v^*$ and $v^d$ exceeds the cost of making an active choice $c$, however, this agent rationally makes an active choice and receives $v^* - c$ instead of $v^d$. Importantly, the expected welfare is

[34] As Morrison and Taubinsky (forthcoming) point out, one can think of the parameters estimated in this way as reduced-form representations of an underlying model that looks more like a rational inattention model.

equal to $v^* - c$ no matter how big the potential welfare loss gets because once the gap between $v^*$ and $v^d$ exceeds $c$ and triggers the agent to make an active choice, the value of the default no longer matters.

The green line represents the 'boundedly-rational' case, with $k = 0$ as before but now with $0 < \beta < 1$. As in Figure A31, this agent mimics the fully rational agent when the potential welfare loss is less than $c$, $v^* - c < v^d$. However, this agent continues to neglect to make an active choice beyond that range. Instead, this agent continues to be passive until $v^d = v^* - \frac{c}{\beta}$. This occurs because $0 < \beta < 1$ causes this agent to over-value the cost $c$ of making a choice today relative to the benefit of that choice tomorrow, $v^* - v^d$. This agent thus *mistakenly* fails to make an active choice when $v^d \in (v^* - \frac{c}{\beta}, v^* - c)$. Figure A32 illustrates why this is a mistake: The boundedly-rational agent experiences a greater welfare loss than the rational agent would have if faced with the same decision. When the $v^d$ exceeds $v^* - \frac{c}{\beta}$, however, this agent makes active choices and receives payoff $v^* - c$, as the rational agent does. This figure illustrates the idea highlighted in Carroll et al. (2009) — such an agent may be worse off with a relatively more generous default (such that $v^d \in (v^* - \frac{c}{\beta}, v^* - c)$), than with a harsher default (such that $v^d < v^* - \frac{c}{\beta}$).

The brown line represents the 'random attention' case, where $\beta = 1$ but each agent draws $k$ from a distribution $U[c - (1-\theta), c + \theta]$. Here each agent pays attention with probability $p = \theta$. Under this model, the expected welfare is just equal to $W = \theta(v^* - c) + (1-\theta)v^d$. No matter what the default is, a random $(100 \times \theta)\%$ of agents make active choices and receive according payoffs, with the residual passively accepting the default payoff.

Figure A32 illustrates that, under both the rational and boundedly-rational models, the welfare losses from agent misallocation have a strict upper bound, and therefore welfare has a strict lower bound even as the default gets worse. In the rational model, this loss is bounded by the cognitive cost $c$. This is a direct consequence of the 'rationality' at play: A rational agent will never allow themselves to suffer from a sufficiently poor state of the world that can be fixed with (costly) action. Models that allow for bounded rationality in the active choice decision relax these welfare bounds, though a bound still exists—in the present-bias model, the bound is instead $\frac{c}{\beta}$. Again, boundedly-rational agents will not allow themselves to suffer too much, although in this case they will downweight the consequences of poor plan fit relative to the costliness of reparative action. In contrast, the random attention model *does not have such a bound*. Agents' expected welfare is strictly monotonic in $v^d$. This arises from the fact that this agent does not respond to the state of the world at all and thus in many circumstances will simply take what comes. This important difference between the models of active choice will play a critical role in determining how optimal defaults differ across models.

## D.3 Optimal Default Policy

Given this discussion of welfare, we can discuss how to optimally assign $v^d$ to maximize expected welfare. We focus on a conflict highlighted in Carroll et al. (2009), Bernheim et al. (2015), and Goldin and Reck (2022). They point out that under various normative assumptions, optimal defaults can cut one of two ways. On the one hand, it may be optimal to design default policy to maximize $v^d$, to minimize opt-outs and 'nudge' agents into their best plans through smart defaults (Thaler and Sunstein 2003, Handel and Kolstad 2015a). On the other hand, it may be optimal to design default policy to *minimize* $v^d$, to *maximize* active choice, thus ensuring agents end up in their most-preferred plans.

We can set this question up akin to an optimal taxation problem by taking a first-order condition with respect to $v^d$:

$$\frac{\partial W}{\partial v^d} = \underbrace{1 - a(v^*, v^d, c)}_{\substack{\text{Benefit to}\\ \text{inframarginal}\\ \text{passive agents}}} + \underbrace{\frac{\partial a(v^*, v^d, c)}{\partial v^d}}_{\substack{\text{Marginal agents}\\ \text{induced to not make}\\ \text{an active choice}}} \underbrace{E[v^* - v^d - c | A(v^*, v^d, c) = 0]}_{\text{Value of choice for marginal agents}}$$

As we describe in Section 5.2, this highlights the trade-off of changes to $v^d$. Raising it, by attempting to better suit defaults to agents, is helpful for inframarginal passive agents, whose allocation improves. However, it may damage a set of marginal agents, for whom the default improvement induces them to stop paying attention and follow the default; if the default is worse than what they would have chosen otherwise, this reduces their welfare.

Figure A33 illustrates these effects of a marginal decrease of $\Delta v^d < c - \frac{c}{\beta}$ in the value of the default for the boundedly rational case. As in Figure A32, the value of the default *under the initial default* $v^d$ is on the x-axis and the expected welfare $W$ is on the y-axis. The green line represents the case of the initial default, and the magenta line represents the case of the new, lower-value default. It is straightforward to see that the new, lower-value default results in a welfare loss for the set of agents who fail to make an active choice under either the initial default or the new default, those for whom $v^* - v^d < \frac{c}{\beta} + \Delta v^d$. These agents follow the default in both cases, so the lower defaults obviously lowers their welfare, with this welfare loss illustrated by the area shaded in light red. At the same time, there is a welfare gain for agents for whom the lower default pushes their default value low enough to induce them to make an active choice. These are those for whom the value of the default was previously between $v^* - \frac{c}{\beta}$ and $v^* - \frac{c}{\beta} + \Delta v^d$. They were previously mistakenly making an active choice, since $v^* - c > v^d$, and

These agents have potential welfare loss in the range $\frac{c}{\beta} + \Delta v^d < v^* - v^d < \frac{c}{\beta}$. The welfare gain they experience due to the decrease in the value of the default is illustrated by the area shaded in light blue. When assessing whether a lower-value default harms or improves overall social welfare, the size of the light pink area must be compared to the size of the light blue area. Depending on the primitives, either a lower-value default or a higher-value default could be optimal.

The intuition for the result that a lower-value default could be socially beneficial is that if the policymaker is able to set a default with sufficiently low value, she can 'shock' all agents into making an active choice and receive $v^* - c$ in welfare. But if the policymaker does not go far enough, some inframarginal passive agents will suffer due to the decrease in the value of the default plan, in which they will ultimately enroll. In such a trade-off environment, the planner must generally decide between two extremes: A 'benevolent' default, which provides the maximum achievable default value, or a 'shocking' default, which provides $v^* - c$, although this latter piece assumes that there is a feasible default that is sufficiently punishing to induce universal active choice.[35]

[35]The planner's ability to set default values to be very high or very low is likely to be constrained. Information asymmetries (i.e., not knowing the preferences of agents) restrict a planner's ability to match agents to their optimal plans. Political constraints may limit the planner's ability to set harsh defaults–for example, taxing passive agents is unlikely to be an acceptable policy. The 'shocking' policy studied in Carroll et al. (2009) merely changed the *framing* of the consequences of a failure to make an active

Note, however, that the existence of this trade-off, specifically the existence of the negative (second) effect, depends critically on two conditions. First, it requires $E[v^* - v^d - c|A(v^*, v^d, c) = 0] > 0$, which is equivalent to the idea that, on the margin, agents who fail to make an active choice are experiencing a welfare loss. While this seems like a trivial point, we note that it is *not* true for rationally-inattentive agents. For an agent with the 'rational inattention' parameterization $A(v^*, v^d, c) = v^* - v^d - c$, note that

$$E[v^* - v^d - c|A(v^*, v^d, c) = 0] = E[v^* - v^d - c|v^* - v^d - c = 0] = 0$$

and therefore there is no trade-off in the optimal default policy and thus no benefit to a shocking default. More boundedly-rational models of decision costs do, however, induce 'mistakes at the margin.' For example, the 'mistake on the margin' in the present-biased parameterization above is equal to $E[v^* - v^d - c|\beta(v^* - v^d) - c = 0] = \frac{1-\beta}{\beta}c > 0$ for *any* value of $v^*, v^d$.

The existence of these mistakes at the margin is a necessary condition for the negative effect in Equation (2) to be non-zero, but it is not sufficient. The second necessary condition is that $\frac{\partial a(v^*,v^d,c)}{\partial v^d} < 0$, i.e. that increases in the value of the default option induce some agents to become passive. In other words, if there are no agents on the margin of making an active choice or not, it does not matter for policy whether the (non-existent) marginal agents are making mistakes. While it is likely (though not guaranteed) that there will be marginal agents when attention is rational, it need not be true more generally. When $\beta = 0$ in our parameterization above (e.g. in the 'random attention' case), $\frac{\partial a(v^*,v^d,c)}{\partial v^d} = 0$ *always* and thus there is no trade-off.

Generating optimal default policy with no trade-off is clear: When there is no harm to marginal agents, improved defaults benefit inframarginal passive agents at no cost to the marginals, and thus the optimal default should be to minimize opt-outs by maximizing $v^d$. On the other hand, when there is a trade-off, it may be optimal to ratchet down the value of the default.[36] Prior work, when evaluating this trade-off, has focused primarily on considering the extent to which agents are making a mistake (on the margin and otherwise) when they do not make an active choice. This is an inherently *normative* question. Bernheim et al. (2015) and Goldin and Reck (2022) show that different normative assumptions about whether choice passivity is a 'mistake' or a response to real decision costs can generate both of the two extreme optimal default policies we describe above. While having a specific model of behavior is important, our exercise in this section suggests that simply making an assumption about the extent of the mistake is sufficient for optimal policy even without a fully-specified model.

More importantly, this prior work has restricted itself to model domains in which $\frac{\partial a(v^*,v^d,c)}{\partial v^d} < 0$ by assumption.[37] If this is not true, and agents do not adjust their passivity (or make only very minor adjust-

choice rather than imposing any additional material consequences.

[36] Additional considerations include the extent to which the policymaker can achieve a $v^d$ close to $v^*$ by correctly picking a beneficiary's welfare-maximizing plan option; if they can get very close, a 'paternalistic' default may be optimal even in the presence of a trade-off, because at such high values of $v^d$, if $c > 0$, then $E[v^* - v^d - c|A(v^*, v^d, c) = 0] < 0$. When this is not true, and the policymaker would prefer to ratchet down the value of the default, extreme 'shocking' defaults can arise as an optimum because the share of inframarginal agents who are hurt by such a default decreases as the default becomes more harmful and agents become 'choosers,' and thus $\frac{\partial W}{\partial v^d}$ may *grow* in magnitude as $v^d$ falls.

[37] Goldin and Reck (2022) specifically employ a model akin to our 'rational inattention' model with $A(\cdot) = v^* - v^d - c$, but allow $c$ to have welfare-relevant and welfare-irrelevant components. The size of the welfare-irrelevant component of $c$ in their model is equivalent to the size of what we call the 'mistake on the margin.'

ments) in response to differences in the value of the default option, *then the question of whether or not their passivity is a 'mistake' is moot*. Our analysis in Section 6 evaluates whether this model domain is the correct one to theorize within.

## D.4 Heterogeneity in $c$

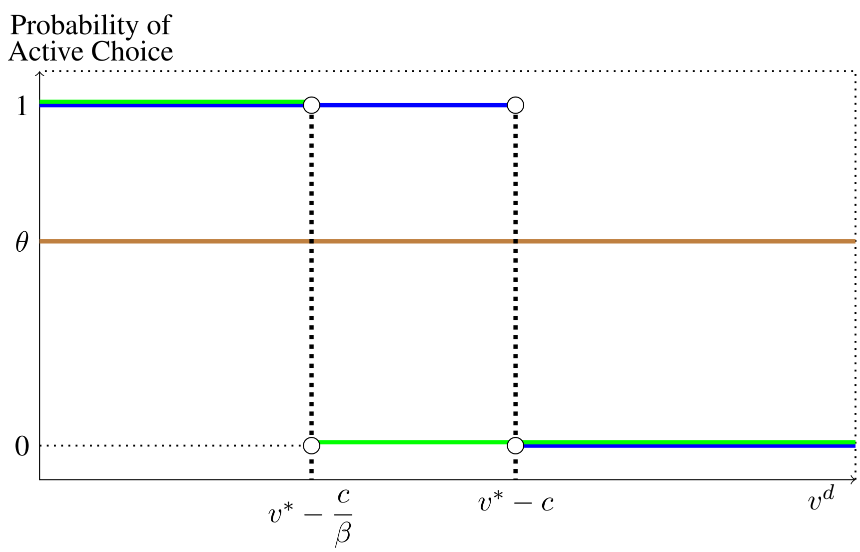
Figure A31 presents the relationship between active choice propensities and default value for a single representative agent with a fixed cognitive cost $c$. However, when we take this model to data, it need not be true that $c$ is constant across the population. Take, for example, the case where $c \sim U[\bar{c} - \epsilon, \bar{c} + \epsilon]$. In Figure A34 we consider how, if we hold the domain of analysis with respect to $v^d$ constant while widening the distribution of $c$, how the relationship between $v^d$ and the probability of active choice will appear. We can see in that figure that, for increasingly wider distributions of $c$, the observed relationship flattens out.

This is important as we attempt to empirically quantify the attention elasticity to $v^d$. If the distribution of $c$ is wide relative to observable variation in $v^d$, then we will estimate a flat relationship between the two even under rational inattention. This is an important motivator of our analysis in Section 4 quantifying the extent of effects of default changes.

Given that the distribution of $c$ is unknown, what can we conclude from observation about active choice responses to default stakes? Consider a setting where $v^*$ is constant and $c$ is randomly distributed with cumulative density function $F$, and assume the econometrician observes two distinct exogenously-assigned defaults, $v_0^d$ and $v_1^d$, with $v_0^d < v_1^d$. Moreover, assume $A(\cdot) = v^* - v^d - c$, i.e., the rational inattention case. For each default $D$, agents will make an active choice if $c < v^* - v_D^d$, and the share who do so will thus be given by $F(v^* - v_D^d)$. If the econometrician runs a regression of active choice status on $D$, the estimated coefficient will reflect the share of beneficiaries who are 'marginal' within that range. They can only be marginal if $c \in (v^* - v_1^d, v^* - v_0^d)$. If $c$ is greater, they will not make an active choice under either default assignment; if $c$ is less, they will *always* make an active choice. Therefore, the estimated coefficient $\Delta$ will reflect $F(v^* - v_0^d) - F(v^* - v_1^d)$, the density of agents with $c$ within the relevant range.

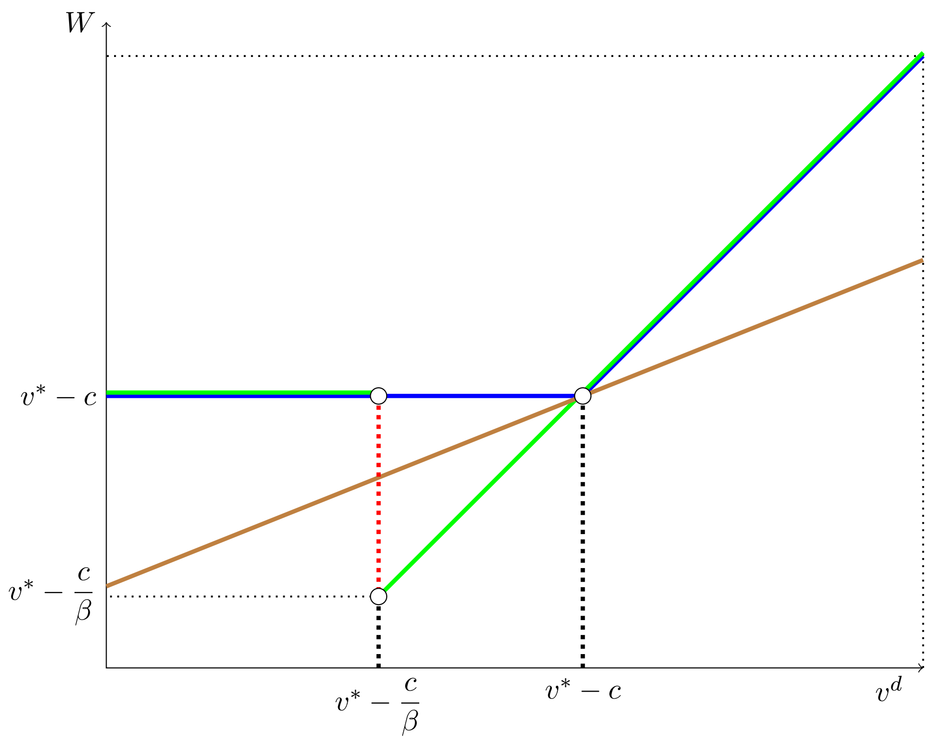
Therefore, for any estimate of $\Delta$, we can rule out any distribution of $c$ with density $\Delta$ in the range $(v^* - v_1^d, v^* - v_0^d)$. To the extent that observed default assignments have larger variation in value $v^d$, we can rule out density in a greater domain.

**Appendix Figure A31:** Theoretical Relationship Between Default Value and Active Choice



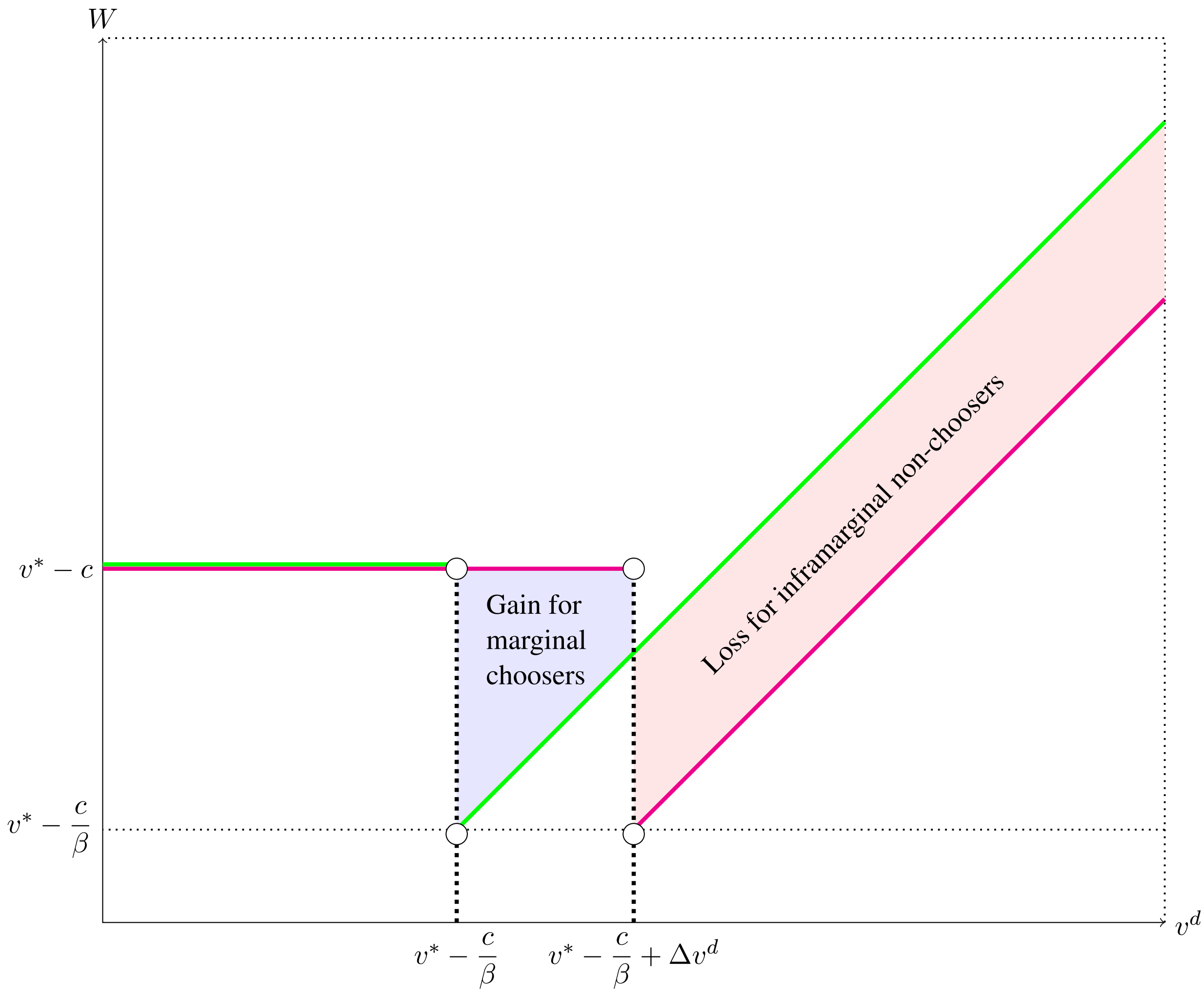

*Notes*: This figure plots the relationship between the value of the default $v^d$ and the ex ante probability of making an active choice under various models of active decision-making. The blue line represents the 'rational inattention' case where beneficiaries put equal weight on the costs of making an active choice and the expected benefits of doing so, $\beta = 1$ and $k = 0$. The green line represents the 'boundedly-rational inattention' case where beneficiaries 'over-weight' costs relative to benefits, with $0 < \beta < 1$ and $k = 0$. The brown line represents the 'random inattention' case where beneficiaries randomly make active choices, with $\beta = c = 0$ and $k \sim U[c - (1 - \theta), c + \theta]$.

**Appendix Figure A32:** Theoretical Relationship Between Default Value and Welfare

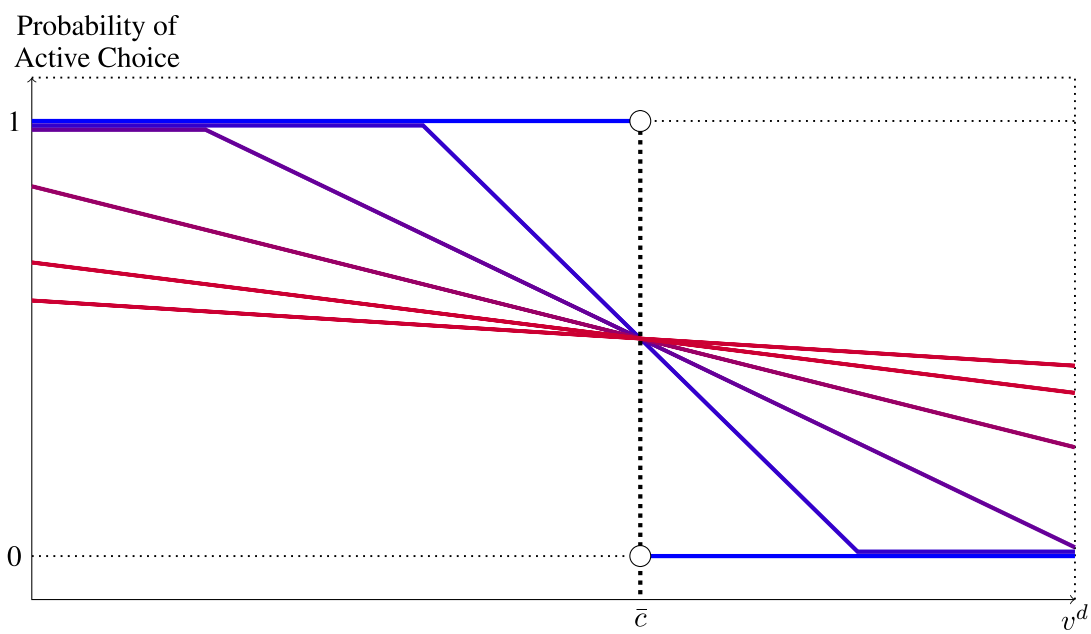



*Notes*: his figure plots the relationship between the value of the default vd against expected welfare (taking into account the probability of making an active choice) under various models of active decision-making. The blue line represents the 'rational inattention' case where beneficiaries put equal weight on the costs of making an active choice and the expected benefits of doing so, $\beta = 1$ and $k = 0$. The green line represents the 'boundedly-rational inattention' case where beneficiaries 'over-weight' costs relative to benefits, with $0 < \beta < 1$ and $k = 0$. The brown line represents the 'random inattention' case where beneficiaries randomly make active choices, with $\beta = c = 0$ and $k \sim U[c - (1 - \theta), c + \theta]$.

**Appendix Figure A33:** Welfare Effects of Default Changes




*Notes*: This figure plots the relationship between the value of the default $v^d$ against expected welfare (taking into account the probability of making an active choice) for a boundedly rational consumer with $0 < \beta < 1$ and $k = 0$. The magenta line plots the same relation when $v^d$ is changed by $\Delta v^d < 0$, with the x-axis representing $v^d$ under the initial default, such that ticks on the axis represent the same individuals across default regimes.




**Appendix Figure A34:** This figure plots the relationship between the value of the default $v^d$ and the ex ante probability of making an active choice under various distributions of cognitive costs. Each line represents an agent following a model of rational inattention with $\beta = 1$ and $k = 0$, with cognitive costs $c$ distributed randomly in $U[\bar{c} - \epsilon, \bar{c} + \epsilon]$. The blue line represents $\epsilon = 0$, while redder lines represent higher values of $\epsilon$, in $\{0.5\bar{c}, \bar{c}, 2\bar{c}, 3\bar{c}, 4\bar{c}, 8\bar{c}\}$.

# E Appendix: Structural Exit Model Derivations

In Section 7, our goal is to decompose the variance in $A$. Reframing this, we want to estimate $\frac{\text{Var}(c_i)}{\text{Var}(A_{it})}$. Assume that we had panel data where we observed $A_{it}$ directly. Then a simple regression of $A_{it'}$ on $A_{it}$ would recover the variance decomposition. To see this, note that the estimated coefficient would be $\frac{\text{Cov}(A_{it}, A_{it'})}{\text{Var}(A_{it})}$. Since $k_{it}$ are i.i.d., $\text{Cov}(A_{it}, A_{it'}) = \text{Var}(c_i)$, and so the estimated coefficient is $\frac{\text{Var}(c_i)}{\text{Var}(A_{it})}$.

However, $A_{it}$ is latent, and instead we observe $a_{it} = 1\{A_{it} \geq 0\}$. The analogue to the above regression, $P[a_{it}|a_{it'}, X]$, does not directly estimate the variance decomposition above. Instead, we must characterize how that object maps onto variances of interest.

To start, note that by construct $k$ is independent across time periods, so $k_{it'}$ is irrelevant for $a_{it}$. Therefore, the value of $a_{it'}$ in predicting $a_{it}$ is the extent to which it is informative about $c$. We can start by explicitly characterizing this information by conditioning out $c$:

$$
\begin{aligned}
P[a_{it}|a_{it'}, X] &= \int_c P[a_{it}|c, a_{it'}, X] \cdot f(c|a_{it'}, X)dc \\
&= \int_c P[a_{it}|c] \cdot f(c|a_{it'}, X)dc
\end{aligned}
$$

where the second equality comes from the fact that, by conditioning on $c$, we have removed any additional information that $a_{it'}$ or $X$ provides, since both are orthogonal to $k_{it}$, the remaining unexplained component of $a_{it}$. The second term is the information that $a_{it'}$ provides about the distribution of $c$ given $X$. This is unknown, but we can decompose it using Bayes' rule:

$$
f(c|a_{it'}, X) = \frac{P[a_{it'}|c, X] \cdot f(c|X)}{P[a_{it'}|X]}
$$

and therefore, plugging this expression into the initial formula:

$$
P[a_{it}|a_{it'}, X] = \frac{1}{P[a_{it'}|X]} \int_c P[a_{it}|c] \cdot P[a_{it'}|c] \cdot f(c|X)dc
$$

The term before the integral is the share of beneficiaries with characteristics $X$ who previously made an active choice, which can be computed directly from the data. The term in the integral has two unknowns. First, we need the conditional distribution of $a$ given $c$. This requires us to make some assumption about the distribution of $k$. Second, we need the conditional distribution of $c$ given $X$, which is an unknown that requires a parametric assumption.

In our empirical exercise, we make two distributional assumptions: That $c \sim \mathcal{N}(\mu X, \sigma^2)$, and that $k \sim \mathcal{N}(0, 1)$. The fixed mean and variance of the latter is required as a normalization, as is typical in parametric models of discrete choice. Given these assumptions, $P[a = 1|c] = P[c + k \geq 0] = 1 - \Phi(-c)$ and $P[a = 0|c] = P[c + k < 0] = \Phi(-c)$, while $f(c|X) = \phi\left(\frac{c-\mu X}{\sigma}\right)$.

With these assumptions, we can once again rewrite our conditional distribution, as

$$P[a_{it}|a_{it'}, X] = \frac{1}{P[a_{it'}|X]} \int_c [1 - \Phi(-c)]^{a_{it}+a_{it'}} \cdot [\Phi(-c)]^{2-a_{it}-a_{it'}} \cdot \phi\left(\frac{c - \mu X}{\sigma}\right) dc$$

which is the closed-form expression for the likelihood of $a_{it}$ given $a_{it'}$, which can be used in a maximum likelihood estimation procedure to estimate $\mu, \sigma^2$:

$$\hat{\mu}, \hat{\sigma}^2 = \arg\max_{\mu,\sigma^2} \sum_i \log\left[\int_c [1 - \Phi(-c)]^{a_{it}+a_{it'}} \cdot [\Phi(-c)]^{2-a_{it}-a_{it'}} \cdot \phi\left(\frac{c - \mu X}{\sigma}\right) dc\right] - \log\left[P[a_{it'}|X]\right]$$

where the latter term does not depend on $\mu, \sigma^2$ and therefore drops out.

Note that the integral within this expression has no closed form. We approximate it using Gauss-Hermite quadrature. This entails approximating the integral with

$$\frac{1}{\sqrt{\pi}} \sum_{k=1}^{n} w_k \left[1 - \Phi(-\mu - \sqrt{2}\sigma x_k)\right]^{a_{it}+a_{it'}} \cdot \left[\Phi(-\mu - \sqrt{2}\sigma x_k)\right]^{2-a_{it}-a_{it'}}$$

where $x_k$ is are the roots of the Hermite polynomial of degree $n$, $H_n(x)$, and

$$w_k = \frac{2^{n-1} n! \sqrt{\pi}}{n^2 [H_{n-1}(x_k)]^2}$$

Preliminary tests suggested that this approximation appears to converge at merely $n = 5$ nodes, for reasonable parameter values. For safety, we approximate using $n = 100$ nodes instead. We estimate standard errors in Table 7 using the bootstrap method.