Do Ordeals Work for Selection Markets? Evidence from Health Insurance Auto-Enrollment

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June 7, 2024

Abstract

Are application hassles, or "ordeals," an effective way to limit public program enrollment? We provide new evidence by studying (removal of) an auto-enrollment policy for health insurance, adding an extra step to enroll. This minor ordeal has a major impact, reducing enrollment by 33% and differentially excluding young, healthy, and economically disadvantaged people. Using a simple model, we show that *adverse selection* – a classic feature of insurance markets – undermines ordeals' standard rationale of excluding low-value individuals, since they are also low-cost and may not be inefficient. Our analysis illustrates why ordeals targeting is unlikely to work well in selection markets.

JEL codes: I13, I18, D90
Keywords: Health insurance; Ordeals; Targeting; Automatic enrollment

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A previous version was circulated with the title, "Reducing Ordeals through Automatic Enrollment." We thank Amina Abdu, Kendra Singh, Mike Yepes, and Olivia Zhao for excellent research assistance. We thank the editor and three anonymous referees for extremely thoughtful comments. We thank Jason Abaluck, Manasi Deshpande, Keith Ericson, and Ben Handel for thoughtful and constructive discussant comments. For helpful feedback and suggestions, we thank Hunt Allcott, Marcella Alsan, Chris Avery, Peter Blair, Zarek Brot-Goldberg, Sam Burn, Amitabh Chandra, Leemore Dafny, Amy Finkelstein, Peter Ganong, Josh Gottlieb, Jon Gruber, Gordon Hanson, Nathan Hendren, Alex Imas, Tim Layton, Jeff Liebman, Lee Lockwood, Amanda Kowalski, Brigitte Madrian, Sendhil Mullainathan, Matthew Notowidigdo, Carol Propper, Wesley Yin, Richard Zeckhauser, and seminar participants at the AEA meetings, ASHEcon, Boston-Area IO Conference, Brown University, Covered California, Harvard Kennedy School, Harvard-MIT-BU Health Economics, Imperial College London, Massachusetts Health Connector, Queen Mary University, USC Schaeffer, and NBER Health Care, Public Economics, and Economics of Aging meetings. We thank the Massachusetts Health Connector (particularly Michael Norton and Marissa Woltmann) for assistance in providing and interpreting the data. We gratefully acknowledge data funding from Harvard's Lab for Economic Applications and Policy and research support from Harvard Kennedy School's Rappaport Institute for Public Policy, Harvard's Milton Fund, and the National Institute on Aging, Grant Number T32-AG000186 (via the NBER). The research protocol was approved by the IRBs of Harvard University and the NBER.

1 Introduction

Should enrolling in public programs be easy or hard? The desirability of enrollment hassles, or "ordeals," for social programs is a classic – and controversial – question in public economics. On the one hand, there is substantial concern about incomplete take-up of programs intended to help the poor (Currie, 2006). A growing body of work argues that the bureaucracy, paperwork, and "administrative burden" of enrollment is a major driver of low take-up and source of frustration with and mistrust of government (Herd and Moynihan, 2019).

On the other hand, a classic line of thinking in economics argues that ordeals can be useful ways to *target* assistance towards those who need or value it most (Nichols and Zeckhauser, 1982; Besley and Coate, 1992). The basic idea follows from the logic of revealed preference. Ordeals work like a non-financial "price" of enrolling, and as in standard markets, prices screen out people with low value (demand) for a program. By excluding low-value types, the government saves money and can redirect aid towards those who need it most. This influential "self-targeting" idea has spawned an active empirical debate, with some research finding that it holds in practice (Alatas et al., 2016; Dupas et al., 2016), while other work argues that behavioral frictions may undermine its validity (Bhargava and Manoli, 2015; Finkelstein and Notowidigdo, 2019; Deshpande and Li, 2019). Importantly, the debate has been framed almost entirely around the self-targeting question: do ordeals effectively screen out *low-value* or *low-need* types in a given setting?

In this paper, we ask whether this is the right way to think about targeting in programs where people vary not just in value or need, but also in their *costs*. We observe that many programs – and especially insurance programs – share a key feature of "selection markets" that have been widely studied in the economics literature (Einav, Finkelstein and Mahoney, 2021). In these settings, enrollee costs vary substantially and tend to be *correlated* with value, often because both are driven by the same underlying factor like risk. For instance, in our health insurance data, the highest-risk (sickest) 10% of enrollees incur *15 times* higher medical costs than the healthiest 10% (about \$1400 vs. \$90 per month). Moreover, the healthy are likely to value insurance less, precisely because they have fewer medical needs and use less care. This example illustrates the key correlation in settings with adverse selection: low-value types also tend to be low-cost.

Our paper's central conceptual point is that adverse selection tends to weaken, and when strong enough undermine, the classic self-targeting case for ordeals. When low-value enrollees are also low-cost, excluding them may yield minimal, or even negative, targeting gains. The key question in selection markets is not whether ordeals screen on value, but whether they screen *more strongly* on social value than on costs. This question is theoretically ambiguous and does not follow from the standard revealed preference logic for ordeals.

We formalize this argument with a mix of theory and evidence from a public health insurance program. We use a natural experiment to study descriptively *how much* ordeals matter for take-up, and which types of people they screen out. We find that even minor hassles lead to major reductions in take-up among an otherwise uninsured low-income population. Consistent with adverse selection, the excluded group is differentially younger, healthier, and poorer – suggesting ordeals screen out people with low private value (demand) but also low cost of insurance.[1] Using an empirical model estimated with our data, we find that ordeals worsen targeting efficiency, despite successfully screening out low-value types. More generally, we show that adverse selection works alongside behavioral frictions to weaken the (revealed preference) link between demand and efficiency that is key to self-targeting. This makes ordeals relatively poorly suited tools for adverse selection markets.

We begin the paper (in Section 2) with a general framework to formalize these ideas about ordeals targeting in selection markets. Ordeals improve welfare if they yield "gains from targeting" – the ability to include efficient (*social value* $>$ *cost*) and exclude inefficient (*social value* $<$ *cost*) types – sufficient to outweigh any direct losses from their hassle or administrative costs. We show that targeting gains can be visualized in simple supply/demand-like graphs of marginal value/cost vs. quantity enrolled as ordeals vary, analogous to the approach of Einav, Finkelstein and Cullen (2010) for visualizing welfare in selection markets. As in their graphs, adverse selection implies that the "marginal cost" curve is not flat (as in a non-selection market) but *slopes downward* alongside marginal value, reflecting the positive value-cost correlation driven by enrollee risk. This shrinks the gains from targeting, reflected in a smaller area between marginal value and cost curves above and below their intersection.

We formalize this reduction in what we call the "adverse selection tax," which equals the coefficient in a regression of enrollee cost on value, or $\hat{\beta} = \frac{Cov(C_i,V_i)}{Var(V_i)} = \rho \cdot \sigma_C/\sigma_V$.[2] When adverse selection is sufficiently strong (roughly, when $\hat{\beta} > 1$), the marginal cost curve becomes steeper than marginal value, and ordeals induce *"backward sorting"* into insurance even when they correctly sort on value. This idea – analogous to the insights of Marone and Sabety (2022) for menu design and sorting with prices – shows the limits of choice and self-targeting mechanisms in adverse selection markets where demand and efficiency are often misaligned.[3]

In addition, we show a second reason adverse selection tends to undermine ordeals: it makes it more likely that the optimal outcome is *universal* – enrolling or excluding everyone – rather than targeted. We call this second idea "*optimal universality*." Graphically, it occurs when the marginal value (MV) curve lies entirely above or below marginal costs (MC), so the two do not intersect. This is more likely when both MV and MC have a similar downward slope because value and cost are strongly correlated. For instance, consider a case where social value and cost align perfectly: $V_i = \delta \cdot C_i$. In this case, net welfare ($= V_i - C_i$) equals $(\delta - 1)\, C_i$ for all i, which is uniformly positive or negative depending on $\delta \gtrless 1$. This example illustrates the key idea of optimal universality: a strong value-cost correlation makes it more likely that targeting using ordeals is counterproductive because universal outcomes are superior.

[1] This also aligns with the groups most likely to be among the 28 million uninsured in the U.S. today (Tolbert et al., 2022).

[2] Here $\rho = Corr\,(C_i, V_i)$, $\sigma_C = StdDev\,(C_i)$, and $\sigma_V = StdDev\,(V_i)$, all evaluated across potential enrollees (i). The adverse selection tax is zero if enrollee costs do not vary ($\sigma_C = 0$) or are uncorrelated with value ($\rho = 0$), and it grows as both of these increase relative to the variation in value. Note that in our notation in Section 2, this exercise applies to social value (V_i^{Soc}) and net government cost (C_i^{Net}).

[3] Conversely, *advantageous* selection – where low-value types have high costs – strengthens the case for ordeals targeting. Because advantageous selection is less common, we do not discuss it in detail. Two settings where it has been found are long-term care insurance (Finkelstein and McGarry, 2006) and Medicare supplemental coverage ("medigap") (Fang, Keane and Silverman, 2008).

Having developed this framework, we next turn to an empirical analysis of ordeals that lets us both estimate the key model parameters and also learn descriptively about ordeals' impact for health insurance programs. Our empirical setting is the Massachusetts health insurance exchange, a program offering subsidized insurance to low-income people without access to other coverage.[4] The program featured a unique source of variation in the complexity of enrollment, driven by changing use of an auto-enrollment policy for the program's poorest individuals, who qualified for free insurance. Prior to 2010, the program required only that these individuals *apply* for coverage, submitting paperwork with information to verify eligibility. Approved applicants were then contacted and asked to choose among several plans offered by different insurers (all of which were free). But if they failed to respond – something that occurred surprisingly often – the program *auto-enrolled* them into a plan using a simple algorithm. In essence, this policy used defaults or "choice architecture" (Thaler, 2018) to streamline take-up and prevent people from falling through the cracks of the system.

Starting in 2010, the program suspended auto-enrollment. Non-responsive, or "passive," individuals were no longer enrolled by default; instead, their default became *non-enrollment*. Effectively, this change added an extra step (active plan choice) to the required take-up process. Although not intended to be onerous – people could choose by phone, mail, or online, and all plans remained free – this change is an example of the type of small take-up friction that is common in many U.S. safety net programs.

We use this variation to estimate the causal effect of the ordeal by studying enrollment changes around the 2010 policy shift. We use a difference-in-difference design, comparing changes in new enrollment for the low-income (treatment) group for whom auto-enrollment stops in 2010 versus a slightly higher-income (control) group for whom it was not used throughout. Our rich administrative data let us observe who enrolled actively vs. passively prior to 2010, and we can also infer the characteristics of marginal enrollees from compositional changes in enrollment around 2010.

This analysis yields two main findings. First, adding a minor ordeal leads to major reductions in health insurance take-up. Prior to 2010, one-third of low-income new enrollees join the exchange passively via auto-enrollment. When the policy is suspended in 2010, the flow of new enrollment falls by a nearly identical 33%. The decline is immediate and persistent, with parallel pre-trends and no concurrent changes for the control group.[5] We also see no evidence of an uptick in active enrollment in 2010, suggesting that passive individuals are unlikely to be deliberately choosing non-response (e.g., because they know they will be auto-enrolled). Rather, when subjected to a small hassle, about one-third of eligible individuals simply fail to take up health insurance.

This effect is quite large. For instance, it is similar to the impact of a $470 (or 57%) annual premium increase based on prior evidence (Finkelstein, Hendren and Shepard, 2019b), and 1.25-2 times larger than the impact of Massachusetts' uninsurance penalty (Chandra, Gruber and McKnight, 2011). It

[4] We study the pre-Obamacare (or ACA) exchange, which operated from 2007-2013 and was called Commonwealth Care (or "CommCare"). As a model for the ACA exchanges that followed, CommCare has been a rich source of evidence on demand, competition, and the impact of policies in health insurance markets (see Chandra et al., 2011, 2014; Finkelstein et al., 2019b; Jaffe and Shepard, 2020; McIntyre et al., 2021; Shepard, 2022; Shepard and Forsgren, 2023).

[5] Further evidence comes from a temporary reinstatement of the auto-enrollment policy in late 2010. Consistent with the policy having a causal effect, we find that new enrollment spikes back up to its pre-2010 level, then falls back down when auto-enrollment is again suspended in early 2011.

is an order of magnitude larger than the 1-4% point effects observed from lower-touch "nudges" (like outreach and assistance) in recent work on health insurance (Goldin et al., 2021; Domurat et al., 2021; Ericson et al., 2023). The findings suggest that *fully automatic* enrollment – not just incremental incentives and nudges – may be a key step to further reduce uninsurance in the U.S.

Our second descriptive finding is that ordeals differentially screen out low-risk individuals, consistent with adverse selection. Relative to active enrollees, passive enrollees are younger and healthier (e.g., 33% less likely to be chronically ill), and especially likely to be young men age 19-34. They incur 44% lower medical spending per month – most of which (a 36% gap) is predictable by their age and diagnosis risk factors. Because of their lower costs, excluding passive enrollees results in a 15% higher average-cost risk pool of enrollees.

We also examine the distributional equity implications of ordeals. We find that passive enrollees are more likely to be very low-income, to live in disadvantaged neighborhoods, and to live near safety net hospitals and clinics. This is consistent with ordeals differentially impacting the poor (Bertrand et al., 2004; Mullainathan and Shafir, 2013). But it is also consistent with evidence that the poor have lower *demand* for health insurance, potentially because of access to charity care when uninsured (Finkelstein, Hendren and Luttmer, 2019a).

Why does a seemingly small hassle matter so much for enrollment? This fact is striking because the benefits of foregone health insurance are likely meaningful.[6] Our evidence is most consistent with behavioral frictions like inattention, forgetting to act, or simply "going with the flow" in insurance choices.[7] We examine but find little evidence of other explanations including stigma or unawareness of the program (since everyone in our sample has already applied for coverage), "choice overload" that leads to passivity (Iyengar and Kamenica, 2010), or passive enrollees already having another form of duplicate insurance.[8]

The final portion of our paper applies the ordeals welfare framework to our setting using the auto-enrollment natural experiment. We specify a rich model allowing for the key features of insurance problem, including heterogeneity in enrollee value (demand), insurer cost (based on medical claims data), and externalities of insurance via savings on uncompensated care. The key empirical challenge – common to most analyses of ordeals – is to infer enrollee value of insurance, given the non-price nature of the take-up barrier. We address this challenge by estimating demand among a higher-income segment of exchange enrollees who face positive prices, drawing on RD-style premium variation used in prior work (Finkelstein, Hendren and Shepard, 2019b). We then project these demand estimates

[6] Passive enrollees (while healthier than average) do use significant medical care and experience medical shocks. Based on our model estimates and prior work on the value of health insurance (Finkelstein, Hendren and Luttmer, 2019a), coverage should be worth about \$550 to \$1,300 for an average passive enrollee over a typical yearlong spell. This is comparable to foregone benefits from failure to take up the EITC or SNAP (Bhargava and Manoli, 2015; Finkelstein and Notowidigdo, 2019).

[7] Consistent with these ideas, we find that passive non-response is more common among immigrants (who may face language barriers), people with signs of address instability, and people transitioning into the exchange from Medicaid (which may involve greater confusion because Medicaid's process is different).

[8] We test this using the state's All Payer Claims Database where we can see the near-universe of health insurance coverage. We see very low rates ($< 4\%$) of duplicate enrollment in the exchange plus other coverage, and no meaningful change in duplication rates around the end of auto-enrollment in 2010.

onto the lower-income population at the level of key observables (cells of age, sex, and medical risk scores). We consider various assumptions for the role of unobserved preferences, as well as alternate methods of estimating value directly from observed medical use in our claims data.

This exercise yields three main results. First, ordeals do screen out lower-value enrollees. In our baseline estimate, passive enrollees have a private (social) value of coverage that is 28% (34%) lower than active types. This finding, which is consistent with the classic ordeals rationale of self-targeting, is robust across a wide range of specifications we consider.

Second, adverse selection substantially reduces, or even reverses, the ordeal's targeting gains. Our estimates suggest substantial cost variation and a strong value-cost correlation that implies an "adverse selection tax" that is large and often exceeds 100%. Correspondingly, the value-cost *ratio* of passive enrollees is similar to or (in our main specification) higher than active enrollees, suggesting that ordeals induce counterproductive "backward sorting" into insurance. We also examine the robustness of this conclusion to varying distributional equity goals, by applying a social welfare weight $\mu > 1$ to enrollee welfare. We find that with even modest equity concerns ($\mu > 1.3$), it becomes optimal to enroll *both* active and passive individuals. The ordeal is still non-optimal — but not because sorting is backwards, rather because the optimal outcome is universal.

Finally, we use the model to compare auto-enrollment vs. subsidies as ways of expanding take-up. We find that the two have similar targeting properties – both enroll a similar young, healthy, and low-cost population – but that auto-enrollment is much more cost-effective because it does not require new spending on inframarginal enrollees. We find that each extra $1 million in public spending covers 55-66% more people if used for auto-enrollment rather than subsidies.

Related Literature Our paper contributes to three main strands of literature. The first studies the nature of ordeals targeting for social programs. Starting from the classic analysis of Nichols and Zeckhauser (1982), the debate has centered around whether ordeals screen out people who value or benefit less from assistance (e.g., Alatas et al., 2016; Dupas et al., 2016; Finkelstein and Notowidigdo, 2019) or who benefit just as much but have less ability to navigate a complex process (e.g., Bhargava and Manoli, 2015; Deshpande and Li, 2019; Homonoff and Somerville, 2021). This debate is part of a broader literature asking when non-price targeting is valuable in social programs (e.g., Kleven and Kopczuk, 2011; Lieber and Lockwood, 2019). We provide evidence in a new and important setting (health insurance) and highlight that the classic debate misses the key role of cost heterogeneity and adverse selection for this question.

Second, our paper contributes to work evaluating "nudges" to increase take-up of social programs, including health insurance (Goldin et al., 2021; Domurat et al., 2021; Banerjee et al., 2021; Ericson et al., 2023). Our results suggest a much larger impact of fully *removing* hassles by changing the default to auto-enrollment. This complements prior work on the large impact of auto-enrollment in other settings (e.g., Madrian and Shea, 2001; Chetty et al., 2014),[9] as well as evidence that defaults

[9]Recent work on 401(k) pensions by Choukhmane (2021) finds that while auto-enrollment has a large *initial* impact on enrollment and savings, people who are not auto-enrolled largely catch up by saving more in the future. Unlike pensions, health insurance is a domain where failure to enroll can have immediate repercussions if an individual gets sick

create inertia in choosing *among* insurance plans (Handel, 2013; Ericson, 2014; Polyakova, 2016; Brot-Goldberg et al., 2021). Default effects are a key example of a broader set of "choice frictions" that have been shown to be prevalent in health insurance markets (Abaluck and Gruber, 2011; Bhargava et al., 2017; Abaluck and Gruber, 2023). Our paper shows that defaults are also important policies for insurance take-up.

Finally, our paper contributes to the literature asking why uninsurance is so persistent in the U.S. A large prior literature has analyzed the impact of financial prices and subsidies for incomplete take-up (Gruber, 2008; Dague, 2014; Frean et al., 2017; Finkelstein et al., 2019b). We show that ordeals and hassles are also likely to be a key barrier, given the U.S.'s fragmented and non-automatic health insurance system. There is growing interest in the role of complexity, transaction costs, and "administrative burden" in shaping enrollment, with emerging evidence that this matters for Medicaid take-up (Aizer, 2007; Arbogast et al., 2022; Wu and Meyer, 2023) and for ACA health insurance marketplaces (Drake et al., 2023; McIntyre et al., 2024). We show, likewise, that imposing even modest hassles leads to non-enrollment by a large share of people – especially the young, healthy, and poor who are disproportionately uninsured today. Our results suggest that as long as take-up is voluntary, getting to universal coverage will likely require some form of auto-enrollment. They also illustrate the surprising power of a feasible form of auto-enrollment that has recently been considered or implemented in several states' ACA exchanges.[10]

Outline of Paper Section 2 presents a conceptual framework for ordeals targeting with adverse selection. Section 3 discusses the setting, the auto-enrollment policy, and our data. Section 4 shows our main results on enrollment impacts, and section 5 presents targeting results. Section 6 implements our empirical model using the auto-enrollment variation. Finally, section 7 concludes.

2 Conceptual Model: Adverse Selection and Ordeals Targeting

In this section, we present a simple framework for the economics of ordeals in programs characterized by adverse selection, that is where enrollee value and costs are positively correlated. Adverse selection is a classic feature of insurance, where individual risk (e.g., health status) is the primary driver of the value-cost correlation. But it is also relevant more generally for transfer programs with varying benefit amounts (e.g., by income or family size), since people who receive smaller benefits also cost less to the government. Our central point is that adverse selection reduces – and may even reverse – the efficiency of the standard ordeals rationale of screening out *low-value* types, since low-value enrollees may not be *inefficient* enrollees.

This section formalizes this argument using a simple model based on the classic insights of Nichols and Zeckhauser (1982), as well as the more recent ordeals framework of Finkelstein and Notowidigdo (2019). Our key innovation is to connect ordeals to the economics of selection markets, visualized

and incurs medical bills. This suggests auto-enrollment is likely to be a consequential policy for health insurance.

[10] This includes Massachusetts, which reinstated a similar form of auto-enrollment in April 2022, partly based on discussions with them about this research.

using the graphical framework of Einav, Finkelstein and Cullen (2010). Our analysis also connects to recent insights about "backward sorting" in selection markets (Marone and Sabety, 2022), in which prices also lead to inefficient sorting between insurance options.

2.1 Model Setup

Consider a population of individuals who qualify for a public program – in our setting, free health insurance – but have not yet enrolled. For each individual i, the program generates social value of

$$V_i^{Soc} \;=\; \mu_i W_i \;+\; E_i \tag{1}$$

where W_i is the program's private welfare to enrollee i (willingness-to-pay), μ_i is the marginal social welfare weight on individual i (capturing distributional equity concerns), and E_i is the social value of any externalities from i's participation in the program. A Kaldor-Hicks efficiency welfare criterion would involve $\mu_i = 1$ for all i, but it may be natural to think of $\mu_i > 1$ for safety net programs where beneficiaries are lower-income. For our empirical work, we simplify by treating μ_i as a constant μ for everyone who qualifies for the program, but in principle μ_i could vary across eligible groups to capture distributional goals.

For individual i, the program involves net government cost $C_i^{Net} = C_i - FE_i$, which equals direct costs (C_i) minus any offsetting fiscal externalities (FE_i).[11] We assume $C_i^{Net} > 0$ so that there is a real fiscal tradeoff of expanding enrollment. Both social value and cost may vary across individuals, potentially creating a rationale for targeting.

The government seeks to target enrollment to maximize total social benefits net of costs. Mathematically, if $A_i \in \{0, 1\}$ indicates whether i is enrolled, the government seeks to maximize net social welfare, or $SW = \sum_i \left(V_i^{Soc} - C_i^{Net}\right) \cdot A_i$. We define γ_i as the net contribution to social welfare of enrolling individual i:

$$\text{(Net Welfare)} \qquad \gamma_i \;=\; V_i^{Soc} - C_i^{Net} \;=\; (\mu_i W_i + E_i) - C_i^{Net} \tag{2}$$

If the government had full information, it would optimally enroll everyone for whom $\gamma_i \geq 0$ and exclude those with $\gamma_i < 0$. Equivalently, if we define $R_i \equiv V_i^{Soc} \,/\, C_i^{Net}$ as the enrollee's *"social value-cost ratio,"* the government optimally enrolls everyone with $R_i \geq 1$ and excludes those with $R_i < 1$.[12] The metric γ_i is a useful targeting index that shows how a government would optimally prioritize enrollment with full information. In practice, however, the government has limited information, so it must use blunt policies like ordeals, which we turn to next.

[11] In our empirical setting we think of these variables as follows. $W_i > 0$ is the benefits of insurance to the individual; $C_i > 0$ is the government's direct subsidy cost for insuring them; and E_i, $FE_i \geq 0$ are savings on (uninsured) uncompensated care borne by private hospitals (E_i) and the government (FE_i). The nature of C_i depends on how insurance is provided. We assume either direct public provision (relevant in programs like Medicaid) or zero-profit contracting with private insurers (which we find to be roughly true in the Massachusetts exchange), which implies that C_i equals i's expected insured medical costs.

[12] The social value-cost ratio is closely related to the marginal value of public funds (MVPF) metric (Hendren, 2016), which is also a (policy-level) benefit-cost ratio.

Ordeals and Take-Up The government has access to a screening mechanism – in our setting, an ordeal – that it uses to limit take-up. Ordeals work by imposing a "friction," $\eta_i \geq 0$, that individuals must overcome to enroll. The friction may vary across individuals and could involve both real costs (e.g., the time and effort of completing paperwork) and behavioral frictions that limit take-up (e.g., inattention). We assume the government can adjust the "intensity" of the ordeal through its policy choices (e.g., how much paperwork to impose). A simple specification that captures this idea is $\eta_i = \sigma \cdot h_i$, where $\sigma \geq 0$ is the ordeal's intensity (a policy choice) and $h_i \geq 0$ captures a person's experienced hassle cost per unit ordeal. The policy of no ordeal is equivalent to setting $\sigma = 0$.

In addition to the ordeal, people may have behavioral biases that affect demand – e.g., biased beliefs about their risk type (Spinnewijn, 2017). We denote the bias by ε_i, and the utility governing take-up as $U_i \equiv W_i - \varepsilon_i$, where $\varepsilon_i > 0$ captures under-valuation and $\varepsilon_i < 0$ over-valuation. With the ordeal in place people take up the program if:

$$\text{(Take-Up)} \qquad U_i = \underbrace{W_i}_{\text{True WTP}} - \underbrace{\varepsilon_i}_{\text{Bias}} \geq \underbrace{\sigma \cdot h_i}_{\text{Ordeal friction}} \tag{3}$$

A comparison of the conditions for who should optimally enroll ($\gamma_i \geq 0 \iff \mu_i W_i + E_i - C_i^{Net} \geq 0$) versus actual take-up ($W_i - \varepsilon_i - \sigma h_i \geq 0$) shows that there may be both under- and over-enrollment among differing groups. All else equal, under-enrollment is more likely for disadvantaged groups (with high welfare weights, $\mu_i > 1$), for people with positive externalities ($E_i > 0$) or under-valuation bias ($\varepsilon_i > 0$), and for people with low cost (C_i^{Net}) relative to WTP. Over-enrollment is more likely for the opposite cases. Imposing an ordeal improves targeting if it reduces over-enrollment more than it exacerbates under-enrollment, in a sense that we formalize below.[13]

We denote the share of people who enroll given an ordeal of intensity σ as $D(\sigma) = Pr(W_i - \varepsilon_i \geq \sigma h_i)$. The share excluded is $1 - D(\sigma)$. The ordeal splits potential enrollees into two groups. For any variable X_i (e.g., value or cost), we denote averages for screened-in enrollees as $\overline{X}_1(\sigma) \equiv E[X_i \,|\, W_i - \varepsilon_i \geq \sigma h_i]$ and for excluded individuals as $\overline{X}_0(\sigma) \equiv E[X_i \,|\, W_i - \varepsilon_i < \sigma h_i]$.

In addition to their impact on take-up, ordeals may impose "direct" or "excess" costs, including both hassle/psychological costs to enrollees and administrative costs to the government. The nature of these costs depends on the specifics of the ordeal and the model of behavior and welfare (Ericson, 2020).[14] Rather than specify it in detail, we write the ordeal's total direct/excess cost as a general

[13] One way to understand misallocation is to define the "wedge" between optimal enrollment vs. take-up utility (absent the ordeal), as:

$$\Delta_i \equiv \gamma_i - U_i \;=\; [(\mu_i - 1) W_i + E_i + \varepsilon_i] - C_i^{Net} \tag{4}$$

In an ideal world, this take-up wedge would be zero, ensuring that people enrolled if and only if $\gamma_i \geq 0$. Imposing an ordeal works like a reduction in take-up utility, so it shifts the wedge from Δ_i to $(\Delta_i + \sigma h_i)$. This will tend to improve welfare if the distribution of $(\Delta_i + \sigma h_i)$ is closer to zero than the distribution of Δ_i. This point is related to the result of Allcott et al. (2022) that "nudges" tend to improve welfare if they reduce the *variance* of net wedges between socially optimal and actual consumption of a good.

[14] In the classic model, ordeals impose a "real" hassle cost on enrollee i of σh_i, which is identical to their impact on take-up behavior, but no costs on non-enrollees (who need not incur the hassle) or administrative costs for the government. Thus, in the classic setup, $L(\sigma) = D(\sigma) \cdot \sigma \overline{h}_1(\sigma)$. However, Ericson (2020) notes that policies like defaults may impact take-up through behavioral frictions like inattention that do not involve real welfare costs for (already-attentive) enrollees.

function, $L(\sigma) \geq 0$, which we assume is weakly positive. As we show below, direct costs are separable from the effect of ordeals on social welfare via *targeting* (who is enrolled vs. excluded), which is our focus in this paper.

2.2 When Are Ordeals Optimal?

We now lay out the general conditions under which an ordeal is desirable, which we relate to adverse selection in the next subsection. Consider an ordeal of strength σ that generates enrollment $D_1(\sigma)$. Net social welfare under this policy is

$$SW_{Ordeal}(\sigma) \;=\; D(\sigma) \cdot \underbrace{\left[\overline{V}_1^{Soc}(\sigma) - \overline{C}_1^{Net}(\sigma)\right]}_{=\overline{\gamma}_1(\sigma)} - L(\sigma) \tag{5}$$

where $L(\sigma) \geq 0$ is the total direct cost of the ordeal via hassles and administrative costs. To be welfare-improving, an ordeal must at least be superior to two trivial alternate policies:

1. **Shutting down the program (*no enrollment*)**, which results in $SW_0 = 0$, and
2. **Enrolling everyone (*full enrollment*),** which results in $SW_1 = E[\gamma_i] \equiv \overline{\gamma}$.

Relative to these alternatives, the ordeal's extra social welfare is $\Delta SW_{Ordeal}(\sigma) = SW_{Ordeal}(\sigma) - \max\{0, \overline{\gamma}\}$, or:[15]

$$\Delta SW_{Ordeal}(\sigma) \;=\; \underbrace{\min\{D(\sigma)\overline{\gamma}_1,\;\; (1 - D(\sigma)) \cdot (-\overline{\gamma}_0)\}}_{\text{Gains from Targeting, } GT(\sigma)} \;-\; \underbrace{L(\sigma)}_{\text{Direct cost}} \tag{6}$$

where we now suppress the dependence of $\overline{\gamma}_{0/1}(.)$ on σ for conciseness. The first term in expression (6) is the ordeal's "gains from targeting," or $GT(\sigma)$. This captures how effectively the ordeal screens or "targets" enrollment to positive net-welfare individuals ($\gamma_i > 0$), relative to the alternatives of full exclusion and inclusion. We show below that $GT(\sigma)$ corresponds exactly to areas between (appropriately defined) marginal value and cost curves of an ordeal, allowing us to display these gains graphically. The second term, $L(\sigma)$, is the ordeal's total direct costs, which need not be incurred if the government simply excludes or includes everyone.

The key take-away of this expression is that an ordeal is desirable only if it achieves positive gains from targeting large enough to exceed the ordeal's direct costs. Positive gains from targeting, in turn, requires that included groups be favorable (positive net welfare) and excluded groups be unfavorable (negative net welfare):

$$\text{(Positive Gains from Targeting)} \qquad \overline{\gamma}_1(\sigma) \;>\; 0 \;>\; \overline{\gamma}_0(\sigma) \tag{7}$$

Additionally, some barriers like stigma may impose psychological costs even on non-enrollees. The general $L(\sigma)$ allows our model to capture any of these cases.

[15] To derive this, we use the fact that $\overline{\gamma}$ is the welfare of the average enrollee in the full population, so for any σ, $\overline{\gamma} = D_1(\sigma)\overline{\gamma}_1(\sigma) + (1 - D_1(\sigma))\overline{\gamma}_0(\sigma)$. Note that our analysis implicitly normalizes the size of the full population (enrollees plus non-enrollees) to be 1.0.

A necessary condition for (7) is that the ordeal induces "effective targeting" between included and excluded groups, or $\Delta\gamma \equiv \overline{\gamma}_1 - \overline{\gamma}_0 > 0$. We call the term $\Delta\gamma$ the "*targeting efficacy.*" It is straightforward to show that $GT(\sigma) > 0$ only if $\Delta\gamma > 0$ and that $GT(\sigma)$ is an increasing function $\Delta\gamma$.[16]

There are two reasons the gains from targeting condition in (7) may fail — both of which, we will argue, become more likely with adverse selection. The two reasons are:

1. **Backward Sorting:** $\overline{\gamma}_1(\sigma) < 0 < \overline{\gamma}_0(\sigma)$. The ordeal sorts "backwards" by including inefficient and excluding efficient enrollees. Note that this implies ineffective targeting, or $\Delta\gamma < 0$.

2. **Optimal Universality:** Either $\overline{\gamma}_1, \overline{\gamma}_0 > 0$ or $\overline{\gamma}_1, \overline{\gamma}_0 < 0$. It is better to to simply include or enroll everyone, rather than screening with the ordeal. Note that this may be true even if targeting is "effective" ($\Delta\gamma > 0$).

In our empirical work, we analyze these conditions for a *particular* ordeal (at a given intensity σ), since this is what we observe. Conceptually, with more variation, these conditions could be assessed *globally* across all $\sigma > 0$ for a given ordeal, which is what we depict in our graphs below.

The Classic Ordeals Debate How do these conditions for ordeal desirability relate to the classic ordeals debate? The classic rationale for ordeals going back to Nichols and Zeckhauser (1982) is that they result in "self-screening" or "self-targeting," in which people who highly value the program enroll, while low-value types drop out. Intuitively, hassle costs screen consumers just like prices in standard markets, with high-value consumers willing and low-value consumers unwilling to buy a good. In its classic formulation, self-screening is a statement about screening on private welfare, W_i. Under self-screening,

$$\text{(Self-screening)} \qquad \Delta W \equiv \overline{W}_1 - \overline{W}_0 > 0. \tag{8}$$

In a model without behavioral biases ($\varepsilon_i = 0$) and homogeneous hassle costs ($h_i = \overline{h}\ \forall i$), self-screening must hold as a consequence of rational choice. The classic critiques of self-screening, therefore, focus on ways that biases or hassles may be larger for high-value types – in our notation, $Cov(W_i, \varepsilon_i) > 0$ and/or $Cov(W_i, h_i) > 0$. For instance, work on the "psychology of scarcity" argues that the poor, for whom social programs are especially valuable, may also experience the largest biases and hassle costs of overcoming ordeals (Bertrand et al., 2004; Mullainathan and Shafir, 2013).[17]

Notice, however, that self-screening on *private* welfare (W_i) is not equivalent to favorable screening on *social value*, $V_i^{Soc} = \mu_i W_i + E_i$. This distinction is often missed in ordeals analyses that do not clearly delineate private vs. social value. We say that an ordeal achieves favorable *social value sorting*

[16] The gains from targeting from (6) yields:

$$GT(\sigma) \ = \ D_1(\sigma)(1 - D_1(\sigma)) \cdot \Delta\gamma \ - \ K(\overline{\gamma}),$$

where $K(\overline{\gamma}) \equiv \max\{(1 - D_1)\overline{\gamma},\ -D_1 \cdot \overline{\gamma}\} \geq 0$ is a (non-negative) correction that captures the fact that targeting is less desirable when a program's overall average welfare ($\overline{\gamma}$) is either very positive or very negative. Because the second term subtracts a non-negative value, $GT(\sigma) > 0$ only if $\Delta\gamma > 0$.

[17] In a related vein, Spinnewijn (2015) and Spinnewijn (2017) argue that behavioral biases tend to reduce the slope of the social value curve relative to demand, making revealed preference sorting less efficient.

if:

$$\text{(Social Value sorting)} \qquad \Delta V^{Soc} \equiv \overline{V}_1^{Soc} - \overline{V}_0^{Soc} > 0. \tag{9}$$

In addition to the ways self-screening can fail, social value sorting can fail if ordeals differentially exclude people with high-welfare weights (μ_i) or with large positive externalities (E_i). This is likewise consistent with the "psychology of scarcity" ideas if ordeals differentially screen out poorer individuals (for whom μ_i is larger in standard welfare functions).

However, we emphasize that the right metric of targeting is not private welfare or even social value, but net social welfare, $\gamma_i = V_i^{Soc} - C_i^{Net}$, or what we have called favorable *targeting efficacy*:

$$\text{(Targeting efficacy)} \qquad \Delta\gamma \equiv \overline{\gamma}_1 - \overline{\gamma}_0 = \underbrace{\left(\overline{V}_1^{Soc} - \overline{V}_0^{Soc}\right)}_{\text{Social Value sorting}} - \underbrace{\left(\overline{C}_1^{Net} - \overline{C}_0^{Net}\right)}_{\text{Cost sorting}} > 0 \tag{10}$$

It is straightforward to see that targeting efficacy and value sorting coincide only in the special case where there is no offsetting sorting on costs. This is reasonable for programs with *constant costs*, or more generally where costs are *uncorrelated* with value. For example, this might be reasonable for slots in a public childcare program, or for a welfare program that gives everyone the same benefit amount. But it is unlikely to apply to insurance programs and other settings characterized by cost heterogeneity and adverse selection, which we turn to next.

2.3 Ordeals Targeting and Adverse Selection

How do the conditions for ordeals being optimal relate to adverse selection? In this subsection, we use our model to analyze the social welfare impact of ordeals. We show that the targeting impacts of ordeals can be visualized in a simple graphical framework, following the approach of Einav and Finkelstein (2011) for selection markets. This lets us visualize the role of adverse selection for the gains from targeting, and therefore the desirability of ordeals.

While the classic ordeals debate has tended to focus on the wedge between individual choice and enrollee's true private welfare (W_i) or true social value (V_i^{Soc}), we use our framework to illustrate how the economics of adverse selection can create an analogous wedge between V_i^{Soc} and *net* social welfare, $\gamma_i = V_i^{Soc} - C_i^{Net}$. Thus, even when ordeals successfully induce self-screening and favorable value sorting, adverse selection can erode or even reverse the gains from targeting.

Adverse Selection and Targeting Adverse selection is a feature typically associated with insurance and other "selection markets" where it is known to unravel trade and distort market outcomes. However, the underlying features driving adverse selection may also be relevant for thinking about targeting in social programs. These two key features are:

1. **Cost Heterogeneity:** C_i^{Net} varies across enrollees (with variance $\sigma_C^2 > 0$).
2. **Value-Cost Correlation:** C_i^{Net} correlates positively with V_i^{Soc}, or $\rho = Corr\left(V_i^{Soc}, C_i^{Net}\right) > 0$.[18]

[18] In many settings, this condition is presented as a positive correlation between direct costs C_i and private welfare

These two features characterize many insurance programs where an individual's value (demand) and cost are both heavily driven by their risk. For instance, in health insurance, sicker individuals tend to have both higher value for insurance and higher expected costs. Adverse selection tends to result in $\overline{C}_1^{Net} - \overline{C}_0^{Net}$ having the same sign as $\overline{V}_1^{Soc} - \overline{V}_0^{Soc}$. Under adverse selection, positive value sorting ($\overline{V}_1^{Soc} - \overline{V}_0^{Soc} > 0$) is not enough for an ordeal to be desirable; it is possible to have small or even negative targeting efficacy ($\Delta\gamma \approx 0$ or $\Delta\gamma < 0$) if sorting on costs is sufficiently large.

While we focus on adverse selection, *advantageous* selection may be relevant in some settings like long-term care insurance. Under advantageous selection, costs vary ($\sigma_C^2 > 0$) but the value-cost correlation is negative ($\rho < 0$). As a result, ordeals will generally target more effectively than without selection, since low-value types (who self-screen out) will also have high costs.

Graphical Analysis We show that the gains from targeting under adverse selection can be illustrated using the familiar graphical framework of Einav, Finkelstein and Cullen (2010) ("EFC") for welfare in selection markets. The intuition is that different levels of the intensity of an ordeal, given by σ in our framework, trace out marginal value and marginal cost curves in much the same way as different prices generate demand and marginal cost curves in the original EFC analysis. For a given ordeal of strength σ, we define the marginal social value curve $MV(\sigma) = E\left[V_i^{Soc} \,|\, W_i - \varepsilon_i = \sigma h_i\right]$ as the expected social value of those for whom a marginally stronger ordeal would cause not to enroll. Likewise, we define the marginal cost curve as $MC(\sigma) = E\left[C_i^{Net} \,|\, W_i - \varepsilon_i = \sigma h_i\right]$. It is straightforward to see that the conditional means in equation (10) ($\overline{V}_1^{Soc}$, $\overline{V}_0^{Soc}$, $\overline{C}_1^{Net}$ and $\overline{C}_0^{Net}$) are the average values of $MV(\sigma)$ and $MC(\sigma)$ to the left and right of $D(\sigma)$.

The key impact of adverse selection in this framework is to make the marginal cost curve *downward-sloping*, since low-value types also have low costs. This, we argue, reduces or reverses an ordeal's gains from targeting, potentially leading to backward sorting. Further, it makes more likely that $MV(\sigma)$ lies entirely above or below $MC(\sigma)$, the condition for optimal universality.

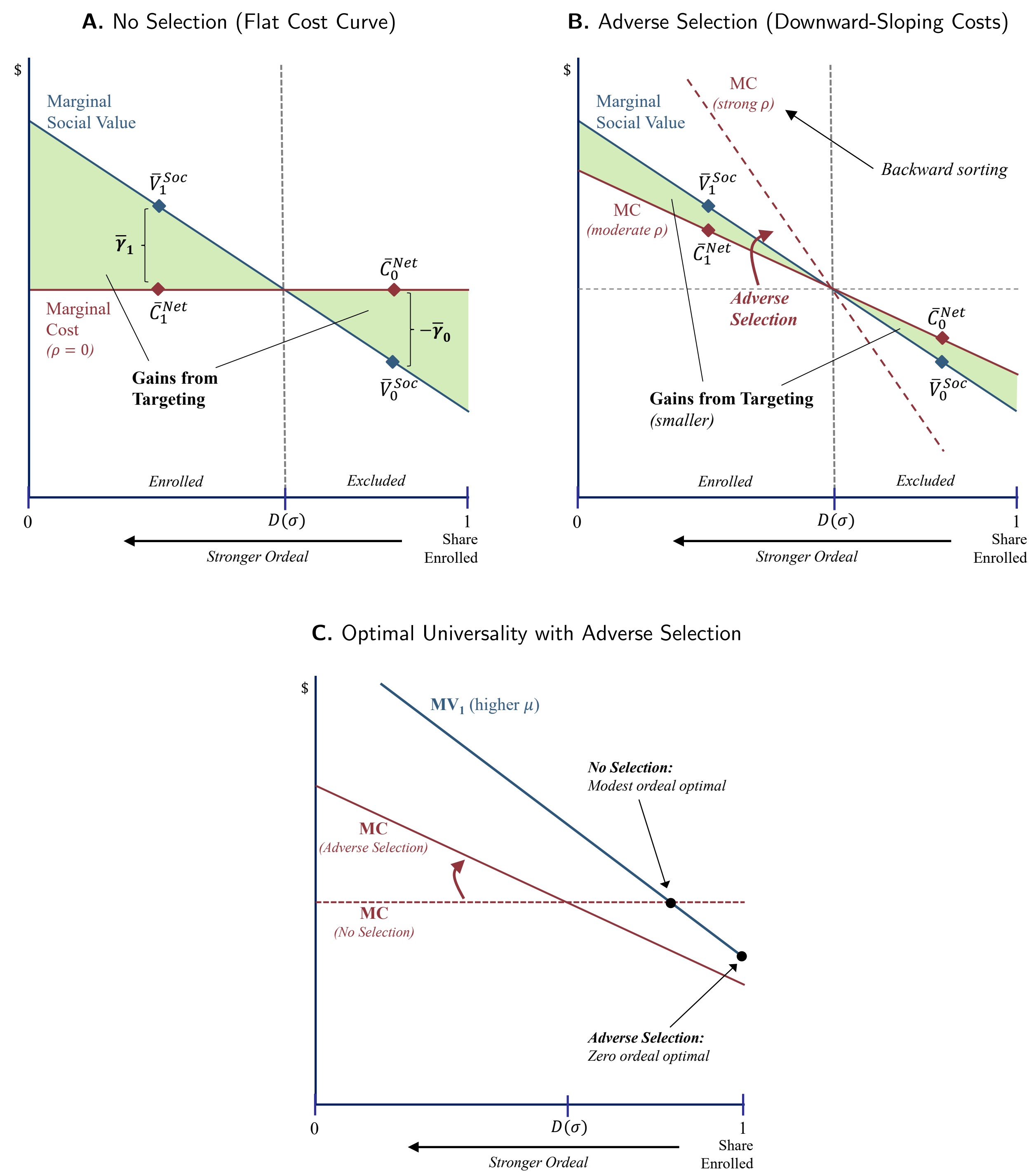
Figure 1 illustrates this adverse selection logic graphically. We start with Panels A-B, which show how adverse selection reduces or reverses the gains from targeting. The curves in each panel depict the marginal social value (blue) and cost (red) curves as the ordeal gets stronger (moving right to left) – an ordeals version of standard demand and marginal cost curves from Einav, Finkelstein and Cullen (2010). The diamonds are average value and cost for included/excluded enrollees under an ordeal, optimally set to maximize targeting gains. In panels A and B, we show the same downward-sloping marginal value curve, reflecting the case in which the ordeal favorably sorts on social value, $\overline{V}_1^{Soc} - \overline{V}_0^{Soc} > 0$. The areas between the value and cost curves, shaded in green, correspond to the gains from targeting, $GT(\sigma)$,[19] and are increasing in $\Delta\gamma = \overline{\gamma}_1 - \overline{\gamma}_0$, as shown in the graph.

Panel A illustrates the classic ordeals case with no selection (i.e., where costs are constant or uncorrelated with value), represented by a flat marginal cost curve that intersects marginal value at an interior point. As a result, targeting efficacy ($\overline{\gamma}_1 - \overline{\gamma}_0$) is equivalent to social value sorting ($\overline{V}_1 - \overline{V}_0$)

W_i. For the purpose of this discussion, we assume that W_i and V_i^{Soc} are highly correlated, as are C_i and C_i^{Net}, so these conditions are aligned.

[19] Technically, gains from targeting equals the smaller of the two shaded triangles.

Figure 1: Gains from Ordeals Targeting with No Selection vs. Adverse Selection



Note: Panels A-B show the targeting gains from ordeals in two cases: (1) the "standard" ordeals case without selection (a flat marginal cost curve, panel A), and (2) with adverse selection (downward-sloping cost curve, panel B). Both panels depict enrollee value and cost curves for marginal enrollees as the ordeal strengthens and enrollment drops (moving right to left), using a setup similar as Einav, Finkelstein and Cullen (2010). The green shaded areas are the "gains from targeting," which shrinks or becomes negative under adverse selection. Panel C shows how adverse selection increases the likelihood of "optimal universality" when the MV curve shifts upward due to a higher social welfare weight, μ. With no selection, MV_1 still intersects MC, implying that a (more modest) ordeal is still optimal. With adverse selection, MV_1 lies entirely above MC, implying full enrollment (zero ordeal) is now optimal.

because there is zero sorting on cost. An ordeal, therefore, achieves positive gains from targeting as long as the value curve is downward sloping, that is $\Delta V^{Soc} > 0$. This is the key idea underlying the classic "self-screening" and "social value sorting" rationales for ordeals described above.

Panel B shows a first case with adverse selection. The marginal value curve remains downward sloping, but now the marginal cost curve is also downward sloping, capturing the positive value-cost correlation. We show a case where the $MC(\sigma)$ curve rotates around its intersection point with $MV(\sigma)$, so the two curves continue to intersect. Because of this rotation, the gains from targeting (as shown in the green shaded area) are substantially reduced (when ρ is modest) and may be negative (when ρ is large). The key question for targeting efficacy is no longer whether the marginal value curve is downward sloping but whether it is *steeper* than marginal costs. In the case illustrated by the dashed red curve – where $MC(\sigma)$ is steeper than $MV(\sigma)$ – the ordeal leads to "*backwards sorting*." In this case, the ordeal targets inversely from what is desirable: those who are enrolled have negative surplus, while those who are excluded have positive surplus. This type of backward sorting is closely related to the idea that price-based sorting may also be inefficient in insurance markets (Marone and Sabety, 2022).[20]

Panel C shows how adverse selection may lead to optimal universality. We show both the no selection and "modest" adverse selection $MC(\sigma)$ curves from panels A-B but now consider what happens if the $MV(\sigma)$ is higher — e.g., because society places a higher welfare weight (μ) on program enrollees. With no selection, a more modest but still positive ordeal is optimal because the marginal value and cost curves continue to intersect. But with adverse selection, the MV curve lies *entirely above* MC, implying that full enrollment (zero ordeal) is optimal. The same idea applies in reverse if the marginal value curve is lower (via a lower μ), with adverse selection making it more likely that no enrollment is optimal (see Appendix Figure A.1). Intuitively, adverse selection makes these "universal" optima more likely because the similar downward slope of MV and MC makes them less likely to intersect within a given range.

Mathematical Analysis We now formalize these arguments. We start with the claim that adverse selection reduces or reverses the gains from targeting – the sorting argument shown in Figure 1B. Note that given estimates of V_i^{Soc} and C_i^{Net}, we can quantify the value-cost relationship by considering the linear projection of enrollee costs onto value: $C_i^{Net} = \overline{C} + \hat{\beta} * V_i^{Soc} + \omega_i$, where $\overline{C}$ is the mean of net costs and ω_i is a residual capturing cost heterogeneity orthogonal to value. This projection can always be performed, and results in the standard regression coefficient $\hat{\beta} = \rho \cdot \frac{\sigma_C}{\sigma_V}$, where σ_C and σ_V are the standard deviations of cost and value, and $\rho \in [-1, 1]$ is the value-cost correlation. Applying

[20] Sorting may be improved if ordeals (or prices) can be targeted only at high-cost enrollees (Bundorf, Levin and Mahoney, 2012), but this is typically not done because it would be inequitable to the sick. In a different context, the fact that "prior authorization" hassles are targeted at high-cost prescription drugs may explain why these yield savings in excess of their costs (Brot-Goldberg et al., 2022).

this projection to the terms for targeting efficacy in (10) yields:[21]

$$\underbrace{\overline{\gamma}_1 - \overline{\gamma}_0}_{\text{Targeting Efficacy}} = \underbrace{\left(\overline{V}_1^{Soc} - \overline{V}_0^{Soc}\right)}_{\text{Social Value sorting}} \times \underbrace{\left[1 - \overbrace{(\rho \cdot \sigma_C/\sigma_V)}^{\text{Adverse Selection Tax } (\hat{\beta})} - \widetilde{\Delta\omega}\right]}_{\text{Correction for value-cost correlation}}, \quad (11)$$

where $\widetilde{\Delta\omega} \equiv \left(\frac{\overline{\omega}_1 - \overline{\omega}_0}{\overline{V}_1^{Soc} - \overline{V}_0^{Soc}}\right)$ captures the ordeal's sorting on idiosyncratic costs. We call $\hat{\beta}$ the "adverse selection tax," since it captures the degree to which adverse selection (a large covariance between value and costs) "taxes away" the welfare gains from favorable sorting on value.

Equation (11) formalizes the relationship between the classic "self-screening" and favorable value sorting rationales for ordeals, which suggests $\overline{V}_1^{Soc} - \overline{V}_0^{Soc} > 0$, and the true targeting efficacy, $\overline{\gamma}_1 - \overline{\gamma}_0$. If program costs are either constant across enrollees ($\sigma_C = 0$) or uncorrelated with enrollee value ($\rho = 0$), social welfare gains are approximately equal to value sorting. However, as cost heterogeneity (σ_C) and the value-cost correlation (ρ) grow more positive – precisely the two key features of adverse selection laid out above – the adverse selection tax grows, and gains from targeting are diminished. Further, if $\hat{\beta}$ grows large enough that

$$\hat{\beta} = \rho \cdot \frac{\sigma_C}{\sigma_V} > 1 - \widetilde{\Delta\omega}, \quad (12)$$

the correction term becomes negative, and the ordeal leads to backward sorting (on social welfare) despite favorable sorting on value. This corresponds to a "steeper" marginal cost than marginal value curve in Figure 1B. If $\widetilde{\Delta\omega} \geq 0$ – which occurs if an ordeal does not screen, or screens unfavorably, on idiosyncratic costs (the case we usually find in our empirical work) – a sufficient condition for backward sorting is $\hat{\beta} > 1$, or $\rho > \sigma_V/\sigma_C$.

This analysis provides insight into why ordeals will generally work poorly in settings with strong adverse selection, where $\hat{\beta} > 1$. In these settings, *any* ordeal that sorts favorably on value will sort *backwards* on efficiency, unless it happens to screen in people with low *idiosyncratic* costs ($\widetilde{\Delta\omega} < 0$) – something that while possible, is not implied by economic theory. More generally, even modest adverse selection ($\hat{\beta} \in (0, 1]$, or $\rho \in (0, \frac{\sigma_V}{\sigma_C}]$) "taxes" away the gains from value sorting in proportion to $\hat{\beta}$, making the real welfare gains much smaller.[22]

We now formalize the claim that adverse selection makes optimal universality more likely, as depicted in Figure 1C. As in the figure, we consider how shifts in marginal social value driven by a higher/lower social welfare weight (μ) affect the optimality of a given ordeal with strength σ.[23] For the ordeal to yield targeting gains per condition (7), it must be the case that $\overline{\gamma}_1(\sigma) > 0 > \overline{\gamma}_0(\sigma)$, or

[21]We get this from applying the projection to get $\overline{C}_1^{Net} - \overline{C}_0^{Net} = \hat{\beta} * \left(\overline{V}_1^{Soc} - \overline{V}_0^{Soc}\right) + (\overline{\omega}_1 - \overline{\omega}_0)$, which can be rearranged to yield the expression in (11).

[22]One reason $\hat{\beta}$ is likely to be large in low-income populations is that σ_V (at least for private WTP) tends to be small because marginal utility of consumption is high, while σ_C is much larger reflecting variation in health needs.

[23]We make this argument for a particular σ, but an analogous argument applies across a *full range* of values of σ to show that adverse selection makes it more likely that the $MV(\sigma)$ and $MC(\sigma)$ curves do not intersect over this range.

$\overline{V}_1^{Soc}(\sigma;\mu) - \overline{C}_1^{Net}(\sigma) > 0 > \overline{V}_0^{Soc}(\sigma;\mu) - \overline{C}_0^{Net}(\sigma)$, where we highlight that $\overline{V}_1^{Soc}$ and $\overline{V}_0^{Soc}$ are both (increasing) functions of μ. These inequalities, therefore, implicitly define a range of μ over which the ordeal is desirable: $\mu \in [\mu^*_{min}, \mu^*_{max}] \equiv \left[\frac{\overline{C}_1^{Net} - \overline{E}_1}{\overline{W}_1}, \frac{\overline{C}_0^{Net} - \overline{E}_0}{\overline{W}_0}\right]$ as long as $\mu^*_{min} \leq \mu^*_{max}$. Relative to no selection ($\overline{C}_1^{Net} = \overline{C}_0^{Net}$), adverse selection rotates the cost curve, making $\overline{C}_1^{Net} > \overline{C}_0^{Net}$, which pushes upward μ^*_{min} and downward μ^*_{max}. Thus, adverse selection *narrows the range* of social preferences $[\mu^*_{min}, \mu^*_{max}]$ over which ordeals are preferred to universal policies. (See Appendix Figure A.1 for a visualization of this argument.) Further, for sufficiently strong adverse selection, this range becomes null, implying that there is no μ at which the ordeal is optimal.

Broader Implications for Transfer Programs While our emphasis has been on insurance programs, our framework also sheds light on many *transfer* programs where recipient value and public costs are naturally correlated via the (varying) *benefit amounts*, which are both a benefit to enrollees and a cost to the government. For instance, in many means-tested programs, benefit amounts vary with enrollee income or family status. This suggests that the logic of correlated value and costs may apply, and self-targeting may not translate into significant welfare gains. Instead, the desirability of ordeals may depend on whether low-benefit-amount enrollees also tend to be those the government wishes to screen out for other reasons (e.g., because they are less poor, so have a lower social welfare weight).

Our analysis can help interpret the findings in past work. For instance, both Finkelstein and Notowidigdo (2019) (studying SNAP) and Bhargava and Manoli (2015) (studying the EITC) find that hassles on average screen out people who receive smaller benefit amounts from these programs. But the normative implications are different. In SNAP, low-benefit types are generally *higher-income* individuals, for whom economic need is less. But in the EITC, low-benefit types were generally *lower-income* individuals *without kids*, for whom need may be high. By contrast, ordeals screening works well in programs that distribute supplies with *equal costs* for all participants, as in free chlorine solution for water treatment (Dupas et al., 2016).

Connection to Economics of Nudges Our analysis of ordeals relates to the broader economics of "nudges" (Thaler and Sunstein, 2008) and similar non-price interventions. Although the vast majority of this literature focuses on empirical impacts and positive economics, recent work by Allcott et al. (2022) unpacks the welfare implications of nudges. Their work emphasizes that simple *average treatment effects* on demand or adoption of ostensibly beneficial goods or behaviors may be a misleading guide to welfare. Instead, the key welfare question is whether a nudge reduces *choice distortions*, by inducing people to consume or behave more in line with what is socially optimal.[24] A nudge improves social welfare only if it reduces (more than it exacerbates) baseline under- and over-consumption of a good relative to the social optimum.

[24] Allcott et al. (2022) show that this occurs when a nudge reduces the *variance* of "net distortions" – or the (individual-specific) wedge between choice utility and social welfare arising from behavioral biases, externalities, and other factors like markups and taxes. These wedges may be either positive or negative, so a smaller variance implies behavior more in line with social welfare.

This aligns closely with our analysis of take-up and targeting with ordeals for social programs. An ordeal improves welfare only if it corrects (more than it exacerbates) errors of over-enrollment (enrolling $\gamma_i < 0$ types) and of under-enrollment (excluding $\gamma_i > 0$ types) that occur with alternate policies like full inclusion and exclusion. This is exactly what is captured by our targeting efficacy statistic, $\Delta\gamma = \overline{\gamma}_1 - \overline{\gamma}_0$, and by our expression for "gains from targeting" in (6). Indeed, there is a close parallel between our model and the setup of Allcott et al. (2022),[25] suggesting a deep connection between the welfare economics of nudges and ordeals. This also suggests that thinking about nudges through the lens of *optimal targeting* may be a fruitful way to understand their welfare impacts.

3 Setting, Auto-Enrollment Policy, and Data

3.1 Massachusetts Exchange Setting

CommCare Exchange We study Commonwealth Care ("CommCare"), a subsidized insurance exchange in Massachusetts that operated from 2006-2013 before shifting form in 2014 at the ACA's implementation. CommCare covered low-income adults with family income below 300% of the federal poverty level (FPL, or "poverty") and without access to insurance from another source, including an employer or public program (i.e., Medicare or Medicaid). We focus on the population with income below 100% of FPL for whom the auto-enrollment policy applied. Given eligibility rules for other programs, this group is almost entirely childless adults age 19-64.[26]

CommCare offered generous insurance at heavily subsidized premiums. The program specified a detailed benefit structure (i.e., cost sharing rules and covered medical services) that private insurers were required to follows. Each insurer offered a single plan with the standardized benefits but could differ in its network of hospitals and doctors. For the below-poverty group we focus on, benefits were equivalent to Medicaid – i.e., broad covered services with essentially no patient cost sharing (the actuarial value is 99.5%) – and all plans were fully subsidized ($0 premium). This setup is similar to Medicaid managed care programs. As in Medicaid, there is no financial cost to insurance, and the only barriers are enrollment hassles. An important difference from Medicaid, however, is that CommCare does *not* have retroactive coverage; coverage starts the the first day of the month *after* completing enrollment.[27] Therefore, enrollment delays have a meaningful impact, including the risk of getting acutely ill and incurring medical debts before enrollment takes effect.

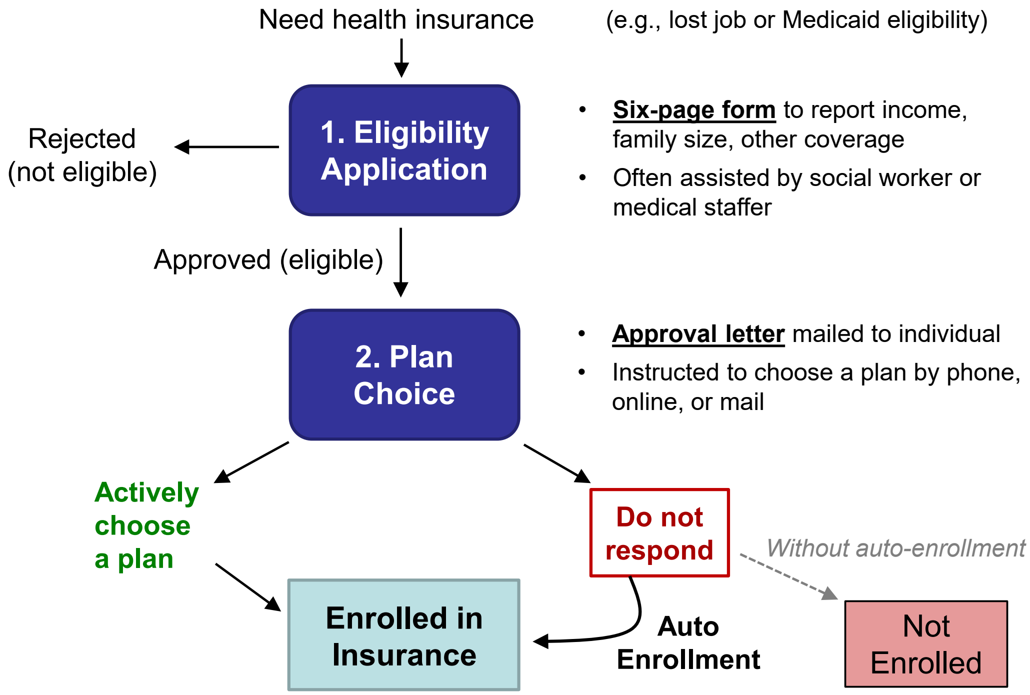
Application and Enrollment Process It is well known that there is substantial "churn" into and out of eligibility for different forms of health insurance – e.g., due to job changes, income fluctuation,

[25]Importantly, we allow C_i^{Net} to vary (whereas marginal cost is fixed in their model) because we are studying a selection market. Finally, their model is more complex because it allows prices to endogenously adjust to nudges (via their impact on supply/demand), which necessitates an analysis of price pass-through impacts that we can ignore.

[26]Medicare covers seniors age 65+, and Massachusetts Medicaid covers children up to 300% of FPL, parents with dependent children up to 133% of FPL, and pregnant women up to 200% of FPL. In addition to the non-elderly, CommCare covered a small number of immigrants age 65+ not eligible for Medicare. As we discuss below, we drop immigrant enrollees from our sample.

[27]By contrast, Medicaid covers medical bills incurred prior to enrollment, typically with a 90-day retroactive period. As a result, Medicaid eligibles have a form of "conditional coverage" that is not available from CommCare.

Figure 2: Enrollment Process and Auto-Enrollment Policy



Note: The figure diagrams the enrollment process for the Massachusetts health insurance exchange we study (CommCare). Prospective enrollees who need health insurance must follow a two-step process. First, they apply for eligibility, completing a six-page form with information on income, family status, and other coverage. Second, if approved, they are mailed an approval letter and asked to choose a (free) health plan by phone, online, or mail. The auto-enrollment policy applies to approved individuals who do not respond to this approval letter within 14 days ("passive" individuals). With auto-enrollment (the policy from 2007-09), they are auto-enrolled into a state-selected plan; without auto-enrollment (post-2010 policy), they are not enrolled unless and until they actively respond.

or family status changes. Therefore, many people newly need health insurance and apply for public coverage. For CommCare, the enrollment process involves two steps, as shown in Figure 2. Step one is to apply for eligibility. This requires completing a six-page application that asks about income, demographics, family status, and access to other health insurance (see Appendix H for snapshots of the form). The state used this information to determine eligibility for Medicaid or CommCare (dual eligibility should not occur), and to sort people into income-based subsidy groups in CommCare. Although the application form is a meaningful hassle, many individuals get help from a social worker or medical staffer in completing it, often just after having visited a medical provider while uninsured.

The second enrollment step is to choose a plan. After determining eligibility, the state notified an individual (by mail and/or email) and provided information on available plans and associated premiums. Appendix H shows this two-page approval letter. To complete enrollment, individuals were asked to choose a plan by calling, going online, or circling a plan choice and returning it by mail. Relative to the initial application, this step was quite simple. However, without auto-enrollment, individuals still had to take action to enroll. Moreover, the action needed to be taken *independently* in response to the approval letter, which could be lost, misunderstood, or forgotten.

3.2 Auto-Enrollment Policy and Timeline

Auto-Enrollment Policy CommCare's auto-enrollment policy set the default outcome for people determined eligible (step #1 of the process) but who did not respond when asked to choose a plan (step #2; see Figure 2). The policy applied only to below-poverty enrollees, for whom all plans were free.[28] This allowed regulators to borrow a policy widely used in Medicaid managed care that "auto-assigns" passive new enrollees into a state-selected plan. Aggregate statistics suggest that auto-assignment in Medicaid is very common: the median state auto-assigns 45% of new enrollees (Kaiser Family Foundation, 2015). However, we are not aware of any *causal* evidence on this policy's impact on take-up, likely because of a lack of variation in its use.

Auto-enrollment applied when individuals entered the market, but with different rules for two groups: (1) "new enrollees" joining for the first time, and (2) "re-enrollees" joining after a gap in coverage. We focus our main analysis on new enrollees. New individuals were mailed a coverage approval letter and given 14 days to actively choose a plan before being auto-enrolled if they failed to respond. This lets us observe mode of enrollment (active vs. passive) directly in our administrative data.[29]

There was one notable exception to the process for new enrollees near CommCare's inception in 2007 when the state "auto-converted" a large population from its pre-RomneyCare uncompensated care pool (UCP). These individuals did not complete a new eligibility application but were determined eligible based on information from their original UCP application, often completed months beforehand. Consistent with the long lag, many of these UCP individuals failed to respond and were auto-enrolled, creating a large spike in auto-enrollment in early 2007. Because of these distinct circumstances, we focus our main analysis on the "steady-state" auto-enrollment period (fiscal years 2008-09), with the initial period (2007) analyzed for comparison and robustness.[30]

Policy Timeline We examine auto-enrollment policy changes during FY 2010 (which ran from July 2009 to June 2010). Facing a Great Recession-related budget shortfall, CommCare needed to cut spending. The program had raised enrollee premiums and copays the prior year, and it was eager to avoid doing so again. Suspending auto-enrollment provided an alternative to reduce enrollment and therefore subsidy spending. The exchange did so as of the start of fiscal 2010, with (because of a lagged impact) a final group of passive enrollees joining in 2010m1 (July 2009). These cuts proved

[28] Auto-enrollment was generally not used for above-poverty enrollees because premiums varied across plans and were typically non-zero, raising concerns about auto-enrolling people into plans that generated a financial debt for them. There were two limited exceptions of auto-enrollment for 100-150% of poverty enrollees, both of which are excluded from our main sample (see discussion below): (1) for re-enrollees prior to 2010 who re-enrolled with a gap of less than 12 months, and (2) for new enrollees during the single month of Dec. 2007 (FY 2008m6).

[29] By contrast, most re-enrollees were *immediately* auto-enrolled in their former plan (without a 14-day window to actively choose), and auto-re-enrollment was also used for some above-poverty enrollees (our control group). For these reasons, we exclude re-enrollees from our main sample, reporting effects on them in robustness analysis (see Appendix B.2).

[30] Appendix C.5 compares our main targeting analysis for the 2008-09 sample (see Section 5.1) to the results for 2007. Interestingly, while auto-enrollment is much more common in early 2007, we find very similar targeting (active vs. passive enrollee characteristics) in both periods.

quite effective, and CommCare unexpectedly came in under budget during 2010. As a result, the program temporarily reinstated auto-enrollment in the final three months of FY 2010. After this, facing continued budget pressures, it was permanently canceled in 2011.

These changes give us variation to estimate the causal impact of auto-enrollment. To be valid, it is important that there not be other concurrent shocks or policy changes that affect enrollment around the same time. Based on background research and discussions with the exchange administrator, this appears to be true, with one exception: an eligibility cut for non-citizen enrollees in 2010m4 (October 2009), two months after the auto-enrollment suspension. To avoid biasing our results, we exclude non-citizen enrollees from our sample in all periods.[31] Aside from this, other enrollment-relevant policies did not change.[32] Nonetheless, to address any unobserved demand shocks, we also use a control group of higher-income enrollees not subject to auto-enrollment.

Other Policy Details Although our analysis focuses on enrollment impacts, other policy details are of interest, including rules for plan auto-assignment. The plan assignment rule had two parts. Passive enrollees with prior enrollment with an insurer in the past 12 months (either in CommCare or Medicaid) were auto-assigned to that insurer. Other new enrollees were randomly assigned to plans, with probability shares following a schedule giving more weight to plans with lower (state-paid) premiums. After enrollment, all new/re-enrollees (both active and passive) could freely switch plans within 60 days of starting coverage. In practice, the vast majority (96% of passive and 98% of active enrollees) stick with their initial plan, consistent with other work finding that default health plan assignment is very sticky (Brot-Goldberg et al., 2021).

These policies raise two interesting issues that we have not explored in this paper. First, random assignment could allow for inferring causal plan effects, as in recent work on Medicaid (Geruso, Layton and Wallace, 2020). In practice, we find evidence of slight demographic imbalance across plans, suggesting the presence of hard-to-observe exceptions to random assignment. We therefore have not pursued this topic further. Second, giving higher probability weights to lower-price insurers should affect competitive incentives. This topic is interesting but would require a different research design to study; we therefore leave it for future work.

3.3 Data and Descriptive Statistics

Exchange Admin Data and Sample Definition Our primary data come from de-identified CommCare administrative records for fiscal years 2007-2014, spanning November 2006 to December 2013 (Massachusetts Health Connector, 2014). For all enrollees, we observe a panel of individual-level

[31] The eligibility change was for legal immigrant residents (typically green card holders) who had not yet cleared their "five-year bar" requirement to receive federal Medicaid matching funds – a group the state calls "aliens with special status" (AWSS). Starting in October 2009, the AWSS group was not eligible to newly enroll in CommCare, and existing AWSS enrollees were shifted into a parallel program. We observe a flag for AWSS status and enrollment in this parallel program, which lets us exclude these individuals from the sample in all periods.

[32] The start of 2010 did see the entry of a new insurer (CeltiCare). But for the below-poverty group, this expanded the choice set of available free plans, which should (if anything) increase enrollment, pushing in the opposite direction of our findings. In practice, CeltiCare had a narrow network and was not popular, with only 1.5% of below-poverty active choosers selecting it during 2010-11. We therefore view the new availability of CeltiCare as having a negligible impact.

demographics and monthly plan enrollment, linked to insurance claims and risk scores. Observed demographics include age, gender, zip code of residence, and family income as a percentage of the poverty line. Insurance claims let us measure individuals' medical conditions and health care use and costs while enrolled. Importantly, the data include a flag for whether each new enrollee is auto-enrolled or actively chooses a plan. This lets us construct the key variables for our main analysis: monthly counts, characteristics, and outcomes for passive and active enrollees.[33]

We are interested in the policy's impact on enrollment totals and composition. For enrollment impacts, the main outcome of interest is counts of new enrollees joining CommCare per month (a flow measure). We use our panel data and a simple model to translate this into an effect on steady state enrollment (a stock measure). For composition, we use variables on demographics, diagnoses, and medical spending during an individual's enrollment spell.

We make several limitations to our main CommCare analysis sample. First, we limit attention to new enrollees who (when they joined the market) were in one of two income groups: (1) the 0-100% of poverty "treatment" group, and (2) a 100-200% of poverty "control" group not subject to auto-enrollment. Second, we exclude from our sample non-citizen enrollees who (as described above) faced an eligibility cutback in October 2009, shortly after the auto-enrollment change (in August 2009). Finally, we limit our main sample period to FY 2008-2011 for analyses of the treatment group and to 2009-2011 for difference-in-differences (DD) regressions comparing treatment and control groups. We exclude 2007 because of the different nature of auto-enrollment during that year (see discussion above). For DD regressions, we further exclude 2008 because of other policy changes that affected the control group in mid-to-late 2008.[34] We end our analysis in 2011 because of a change in plan choice rules for the treatment group at the start of 2012 (see Shepard, 2022).

Other Datasets We draw on two additional datasets for specific pieces of our analysis:

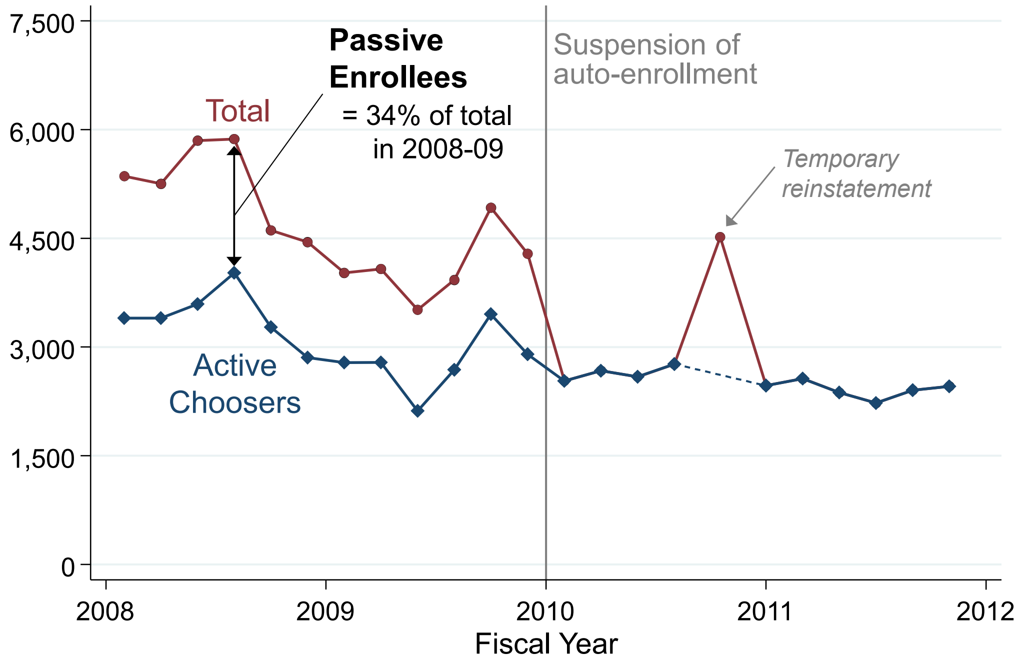
(1) American Community Survey (ACS): For context on uninsurance in Massachusetts, we use the ACS to estimate the CommCare-eligible uninsured population by income group, following a method used by Finkelstein et al. (2019b). Details are in Appendix A.1.

(2) Massachusetts All-Payer Claims Database (APCD): We use the state's APCD (version 3.0, with data for 2009-13) (Mass. CHIA, 2014) to examine whether CommCare enrollees are enrolled in duplicate private insurance, as a possible reason for failing to actively enroll. The APCD is well suited for this purpose because it lets us observe a near-universe of Massachusetts health insurance plans and measure simultaneous coverage. Appendix D describes the data construction method and shows that the APCD's enrollment counts for CommCare closely match our administrative data.

[33] We observe this flag for the FY 2007-2009 period when auto-enrollment is in effect, but due to a technical issue, it is missing during the policy's temporary reinstatement in April-June 2010. For this latter period, we report only aggregate data for all enrollees.

[34] Specifically, for individuals above 150% of poverty, the state's insurance mandate penalty took effect in December 2007 (FY 2008m6), leading to a spike in new enrollment. Also in Dec. 2007, there was a large auto-enrollment for the 100-150% poverty group. For the whole 100-200% poverty control group, there was a change in plan premiums and subsidies at the start of FY 2009 (July 2008). Importantly, none of these changes applied to the treatment group, and policy for the control group was stable throughout the 2009-11 period used in our DD analysis.

Figure 3: Active vs. Passive New Enrollment into the Massachusetts Exchange



Note: The graph shows counts of new enrollees per month for the below-poverty group subject to auto-enrollment. The red series is total new enrollment; the blue is active choosers; and the gap between these is passive auto-enrollment. The vertical line indicates the timing of auto-enrollment's suspension at the start of FY 2010. After this total enrollment equals active choosers, except for the period of auto-enrollment's temporary reinstatement (during which we lack the flag to separate active vs. passive enrollment). Data are bimonthly averages to smooth over fluctuations.

Descriptive Statistics Figure 3 shows data on new enrollment per month in the treatment group (0-100% of poverty) over the main 2008-2011 period.[35] The figure plots both total new enrollment (in red) and the count of active choosers (in blue), with the gap between these being passive enrollees. Passive enrollees represent a sizable 34% share of new enrollment during 2008-09, and new enrollment falls sharply when auto-enrollment was suspended at the start of 2010. The decline is almost identical to the number of passive enrollees during 2008-09. Moreover, when the policy is briefly reinstated at the end of 2010, enrollment spikes up to a similar level as at the end of 2009. Together these facts are consistent with auto-enrollment having a causal effect roughly equal to the full number of passive enrollees in the pre-period.

Appendix Table A.1 further summarizes enrollment statistics, including enrollment counts for the 100-200% of poverty group and on total market enrollment and new- vs. re-enrollment. Appendix Table A.2 reports average consumer attributes; we defer a discussion of these to Section 5 where we compare active vs. passive enrollees.

[35]The points are bimonthly averages to smooth over noise; see Appendix Figure A.2 for the raw monthly data over the full 2007-11 period. As that figure shows, auto-enrollment spiked during early 2007 because of the auto-conversion of the state's uncompensated care pool.

4 Causal Impact of Auto-Enrollment Policy

This section presents our estimates of the impact on take-up of suspending auto-enrollment in 2010. After presenting results in Section 4.1, we provide context on the magnitude in Section 4.2.

4.1 Impact on Health Insurance Enrollment

We use the 2010 policy change to estimate the causal impact of auto-enrollment. To do so, we run difference-in-difference (DD) regressions on counts of monthly new enrollment, comparing the 0-100% of poverty "treatment" group (for whom auto-enrollment is in place through 2009 and suspended in 2010) to the 100-200% of poverty "control" group (for whom auto-enrollment was not in place throughout). The DD regression is:

$$NewEnr_{g,t} = \alpha_g + \beta_t + \gamma \cdot 1\{g = Treat, t \geq 2010\} + \varepsilon_{g,t} \tag{13}$$

where $NewEnr_{g,t}$ is (scaled) new enrollment for income group g (treatment or control) at time t, α_g is a group fixed effect (for the treatment and control groups), β_t is a time fixed effect, and $\varepsilon_{g,t}$ is an error. We run (13) on data from 2009-2011, excluding the period of temporary reinstatement of auto-enrollment at the end of 2010.[36] The dependent variable is "scaled" new enrollment – equal to a group's raw monthly counts divided by its average new enrollment in the pre-2010 period. This ensuring $NewEnr_{g,t}$ has a mean of 1.0 for each g in the pre-period, and lets us interpret estimates as proportional effects. The coefficient of interest is γ, which is the DD estimate of the impact of turning off auto-enrollment (i.e., adding the active choice ordeal).

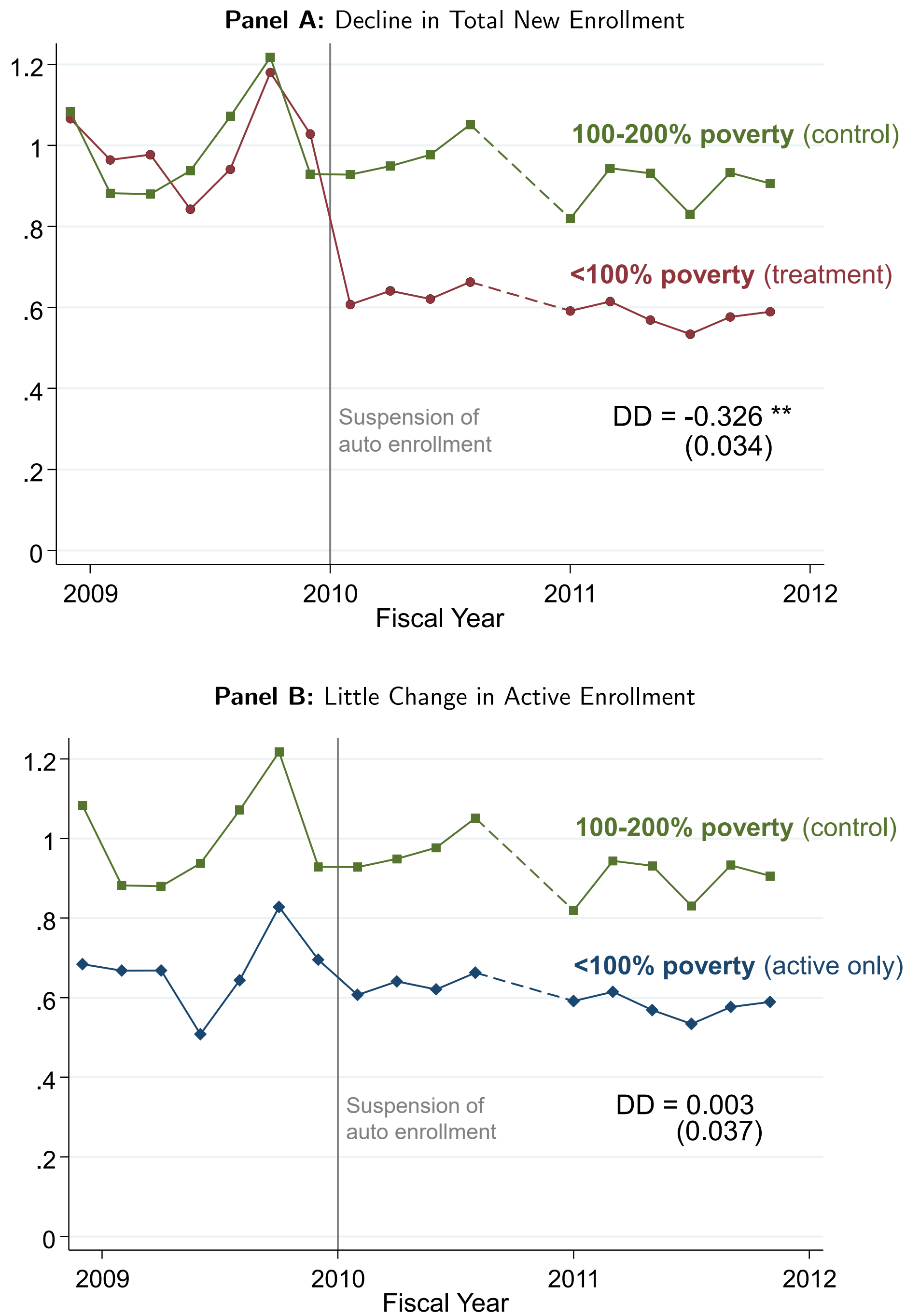
Figure 4 plots the data for the regression in (13) and reports the main DD estimate. Panel A shows results for *total* new enrollment (active plus passive). Trends for both groups are parallel in the pre-period, and treatment group enrollment drops sharply and persistently at the policy change. The DD estimate of $\gamma = -0.326$ implies that suspending auto-enrollment reduced new enrollment by 32.6% of the pre-period mean. In the reverse direction, new enrollment was 48% (= 0.326/(1-0.326)) higher when auto-enrollment was in place.

Figure 4B shows the impact on the number of *actively choosing* new enrollees. In principle, auto-enrollment might induce some attentive individuals to be "purposely passive" because they know the stakes are low – e.g., if they view CommCare plans as roughly equivalent and are happy to let the regulator select for them.[37] If this were true, we would expect these purposely passive individuals to actively enroll when auto-enrollment stops in 2010, resulting in an uptick in *active* enrollment. Instead, Figure 4B shows that there was no change in active new enrollment around the policy change, with a DD estimate of almost exactly zero ($\gamma = 0.003$) and no sign of an uptick in the two years

[36] The time unit (t) is bimonthly periods, averaging over new enrollment in pairs of months, which smooths over a few single months when auto-enrollment appears not to have occurred followed by a surge in auto-enrollment the next month. We calculate standard errors using the normal linear model given the small samples sizes but verify that robust standard errors are essentially the same.

[37] Enrollees were informed about the auto-enrollment policy in the coverage approval letter, which stated: *"If you do not choose a health plan by [date], the Connector will choose one for you."* After early 2010, this language was removed, and enrollees were sent periodic reminder letters if they had qualified but not enrolled in coverage.

Figure 4: Enrollment Impact of Auto-Enrollment's Suspension



Note: The figure shows scaled new enrollment per month into CommCare and estimates of the DD specification (13) for estimating the causal effect of auto-enrollment's suspension. Each panel compares trends for below-poverty enrollees (the treatment group) versus 100-200% of poverty enrollees (the control group, not auto enrolled). Each income group's series is rescaled by dividing by the group's pre-period mean new enrollment, which makes DD estimates interpretable as a proportional change. The temporary reinstatement period is excluded (as indicated with dashed lines). Panel A shows that *total* new enrollment falls sharply (by 32.6%) for the treatment group at the start of 2010, consistent with a causal effect of the policy. Panel B shows that the number of *active* new enrollees is flat through the policy change.

following the policy change. As a further test, Appendix Figure A.3 shows that we see no evidence of compositional changes in the characteristics of active enrollees, which we would expect if some people shifted to active choice.

This evidence suggests two facts about the ordeal of requiring active plan choice to get insurance. First, failure to actively enroll is unlikely to have been a strategic or purposeful decision; instead, passivity is more likely due to inattention or misunderstanding of enrollment rules. Second, active choice is unlikely to involve significant costs to inframarginal enrollees. If it did, we would expect some to substitute towards passivity when auto-enrollment is an option.

Effect on Steady-State Enrollment The results so far are on the *flow* of new enrollees, which falls immediately when auto-enrollment ends. The *stock* of total enrollment, however, changes more gradually, as existing enrollees exit while fewer new enrollees enter each month. To estimate the impact on steady-state enrollment, Appendix B.3 uses the data to calibrate a simple stock-flow model. We find that suspending auto-enrollment reduces steady-state enrollment by 24% – or in the reverse direction, enrollment is 32% higher with auto-enrollment in place. (This estimate is slightly smaller than the impact on new enrollment because passive enrollees have shorter durations.) The estimates from the stock-flow model are highly consistent with the raw data on the stock of below-poverty enrollment, which falls by 23% from late 2009 to the end of 2011 (Appendix Figure A.7).

Robustness: Alternate Specifications and Effects on Re-Enrollment These estimates are quite robust to alternate specifications and control groups. Appendix Table A.3 shows that the estimated 33% fall in new enrollment is little changed when we: (1) use alternate income groups as controls (e.g., 100-150% FPL only, or 100-300% FPL), (2) use no control group (a simple pre/post difference), and (3) include the "temporary reinstatement" period in the regressions. Additionally, while the analysis so far has been limited to new enrollees, Appendix B.2 shows that there are similar impacts on the number of re-enrollees joining the exchange after a break in coverage. We find that re-enrollment falls 35-39% at the start of 2010, very similar to the 32.6% fall for new enrollment. We therefore conclude that our main estimates on new enrollees are representative of the policy's overall impact.

4.2 Magnitude: Comparison to Other Take-up Policies

How should we interpret the magnitude of the impact of auto-enrollment – a 48% increase in new enrollment and 32% increase in steady state? Several benchmarks provide context for this estimate. First, relative to other "nudge" interventions to increase health insurance take-up, these are very large impacts. Several recent randomized experiments have tested nudges like reminder mailings/phone calls, simplified plan information, and a simpler take-up process (Domurat et al., 2021; Myerson et al., 2021; Ericson et al., 2023). These studies find take-up impacts of 1-4 percentage points among a similar passive population (people who have qualified for coverage but not chosen a plan).[38] Similarly,

[38] Goldin, Lurie and McCubbin (2021) study a similar mail outreach intervention on uninsured individuals identified in tax filings. They likewise find a modest take-up impact of +1.1 percentage points, though even this small impact led to a meaningful decline in mortality among the marginally insured.

evidence from Aizawa and Kim (2020) suggests that a three-fold increase in government advertising of ACA marketplaces would increase market-level enrollment by 1.3 percentage points (or 7.6%). By contrast, our auto-enrollment policy leads to an *order of magnitude larger* impact: nearly complete take-up among the passive group and a 30-50% increase in the total enrolled population. These results suggest that while information and simplification matters, *making enrollment the default* may be critical to substantially boost take-up.

A second benchmark is the impact of financial incentives. Our estimated steady-state impact of auto-enrollment is nearly identical to the 33% effect of subsidies that reduce enrollees premiums by $39-40 per month, or $468-480 per year (a 57% average reduction), in prior evidence from the Massachusetts exchange (Finkelstein, Hendren and Shepard, 2019b). It is somewhat larger than the 20-26% impact of introducing Massachusetts' uninsurance penalty (Chandra, Gruber and McKnight, 2011).[39] Therefore, auto-enrollment has an impact comparable to sizable changes in financial incentives.

Despite its large impact, the targeted nature of the auto-enrollment policy – applying only to people who had already qualified for coverage – meant that its impact on overall uninsurance was more modest. Using ACS data, we estimate that Massachusetts had about 300,000 uninsured people in 2009, of whom about 62,000 had incomes below poverty and were likely CommCare eligible. Relative to this denominator, auto-enrollment's 14,900-person impact (see Appendix B.3) represents a 24% decline in the eligible uninsured population.

5 Targeting Implications of Auto-Enrollment

In this section, we study the targeting implications of auto-enrollment. Who are the marginal enrollees, and how do they compare to inframarginal (active) enrollees? How does auto-enrollment affect the market risk pool? What mechanisms may explain passive individuals' failure to actively enroll? These questions matter both for the policy's positive economic implications and for its welfare interpretation. Section 5.1 provides descriptive evidence on targeting implications, comparing marginal (passive) vs. inframarginal (active) enrollees on characteristics related to the value and cost of insurance. Section 5.2 shows evidence that auto-enrollment is unlikely to be (invalidly) enrolling individuals with duplicate private health insurance. Section 5.3 assesses mechanisms, both rational and behavioral, for why a small hassle deters so many people from taking up free coverage.

5.1 Targeting Implications and Impact on Market Risk Pool

To study the targeting implications of auto-enrollment – i.e., inferring its marginal vs. inframarginal enrollees – we employ two methods. The first is motivated by our finding in Section 4.1 that the number and composition of active enrollees is unaffected by the end of auto-enrollment in 2010. This suggests that passive behavior is in a sense "exogenous" to the policy environment. If correct, this

[39] Evidence from the ACA – which involves a somewhat higher-income population than in CommCare – suggests smaller impacts of both subsidies and uninsurance penalties (see e.g., Frean et al., 2017; Lurie et al., 2019). The 32% impact of auto-enrollment is even larger relative to subsidies and penalties based on these ACA estimates.

means that *observed passive* enrollees (prior to 2010) are also *marginal* enrollees who would not have enrolled without the policy in place.[40] Thus, we are in the fortunate position of directly observing who is a marginal vs. inframarginal enrollee (something that is rarely true in the targeting literature). A simple comparison of passive vs. active enrollees, therefore, should faithfully characterize marginal vs. inframarginal individuals. We use this method for our main analysis, controlling for entry timing using cohort fixed effects.[41]

Our second method uses the *policy change* to infer marginal enrollee characteristics from compositional changes in new enrollment at the start of 2010. This method has the advantage of not requiring the assumption of exogenous passivity. However, it is statistically much less powerful and may suffer problems if enrollee attributes are trending over time. We therefore implement it as a robustness check, using the simple active vs. passive comparison for our main estimates.

Characteristics of Passive Enrollees Table 1 shows the results from our main method comparing passive vs. active enrollees. Overall, the results suggests four main patterns about passive (relative to active) enrollees:

1. **Younger, healthier, and more male:** Passive enrollees are younger by 3.8 years on average and are 22% more likely to fall into the youngest age 19-34 group. They are also more likely to be male, with an especially large share (44% higher) of young men age 19-34 – a group often called "young invincibles" in insurance discussions. Likewise, passives enrollees are healthier, with 33% lower rates of any chronic illness and 49% lower rates of severe chronic illness. Overall, passive enrollees have 36% lower medical risk scores, a measure of predicted medical costs based on age, sex, and diagnoses.[42] Figure 5 visualizes these patterns in a different way by plotting the passive enrollment rate by age, sex, and risk score groups. Passive rates decline with age and risk, though they exceed 20% even for the oldest and sickest groups.

2. **Lower medical costs:** Consistent with their youth and health, passive enrollees incur 44% lower monthly medical costs ($228 per month vs. $408 for active enrollees), and are more likely to have zero spending. The slightly larger gap for spending (-44%) relative to risk score (-36%) suggests passive enrollees may also be unobservably healthy. Because the government pays insurers using *risk-adjusted* capitation, passive enrollees' lower risk scores imply that the government also incurs lower costs to cover them.[43]

[40] More generally, one could think of passive enrollees as falling into two groups: (1) "always passives," who are passive regardless of the policy, and (2) "conditional passives," who are passive under auto-enrollment but make sure to actively enroll when it is gone. Our evidence in Section 4.1 suggests that there are few if any conditional passives in our setting.

[41] This lets us control for any time trends (e.g., medical cost growth) that could affect results if passive rates vary over time. In practice, these fixed effects have little impact on results. The specific method is as follows. Let $Y_{i,c}$ be a characteristic/outcome for new enrollee i who joins CommCare in entry cohort c (i.e., in a given year-month). We regress $Y_{ic} = \alpha_c + \delta \cdot 1\{Passive_i\} + \varepsilon_{i,c}$, which includes a cohort fixed effect (α_c). Table 1 reports the mean for active enrollees ($\overline{Y}_{active}$), the adjusted mean for passive enrollees ($= \overline{Y}_{active} + \delta$), and the difference between the two (δ).

[42] We use the HHS-HCC risk score (silver-CSR version), as used in the ACA Marketplaces, calculated based on diagnoses observed on claims during an enrollee's first 12 months enrolled.

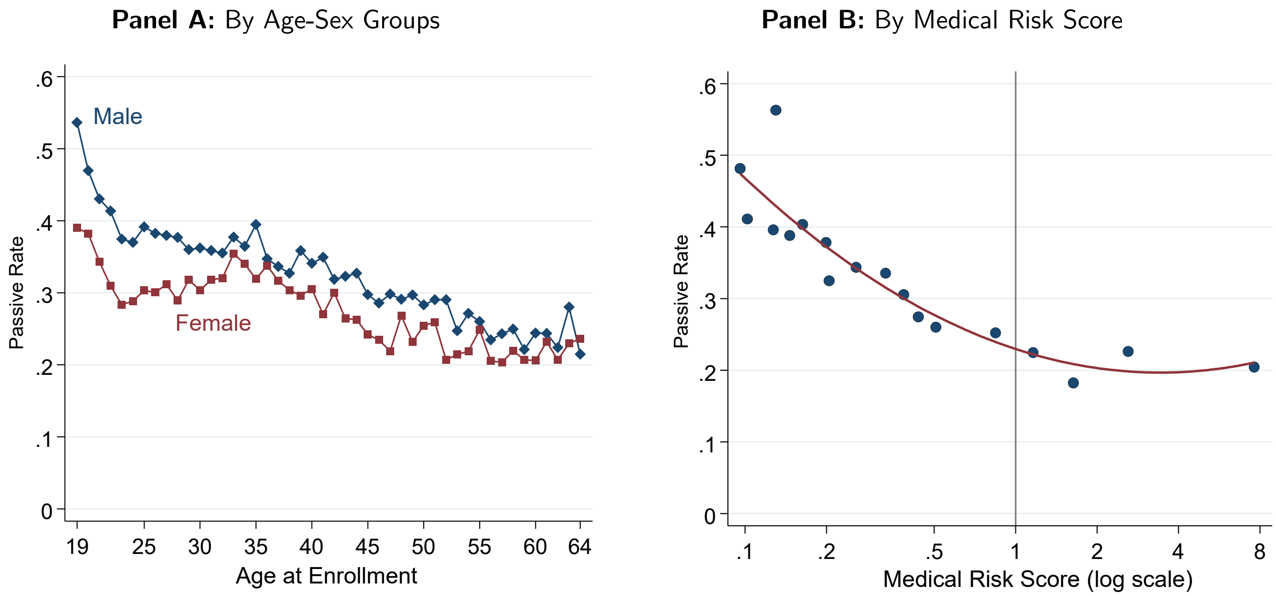
[43] Up to 2009, CommCare used a crude risk adjustment system that varied rates by age-sex-region cells. Under this system (which we can observe), the average government payment for passives was 8% less than for active enrollees ($344

Table 1: Targeting Implications: Comparing Active vs. Passive Enrollees

Variable	Active Enr. (1)	Passive Enr. (2)	Diff. (3)	(S.E.) (4)	[% Diff] (5)
A. Age and Sex					
Average Age (years)	35.6	31.8	-3.8	(0.1)	[-11%]
Age 19-34	0.535	0.652	+0.118	(0.003)	[+22%]
Age 35-54	0.339	0.271	-0.068	(0.003)	[-20%]
Age 55+	0.126	0.077	-0.049	(0.002)	[-39%]
Share Male	0.538	0.625	+0.087	(0.003)	[+16%]
Male Age 19-34	0.286	0.411	+0.125	(0.003)	[+44%]
B. Health Status and Medical Spending					
Any Chronic Illness	0.641	0.427	-0.215	(0.003)	[-33%]
Severe Chronic Illness	0.158	0.081	-0.077	(0.002)	[-49%]
Risk Score (HCC)	1.011	0.644	-0.367	(0.015)	[-36%]
Average Cost ($/month)	$408	$228	-$181	(5.6)	[-44%]
Any Spending (>$0)	0.894	0.709	-0.185	(0.003)	[-21%]
C. Income & Area Disadvantage					
Income / Poverty Line	0.248	0.200	-0.049	(0.004)	[-19%]
High-Disadvantage Area	0.320	0.401	+0.082	(0.003)	[+25%]
Share Black (in zipcode)	0.082	0.106	+0.024	(0.001)	[+29%]
Share Hispanic (in zipcode)	0.137	0.162	+0.025	(0.001)	[+18%]
Near Safety Net Hosp/CHC	0.371	0.458	+0.087	(0.003)	[+23%]
D. Duration Enrolled					
Average (months)	16.5	11.9	-4.6	(0.1)	[-28%]
Share 1-3 months	0.154	0.228	+0.075	(0.002)	[+48%]
Share 12+ months	0.559	0.441	-0.119	(0.003)	[-21%]
Share 16+ months	0.297	0.168	-0.129	(0.003)	[-43%]

Note: The table shows differences in characteristics/outcomes for passive vs. active enrollees in our main sample of below-poverty new CommCare enrollees during FY 2008-09. Estimates control for entry cohort fixed effects and (for all variables except duration in panel D) are weighted averages by months enrolled (capped at 12 months). Health and cost measures are based on claims during the enrollee's first 12 months enrolled. Chronic illnesses follow a classification of ICD-9 diagnosis codes shared with us by David Cutler. Risk score is based on the HHS-HCC model (silver-CSR version) used for risk adjustment in the ACA, re-normalized to have mean 1.0 in the CommCare data. Income refers to family income as a share of the federal poverty level (FPL). High-disadvantage areas are zipcodes (ZCTAs) in the 75th percentile or higher of the social deprivation index (SDI) produced by the Robert Graham Center based on ACS data (see https://www.graham-center.org/rgc/maps-data-tools/sdi/social-deprivation-index.html), which also includes data on zipcode-level shares black and hispanic. Near safety net hospital or Community Health Center (CHC) refers to the share of enrollees living in zipcodes within 2 miles of one of these facilities.

Figure 5: Passive Enrollment Rate by Age, Gender, and Medical Risk



Note: The figure plots variation in the passive enrollment rate – the share of new enrollees who join passively – by age-sex groups (Panel A) and medical risk score bins (Panel B). The data are for our main sample: new enrollees in the relevant below-poverty income group during fiscal years 2008-09. The medical risk score is the HHS-HCC risk score (silver-CSR version) used by the ACA Marketplaces, calculated based on diagnoses observed on claims during the first 12 months of enrollment.

3. **More economically disadvantaged:** Passive enrollees are more disadvantaged across several metrics. Their incomes are slightly lower (20% vs. 25% of poverty). Their differences in neighborhood characteristics (based on zipcode) are larger. Passive enrollees are 25% more likely to live in a zipcode in the top quartile of the Social Deprivation Index, a measure based on Census data.[44] Their zipcodes include a higher share of Black and Hispanic residents.

4. **Shorter durations:** Passive enrollees are enrolled for shorter periods, with average durations 4.6 months (or 28%) shorter. Although we do not observe the reason for these shorter spells, an analysis of the time pattern of exits (see Appendix C.2) suggests a combination of two factors: (1) a higher rate of brief 1-3 month spells, and (2) a higher exit rate during annual eligibility redetermination (12-14 months into the spell). The latter is consistent with a failure to complete redetermination paperwork, another administrative hassle.

A natural question is whether measured risk differences are driven by passive enrollees' shorter durations (see point #4 above), which limits the period over which medical conditions can be observed

vs. $373 per month). Starting in 2010, the program shifted to a stronger diagnosis-based risk adjustment, similar to the HCC risk scores we report. Although we lack full data until 2011 on CommCare's risk adjuster, the 36% lower HCC scores suggest rates would be substantially lower for passives.

[44]We use the Social Deprivation Index (SDI) developed by the Robert Graham Center (see https://www.grahamcenter.org/rgc/maps-data-tools/sdi/social-deprivation-index.html). SDI is an index of area-level deprivation derived from ACS data, based on income, education, housing, employment and other demographics. We define high disadvantage as neighborhoods in the top quartile of the SDI based on the national distribution.

in claims data. In practice, this does not appear to be a major source of bias. Appendix C.1 shows that health differences are robust to using shorter measurement periods (including using just the first month enrolled) and to examining a balanced panel of active and passive enrollees enrolled for the same duration.

In line with their residence in lower-income neighborhoods, passive enrollees are also more likely to live nearby (within 2 miles) a safety net hospital or community health center. This proximity raises the question of whether they use more "uncompensated care" – an important social cost of uninsurance (Finkelstein, Mahoney and Notowidigdo, 2018) that we include in our model in Section 2. Appendix C.3 presents analysis to test this idea. A limitation is that we cannot directly observe care used by active vs. passive individuals when *uninsured*. However, based on care use when insured, passive enrollees obtain a larger share of their care from standard sources of uncompensated care, including emergency rooms and safety net hospitals.

Interpreting the Differences Overall, this evidence is consistent with the two main features of our ordeals targeting framework in Section 2: *self-targeting* and *adverse selection*. Consistent with self-targeting, passive enrollees (those screened out by ordeals) have attributes consistent with lower demand (value) for health insurance. This includes the young and healthy, who on average need less medical care, and shorter-duration enrollees, who may only have a brief need for public coverage (e.g., between jobs). Demand for health insurance also tends to be low among the poor (Finkelstein et al., 2019a,b; Tebaldi, 2020).

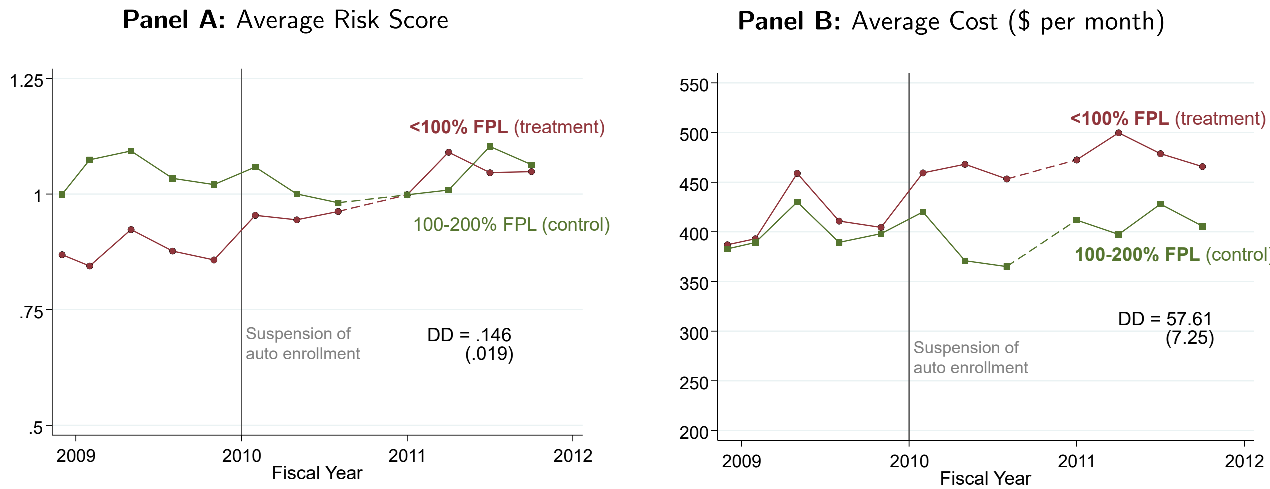
But consistent with adverse selection, these same low-demand individuals also incur much lower costs. Passive enrollees incur 44% lower monthly medical costs – and including their shorter durations, their average per-spell costs are 60% lower. This is natural in an adverse selection market where both value and costs are driven by an enrollee's medical risk (and by their enrolled duration). As a result, our theory suggests that *self-targeting* may not translate into *socially* beneficial targeting. We evaluate this idea more formally using our empirical model in Section 6 below.

Robustness: Inference Using the Policy Change (and Risk Pool Impacts) As a robustness check, we use the 2010 policy change to infer marginal enrollees. Prior to 2010, new enrollees include both active and passive individuals; afterward only active choosers enroll. Marginal enrollees' characteristics, therefore, can be inferred from the *compositional* change at the start of 2010. To implement this, we run DD regressions analogous to equation (13) but with a dependent variable of characteristics/outcomes of new enrollees. Regressions are run on individual-level data, clustering standard errors at the income group-by-month level.

Figure 6 shows the raw data and DD estimates for two key risk pool variables: average risk score (panel A) and average cost (panel B) for new enrollees. There is a clear increase in both measures for the treatment group (red) relative to controls (green) after auto-enrollment is suspended.[45] The

[45]Counterintuitively, prior to 2010 the controls have higher risk scores but similar costs to the treatment group, and this pattern flips in 2010+. This occurs because CommCare provided more generous benefits to the treatment group, including dental care and slightly lower copays, which results in higher costs partly through a moral hazard effect (see

Figure 6: Effect of Auto-Enrollment Suspension on Enrollee Risk Pool



Note: The figure shows data on average risk score (Panel A) and monthly medical costs (Panel B) for new enrollees, and estimates of the DD specification (13) using quarterly time periods. Each panel shows trends for below-poverty enrollees (the treatment group) versus 100-200% of poverty enrollees (the control group). The temporary reinstatement period is excluded (as indicated with dashed lines). When auto-enrollment is suspended, average risk score rose by 14.6% of the market average (which is 1.0), and average medical costs rose by $57.61 per month, also about a 15% increase. Both are consistent with the suspension of auto-enrollment resulting in higher-cost risk pools.

effects are large, with DD estimates suggesting a 0.146 increase in average enrollee risk (implying 14.6% higher costs) and $57.6 increase in average monthly cost (also about a 15% increase). This implies that marginal enrollees screened out are lower-risk and lower-cost, just as we found in Table 1. We can further compare the methods quantitatively by calculating what Table 1 predicts for the analogous change in average risk score and cost, assuming that passive behavior is exogenous.[46] This exercise predicts a 0.119 increase in average risk score and $58.8 increase in average cost, which are very close to (and statistically indistinguishable from) the DD estimates in Figure 6.[47]

5.2 Do Passive Enrollees Have Duplicate Private Insurance?

A relevant question for the targeting implications of auto-enrollment is whether it enrolls people who *already* have private health insurance, making CommCare duplicative. Although duplication is not supposed to occur – CommCare applicants must attest to not having access to any other health insurance (including any *offer* of job-based coverage) – enforcement could be imperfect. If auto-enrollment "over-enrolls" individuals who already have other coverage, it would be a failure of

Chandra, Gruber and McKnight (2014)).

[46]To do so, note that for any variable Y, $\overline{Y}_{Pre2010} = s_P\overline{Y}_P + (1 - s_P)\overline{Y}_A$, and $\overline{Y}_{Post2010} = \overline{Y}_A$, where "$P$" and "$A$" subscripts refer to passive and active enrollees. Therefore, $\Delta\overline{Y} = \overline{Y}_{Post2010} - \overline{Y}_{Pre2010} = s_P \cdot \left(\overline{Y}_A - \overline{Y}_P\right)$. We calculate $\Delta\overline{Y}$ using the estimates for $\overline{Y}_A$ and $\overline{Y}_P$ in Table 1 and $s_P = 0.326$ from Figure 4.

[47]Appendix C.4 shows a similar robustness analysis for all variables in Table 1; the appendix also describes the methods in greater detail. For all variables, our main method and the DD estimates are directionally similar, always generating estimates of the same sign. Moreover, the methods usually yield quantitatively similar estimates with overlapping confidence intervals.

"statutory targeting" based on program eligibility rules – something that has been observed for transfer programs in a developing country context (Alatas et al., 2016).

To test this story, we draw on evidence from the Massachusetts APCD to measure rates of simultaneous duplicate coverage in CommCare and private insurance, a measure of whether "over-enrollment" occurred in practice.[48] We define the "duplication rate" as the share of CommCare enrollment months during which the member was simultaneously enrolled in other private insurance.[49] Appendix D.1 provides additional details on the data and method.

Overall, we find little evidence of meaningful duplicate coverage in CommCare. The average duplication rate is quite low, just 3.1% of enrollee-months, and the rate is even lower at the beginning of enrollment spells when auto-enrollment occurs (see Appendix Figure A.13). Moreover, there is little evidence that duplication is higher for passive enrollees. Although we cannot distinguish active vs. passive enrollees in the APCD, we can study how duplication rates *change* for new enrollees into CommCare just before vs. after auto-enrollment is suspended in 2010. In practice, the duplication rate rises slightly after the policy change, consistent with marginal (passive) enrollees having lower duplication rates. However, duplication rates are low both before and after the change. Our overall conclusion is that duplicate coverage is rare and is unlikely to explain failure to actively take up coverage.

5.3 Mechanisms: Why Do People Fail to Take Up Free Insurance?

Why do so many people fail to enroll in free health insurance when faced with a small hassle? In this subsection, we provide descriptive evidence to assess the mechanisms involved, including both rational and behavioral explanations. We argue that non-enrollment is unlikely to be explained by fully rational and informed stories, in which individuals are passive because they do not need or benefit from (free) public health insurance. Instead, we argue that behavioral "frictions" are likely involved – with the most likely frictions being inattention and limited understanding of program rules.

5.3.1 Evidence against Fully Rational Non-Enrollment

We start by providing evidence against fully rational and informed non-enrollment. We start by noting that several facts about the institutional setup make this *a priori* less likely. First, everyone in our sample – including passive enrollees – has already *chosen* to apply for public coverage (in "step one" of the process). This suggests that they have some awareness of the program and a desire to enroll. Moreover, the insurance is free and extremely generous, with zero deductible and close to zero cost sharing (the actuarial value exceeds 99%). Although there are some limits (e.g., on networks), it

[48] Ideally, we would want to measure the *counterfactual* of whether CommCare enrollees obtain other insurance if they were (exogenously) kicked out of CommCare. While we cannot measure this counterfactual directly, the observed duplication rate provides suggestive evidence on whether over-enrollment is a problem in general.

[49] We do not include duplicate coverage in CommCare plus Medicaid because the two programs use a unified enrollment system, which should automatically prevent duplicate enrollment. Most of the same insurers operate in both programs, and we have some concerns that the insurance type is sometimes mislabeled, which could lead to false positives.

seems implausible that enrollees would face fewer limits or costs if they were uninsured, the relevant counterfactual.

Some simple facts further indicate that passive enrollees are likely to obtain meaningful benefits from health insurance. Although passive enrollees are *relatively* healthy, they are not *uniformly* so. Indeed, over 40% have a chronic illness, and 8% have a severe chronic illness (Table 1). Their average spending of $228 per month is large relative to their very low incomes (the individual poverty line in 2009 was $903/month). Appendix Figure A.11 shows that passive enrollees experience meaningful rates of medical shocks (e.g., high-cost months, emergency hospitalizations) that while less frequent, still occur 60-75% as often as for active enrollees. Further, Figure 5 shows that even among the oldest and sickest enrollees, passive rates exceed 20%. Thus, while good health is predictive of being passive, it is clearly not the full explanation.

Finally, we argue that access to charity care is unlikely to be a perfect substitute for formal insurance that drives its (true) value down to near-zero. First, passive enrollees use a meaningful amount of care in categories that are less available via charity care, including prescription drugs.[50] Second, the prior literature on the value of insurance to the poor suggests that while value is *low*, it is far above *zero*. For instance, a key paper in this literature, Finkelstein, Hendren and Luttmer (2019a), finds that the individual value of insurance is just 20-48% of insured medical expenses. Applied to our passive enrollees (who spend $228 per month when insured), this would imply a value of $46 to $109 per month – or $550 to $1,300 over a typical 12-month enrollment spell. This is a sizable amount. For instance, it is comparable to foregone benefits from failing to take-up the EITC or SNAP (Bhargava and Manoli, 2015; Finkelstein and Notowidigdo, 2019) and from losses due to insurance plan choice errors (Abaluck and Gruber, 2011; Bhargava, Loewenstein and Sydnor, 2017).

5.3.2 Evidence on Behavioral Frictions

We test two types of behavioral explanations: (1) those in which the *complexity of plan choice* is the key barrier, and (2) those in which *taking action* is the key barrier, for instance because of inattention or misunderstanding the steps required to enroll. We find little evidence of (1) but suggestive evidence consistent with (2).

(1) Choice Overload One reason people might be passive when asked to select a health plan is that they become overwhelmed by the choice, as in models of "choice overload" (Iyengar and Kamenica, 2010). We note that choice overload is *a priori* less likely in the CommCare setting, which featured a relatively simple choice set with at most 4-5 plans available.[51] Further, the passive enrollment rate

[50] We observe that 25% of passive enrollees take a regular prescription medication every month they are enrolled, with an average cost of $45 per month. Over a typical 12-month enrollment spell, these prescription costs alone would add up to $540.

[51] There were four plans prior to 2010, and a fifth (CeltiCare) entered during 2010. This is much simpler than other U.S. insurance programs. For instance, Medicare Advantage features an average choice set with 33 options (see https://www.kff.org/medicare/issue-brief/medicare-advantage-2021-spotlight-first-look), and Medicare Part D feature 25-35 plan options (see https://www.kff.org/medicare/fact-sheet/an-overview-of-the-medicare-part-d-prescription-drug-benefit).

is unrelated to the choice set size, which varies across areas due to selective insurer entry. Appendix Table A.7 shows that the passive rate varies in a narrow range of 33-35% across all choice set sizes – including at 34% in areas with just a *single* plan (i.e., no real choice). Moreover, passivity does not change significantly when a plan enters or exits a region. We conclude that there is little evidence that choice overload is responsible for passive behavior in this context.

(2) Inattention or Misunderstanding A second type of reason for passivity is that some people are inattentive or misunderstand the steps required to enroll in coverage.[52] If so, requiring an additional step of action – even a seemingly simple step – will lead some individuals to "fall through the cracks" and not enroll. We present three sets of facts consistent with a role for inattention and/or misunderstanding. These are discussed here, with the underlying analyses presented in Appendix C.8.

(1) "Lost in the mail": A natural reason for inattention is if some people do not receive the approval letter instructing them how to actively enroll. Anecdotally, address errors are a common problem in welfare programs, partly because of greater residential instability in low-income populations. To test for this, we construct a proxy for "address mismatches" based on observing different zipcodes in CommCare's enrollment file (based on the address used in administrative mailings) vs. on the enrollee's first observed medical claim (submitted by the medical provider, often based on paperwork filled out at a visit). As detailed in Appendix C.8, address mismatch is surprisingly common, occurring for about one-third of enrollees. Moreover, it is predictive of passive behavior. After conditioning on the sample with an observed claim in their first 6 months, the passive rate is 28% for mismatched, about 3% points (or 13%) higher than for non-mismatched people. This pattern is robust to controlling for demographics, health, and timing of the first claim.

(2) Special barriers: Misunderstanding may be more common in groups that face special barriers to interacting with the state and learning about take-up rules. This idea is consistent with the evidence, shown above, that socioeconomically disadvantaged groups are more likely to be passive. Another such group is immigrants, who likely face greater language and cultural barriers.[53] Consistent with this, passive rates are higher for immigrants (41% rate), about 7% points (or 21%) higher than for non-immigrants (34%).

(3) Cross-program transitions: Misunderstanding or inattention may be more common when people transition between public programs in which take-up rules differ. We observe two types of transitions in our data: (1) a large shift of enrollees from the state's Uncompensated Care Pool (UCP) to the CommCare exchange in early 2007, and (2) regular transitions from Medicaid into CommCare (e.g., due to changes in income, age, or family status). Active plan choice was not required in either the UCP or Medicaid, so there may be greater confusion in these groups about enrollment processes in CommCare. Consistent with this, passive rates are much higher for these transitions. People transitioning from the UCP had a 60% passive rate (vs. 40% for other enrollees at the same time in

[52] There is substantial evidence of limited attention/understanding and other behavioral frictions for consumers choice *among* health plans (e.g., Abaluck and Gruber, 2011; Handel, 2013; Ericson, 2014; Handel and Kolstad, 2015). Thus, it is plausible to think that the same issues might affect whether people enroll in health insurance in the first place.

[53] Immigrants were excluded from our main analysis sample, as discussed in Section 3.3. For this analysis, we augment the main sample to re-include them.

early 2007). People transitioning from Medicaid have a 39% passive rate (vs. 31% for non-Medicaid enrollees). The latter is partly driven by very high passivity for kids transitioning off of Medicaid at age 19 (Jácome, 2020), but passive rates are higher for Medicaid transitions even controlling for age, gender, and health covariates.

6 Empirical Model and Policy Tradeoffs

In this section, we empirically apply our model from Section 2 to our health insurance setting in Sections 6.1-6.3, using a combination of our administrative data, the auto-enrollment natural experiment, and outside estimates. We use the estimates to assess the question with which we started the paper: how well do ordeals work to target enrollment in health insurance?

6.1 Model Implementation

Our ordeals welfare framework requires estimates of four objects for enrollees: (1) the direct medical cost of insurance, C_i, (2) the enrollee value of insurance, W_i, (3) social spillovers, E_i, and (4) fiscal externalities, FE_i. Together, these let us calculate $V_i^{Soc} = \mu W_i + E_i$ (for various assumptions on the social welfare weight μ) and $C_i^{Net} = C_i - FE_i$, which together are sufficient for net social welfare, $\gamma_i = V_i^{Soc} - C_i^{Net}$.

Our natural experiment and rich insurance claims data let us directly measure the distribution of marginal (passive) and inframarginal (active) enrollees, and their medical costs (C_i). We assume that the government either directly pays medical expenses (as in traditional Medicare and Medicaid) or engages in zero-profit contracting with private insurers (as we find is roughly true in Massachusetts).[54] In both cases, medical costs for individual i in the claims data are a reasonable estimate of the government's marginal cost when they enroll in insurance (i.e., C_i in the model).[55] With this assumption, our claims data give us a direct estimate of C_i, and the average cost for active ($\overline{C}_1$) and passive ($\overline{C}_0$) enrollees.

To estimate the remaining items (2)-(4), we combine what we do observe with information from other studies and data sources. In what follows, we describe our strategy for estimating each term.

[54] Appendix Table A.9 shows evidence of this zero-profit contracting for the below-poverty population, for whom CommCare negotiated a separate set of payment rates directly with insurers (as opposed to the bidding system used for higher-income groups). The table compares the government's payment and insurer's cost for active and passive enrollees. Insurers earned small overall margins (of about 4%, or \$16 per enrollee-month), despite overpaying for passive and underpaying for active enrollees. The table also shows that had the exchange paid using more sophisticated risk adjustment, this group-specific over/under-payment would shrink, but overall profit margins would remain near-zero. We interpret this as evidence that (1) CommCare was able to negotiate lower average prices for the below-poverty population as a whole because of the inclusion of healthier auto-enrollees, and (2) average prices paid approximately reflect average costs.

[55] This relationship is immediate when the government directly pays claims. In the zero-profit contracting case, the relationship follows from the fact that the government's total payments equals insurers' total cost for all enrollees. When i is enrolled, insurers' total costs increase by C_i, and to maintain zero profits, the government's extra cost is also C_i. Note that this analysis abstracts from any non-medical administrative costs (for either government or private insurers), which we cannot directly measure in our claims data.

1) Uncompensated Care Costs The main component of social and fiscal externalities is uncompensated care, so we start with estimating it. In our data, we observe medical costs when insured, C_i.[56] To estimate uncompensated care costs that i would incur if *uninsured*, we proceed in two steps. First, the uninsured use less care than the insured because of moral hazard, which we assume increases costs by a constant factor, $1+MH$. Second, the uninsured themselves pay only a share, $\phi < 1$, of their medical bills, with uncompensated care covering the other $1-\phi$. Thus, uncompensated care costs equal:

$$C_i^{UC} = \left(\frac{1-\phi}{1+MH}\right) \cdot C_i \tag{14}$$

Estimating C_i^{UC} requires values for ϕ and MH. For our baseline estimates, we draw on the analysis of Finkelstein, Hendren and Luttmer (2019a) of the Oregon Health Insurance Experiment. They estimate a moral hazard effect of $MH = 33.3\%$ and an uninsured out-of-pocket share of bills of $\phi = 0.21$, both of which we treat as constant across enrollees.[57] Using this method, therefore, we estimate $C_i^{UC} = 0.59C_i$.

We consider two alternatives in sensitivity analysis. First, as extreme upper and lower bounds, we consider $\phi = 0$ (full uncompensated care) and $\phi = 1$ (implying $C_i^{UC} = 0$). Second, we construct new estimates using data from a Massachusetts program, the Health Safety Net (HSN), that covers a subset of medical expenses for uninsured low-income adults. The HSN is an uncompensated care pool that (unlike most similar programs) pays based on formal claims, which are observable in the state's APCD. We use this data, combined with estimates of total uninsurance from the ACS, to estimate uncompensated care costs by age-sex group, which we then project onto our CommCare data. The method involves several assumptions, which we detail in Appendix E.

2) Social and Fiscal Externalities of Insurance Having estimated uncompensated care costs, we divide its incidence between the government (part of FE_i) and private providers (part of E_i). We assume that the government bears a fixed share, $\psi_G \in [0,1]$, of costs, which implies:

$$FE_i = \psi_G \cdot C_i^{UC} \qquad \text{and} \qquad E_i = (1-\psi_G)\, C_i^{UC}. \tag{15}$$

Note that this assumes no other externalities of insurance besides uncompensated care, which is a conservative assumption.[58] To estimate ψ_G, we draw on the evidence from Garthwaite, Gross and

[56] Technically, we observe *realized* medical spending, which differs from *ex-ante* expected costs due to the realization of an *ex-post* health shock. We assume throughout that this shock is idiosyncratic and additively separable, so that it averages to zero in any sufficiently large group g (e.g., passive enrollees). Formally, let C_i be realized costs, and $E(C_i)$ be expected costs. We assume that $C_i = E(C_i) + \omega_i$, with $E[\omega_i] = 0$, and ω_i independent of all other variables in the model including group membership. Under these assumptions, $\overline{C}_g = \frac{1}{N_g}\sum_{i\in g} C_i = \frac{1}{N_g}\sum_{i\in g}[E(C_i) + \omega_i] \to \frac{1}{N_g}\sum_{i\in g} E(C_i)$ for large enough N_g.

[57] FHL estimate that in the Oregon experiment, health insurance increases annual medical spending by \$900, which is 33.3% of the control complier (uninsured) mean of \$2700. They estimate that control compliers (the uninsured) spend \$569 per year in out-of-pocket expenses, which implies $\phi = 569/2700 = 0.21$. We treat MH and ϕ as constant across enrollees, implying C_i^{UC} scales proportionally with insured costs, since it is unclear how to estimate heterogeneity. If anything, the evidence suggests that C_i^{UC} are disproportionately larger for passives, suggesting we may (conservatively) understate their relative efficiency

[58] For instance, there is evidence that health insurance for kids leads to long-run economic gains that boost future tax revenue (Brown, Kowalski and Lurie, 2020) and that insurance for young adults reduces crime (Jácome, 2020). We do

Notowidigdo (2018), who study the impact of uninsurance on hospital uncompensated care costs and profits. They find that for every $1 higher uncompensated care costs, hospitals absorb $0.60-0.67 in lost profits. In our main estimates, we set $\psi_G = 0.635$, the midpoint of this range.

3) Enrollee Value of Insurance Estimating value (or WTP) is challenging in our main sample because of a lack of price variation – all plans are free. Moreover, the presence of frictions raises concerns about inferring low WTP directly from passive behavior, which may be a consequence of enrollees having high frictions (e.g., inattention or forgetfulness). To make progress, we follow the "rational consumer benchmark" approach described by Bernheim and Taubinsky (2018), which has also been implemented by Bronnenberg et al. (2015) and Allcott, Lockwood and Taubinsky (2019). The approach involves estimating preferences among a well-informed reference population (the "benchmark") in order to impute the WTP of another group. We use price variation for higher-income CommCare enrollees (150-250% of poverty) who all pay positive prices, replicating and extending the demand estimation method of Finkelstein, Hendren and Shepard (2019b) ("FHS"). We then project these demand estimates onto our below-poverty population at the level of detailed observables (age-sex-risk group cells).

This exercise rests on two assumptions: (1) that higher-income enrollees reveal their WTP when making active choices, and (2) that age-sex-risk observables are sufficient for projecting WTP onto lower-income groups. Assumption (1) is consistent with a model of pure inattention frictions (e.g., forgetting to act) that prevent passive types from enrolling but do not bias demand estimates for active choosers. This assumption implies that demand reveals true WTP *among the sample of higher-income active enrollees* (150-250% of poverty).[59] Assumption (2) allows us to impute this WTP distribution onto our lower-income (0-100% of poverty) population of interest, conditional on age-sex-risk cells. However, it is vulnerable to concerns about selection on unobserved preferences. To address this, we examine robustness to alternative assumptions about unobserved sorting, described in greater detail below.

We summarize the method here, with details and estimates presented in Appendix F. FHS use RD variation in subsidies and premiums to estimate a demand (WTP) curve for insurance. They observe three income thresholds at which premiums increase discretely – from $0 to $39 per month (at 150% of poverty), from $39 to $77 (at 200% of poverty), and from $77 to $116 (at 250% of poverty). By observing how much enrollment falls at each threshold, they infer points on an insurance demand curve. These can be linearly connected and extrapolated to generate a full demand curve $D(s)$, where $s \in [0, 1]$ indexes people from highest to lowest WTP.

To adapt the FHS method to our problem, we make two adjustments. First, we use 2009-11 data, matching our analysis period. Second, we use the micro-data to estimate demand separately by cell of g ={age group, sex, risk score bin}. We use roughly five-year age bins and quintiles of HCC risk score, with an additional category for the sickest 5% of enrollees. With a demand curve for

not include these, since it is unclear how to estimate their distribution for different types of enrollees.

[59] Of course, this benchmark may under/over-state the value of insurance if higher-income active choosers suffer from behavioral biases or liquidity constraints. Our analysis that scales enrollee welfare by a range of social welfare weights, μ, can partly address this concern.

each cell, $D_g(s)$, we project WTP onto each enrollee i in our below-poverty sample using the average WTP for their g cell – i.e. $W_i = E\left[D_{g(i)}(s)\right]$, where the average is over s.[60] This method lets us capture WTP heterogeneity via observable factors included in g (age, sex, and medical risk). We also consider several assumptions for *unobserved* sorting between active vs. passive enrollees – including no sorting, perfect sorting, and (for our baseline specification) unobserved sorting of "equal magnitude" to observed sorting, in a sense formalized in Appendix F.[61]

We consider several alternatives in sensitivity analysis. In addition to variations on the demand-based approach (e.g., no or perfect unobserved sorting), we consider mapping insured medical costs (which we observe) to enrollee WTP using simple relationships estimated in the literature. Specifically, Finkelstein, Hendren and Luttmer (2019a) find that low-income Medicaid enrollees value insurance at 20-48% of insured costs (i.e., $W_i = \kappa \cdot C_i$ for $\kappa \in [0.20, 0.48]$); we report estimates for the endpoints of this range. We also consider a plausible lower bound in which WTP equals expected uninsured out-of-pocket (OOP) costs (with no value for risk protection), based on the framework underlying equation (14). This implies $W_i = \left(\frac{\phi}{1+MH}\right) C_i = 0.16C_i$ given the values of $\phi = 0.21$ and $MH = 0.333$.

Finally, we examine implied WTP for full insurance from a simple model of homogeneous risk aversion, under a benchmark assumption of no moral hazard or uncompensated care. Specifically, we simulate the value of insurance using observed medical claims and an exponential utility function with coefficient of absolute risk aversion of $\alpha = 8.6 \times 10^{-5}$ taken from Handel and Kolstad (2015).[62]

4) Social Welfare Weight (μ) Our key value statistic is the *social* value of insurance, $V_i^{Soc} = \mu W_i + E_i$, which scales enrollee WTP (W_i) by a social welfare weight, μ (and adds externalities, E_i). For simplicity, we use a constant μ for all eligible individuals, but we consider a range of values to capture distributional goals. Our baseline calculations use $\mu = 1$ (i.e., Kaldor-Hicks efficiency), but we consider a range of $\mu \in [0.5,\ 3.0]$ for robustness — where $\mu > 1$ allows for a social value of redistribution, while $\mu < 1$ captures tight public budgets.

6.2 Results: Model Estimates and Targeting

Figure 7 shows our model's baseline estimates and the selection properties of auto-enrollment, comparing active vs. passive enrollees in our main sample (as used in Table 1 above). Panel A of Figure

[60] Calculating average WTP (the conceptually correct statistic) requires using the linearly extrapolated portion of the demand curve, which comprises about the bottom 30-40% of demand. As robustness, we also examine the median and 75th percentiles of WTP, which are much less likely to be extrapolated. These generate smaller estimates of WTP but similar implications for the *relative* WTP and MVPF for active vs. passive enrollees.

[61] Briefly, unobserved sorting relates to the range of s over which we average to calculate $W_i = E\left[D_{g(i)}(s)\right]$. For no sorting, we average over $s \in [0, 1]$ for both actives and passives; therefore, WTP is equal for everyone *within* a g cell. For perfect sorting, we assume that within each g cell, actives comprise the highest 67% of WTP types ($s \in [0.0.67]$) while passives comprise the lowest 33% WTP types ($s \in [0.67, 1.00]$), where 33% is the overall share of passives in our data. For our baseline specification, we assume "equal" sorting on unobservables and observables. Formally, we calculate the probability that a random active enrollee is in a g cell with higher estimated WTP than a random passive enrollee. This is 56% in our data. We then set the averaging ranges of s so that this probability is also 56% *within* each g cell (i.e., unobserved sorting), which we show corresponds to $s \in [0, 0.96]$ for actives and $s \in [0.08, 1.00]$ for passives.

[62] We compute expected utility, $\overline{u}_{g(i)} = E\left[\frac{-1}{\alpha} \cdot exp\left(\alpha C_i\right)\right]$, separately by cells of $g = \{$age group, sex, risk score bin, passive vs. active$\}$, taking the expectation over the observed distribution of monthly medical spending C_i within each cell. WTP for individuals in each cell is defined as the certainty equivalent, $W_i = \frac{1}{\alpha} \cdot log\left(-\alpha \cdot \overline{u}_{g(i)}\right)$.

7 shows selection on social value, which includes both enrollee value and uncompensated care savings to private providers. Both the mean and the distribution of social value is lower for passive enrollees. On average, passive enrollees have both a lower private value of insurance (about 28% less than active enrollees) and use less uncompensated care when uninsured since they are healthier. Their average social benefit is \$143 per month, about 34% less than for active enrollees at \$217 per month. This finding that passive enrollees have lower (private and social) value of insurance than actives holds across every sensitivity analysis we consider, including different assumptions for demand estimation and alternate measures of uncompensated care (see Appendix Table A.10). Our estimates, therefore, robustly suggest the active enrollment ordeal screens out low-value types, consistent with self-targeting and favorable sorting on value.

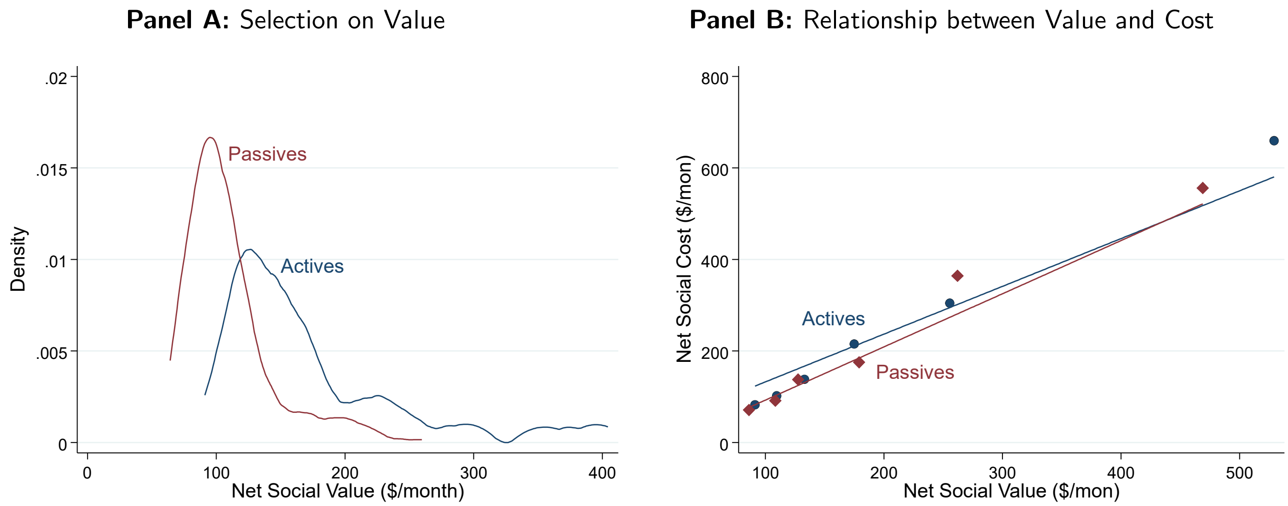
While there is favorable sorting on value, value and costs are also strongly correlated. Panel B of Figure 7 is a binned scatter plot showing the relationship between social value and net public costs, again comparing active and passive enrollees.[63] There is a strong positive correlation between value and cost that holds similarly for both active and passive enrollees. Moreover, the two best-fit lines are nearly on top each other, suggesting that the ordeal achieves little sorting on residual costs (ω_i) conditional on value. Instead, passive enrollees are simply low-value types who also have (proportionally) lower costs. In contrast to the standard case considered in the ordeals literature, screening out low-benefit types is insufficient to make the ordeal well-targeted.

Value-Cost Correlation and the Adverse Selection "Tax" As discussed theoretically in Section 2, a positive value-cost correlation, ρ, reduces the social gains from screening out low-value types, since they also have low costs. The extent of sorting on cost relative to sorting on value is captured by the term $\hat{\beta} = \rho \cdot \frac{\sigma_C}{\sigma_V}$, which we call the "adverse selection tax" on targeting efficiency. In the classic ordeals case with constant or uncorrelated costs ($\rho = 0$), targeting efficiency is purely a function of value sorting. But as the value-cost correlation and the variance of costs increases, this tax becomes larger, which reduces targeting efficiency relative to sorting on value. Overall, the correction term for cost sorting – or the *rate of selection on cost* ($= \Delta C^{Net}/\Delta V^{Soc}$) – equals the sum of the adverse selection tax and any selection on residual costs (ω_i) uncorrelated with value (see Equation 11).

Table 2 shows how this plays out using our estimates of social benefit and cost for both our baseline specification and several alternatives, using $\mu = 1$ for the social welfare weight on beneficiaries. Robustly across all specifications, we find a substantial positive value-cost correlation, ρ, which is 0.69 in our main specification. Correspondingly, we find substantial rates of selection on cost for the ordeal, exceeding 100% in both our baseline and three of the remaining four specifications. The lone exception is the "perfect sorting" specification, which reflects an extreme assumption on how well ordeals sort on unobserved value. But even in the perfect sorting case, we estimate a rate of selection on cost of 58% — i.e., the social gains from targeting are limited to $1 - 0.58 = 42\%$ of the active-passive difference in

[63] At the individual level, we observe realized – not expected – costs. We estimate expected medical costs by taking the mean of monthly realized costs (weighted by number of months enrolled) by cell of g ={age group, sex, risk score bin} interacted with whether the individual was passive or active. Panel B of Figure 7 can therefore be thought of as displaying the joint distribution of social value and expected medical cost at the {age group, sex, risk score bin, active vs. passive status}-cell level.

Figure 7: Model Estimates: Selection on Value and Cost



Note: Panel A plots the density of our estimates of social value separately for both active (in blue) and passive (in red) enrollees, under our baseline demand and uncompensated care assumptions. For ease of visualization, only the bottom 90% of each distribution is shown in Panel A. Panel B illustrates the joint distribution of social value and net costs for active (blue circles) vs. passive (red diamonds) enrollees, along with respective best-fit lines. The sample for both figures is our main 2008-09 new enrollee sample in the below-poverty group, just as in Table 1. See Section 6.1 for the model estimation method. Both figures plot the distribution of estimates (mean WTP and mean costs per month, weighted by number of months enrolled) at the {age group, sex, risk score bin, active vs. passive status}-cell level.

value. Thus, our results suggest that adverse selection tends to reduce, and in many cases overturns, the gains from screening out low-value enrollees.

Value-Cost Ratios and Targeting When the government pays the full cost of insurance, as in CommCare, the value-cost ratio for active and passive enrollees ($\overline{R}_g = \overline{V}_g^{Soc}/\overline{C}_g^{Net}$ for group g) – or social benefit per dollar of net government spending – is informative for targeting efficiency. Table 3 shows the value-cost ratios for both active and passive enrollees in our main sample. In our baseline model (with $\mu = 1$, shown in columns 1-2), we find a higher social value-cost ratio for passive enrollees at 1.00, compared to 0.85 for actives. Mechanically, this reflects the correction for value-cost correlation described above: passive enrollees' proportional cost difference (-44%) exceeds their difference in social value (-34%). Thus, under our baseline specification, the ordeal targets ineffectively ($\Delta\gamma = \overline{\gamma}_1 - \overline{\gamma}_0 < 0$) and results in backward sorting. In principle, it would be optimal to exclude the active enrollees and enroll the passives, but the ordeal does the opposite.

Columns 3-4 of the table show what happens when we allow for distributional concerns by increasing the social welfare weight μ to 3.0, thus scaling up the social value of enrollee welfare. In this case, it is optimal to cover *both* active and passive enrollees because both their value-cost ratios exceed one. Thus, with $\mu = 3$ we are in the "optimal universality" case discussed in the theory.

Appendix Table A.10 reports a variety of sensitivity analyses on these targeting results, using different estimates of enrollee value and uncompensated care. As already noted, the finding that (private and social) value is lower for passive enrollees is highly robust, holding in every specification.

Table 2: Value-Cost Correlation and Targeting

	Value and Cost Specification				
		Sensitivity Analyses			
	Baseline (1)	**No Unobserved Sorting** (2)	**Perfect Unobserved Sorting** (3)	**WTP = OOP Costs** (4)	**Baseline w/ HSN Uncomp. Care Estimates** (5)
A. Joint Distribution					
Value-Cost Correlation *(ρ)*	0.69	0.69	0.66	1.00	0.21
Std. dev. of Net Cost *(σ_C)*	\$246	\$246	\$246	\$246	\$392
Std. dev. of Social Value *(σ_V)*	\$156	\$155	\$183	\$147	\$116
B. Effect of Value-Cost Correlation					
Adverse Selection Tax *(= ρ * (σ_C / σ_V))*	109%	110%	89%	167%	71%
Selection on Residual Cost *(= $\Delta\tilde{\omega}$)*	42%	103%	-30%	0%	282%
Total Effect (= ΔC^{Net} / ΔV^{Soc})	**151%**	**212%**	**58%**	**167%**	**353%**

Note: Column 1 show results from our baseline model estimates, while columns 2-5 show sensitivity to alternative specifications. The sample is our main 2008-09 new enrollee sample in the below-poverty group, just as in Table 1. See Section 6.1 for the model estimation method. Panel A shows properties of the joint distribution of our estimates of social value V^{Soc} and expected net cost C^{Net}, computed at the level of demographic cells defined above in Section 6.1. Panel B shows the implication of the joint distribution for targeting of an ordeal which screens on V^{Soc}, under a baseline assumption of Kaldor-Hicks efficiency ($\mu = 1$). The adverse selection tax, defined as the regression coefficient $\rho \cdot \sigma_C/\sigma_V$, gives the rate at which screening on value also generates screening on cost. We also estimate $\Delta\tilde{\omega}$, the extent to which the enrollment ordeal selects on residual costs (unexplained by social value), which is relative to ΔV^{Soc}.

We also generally find that passive enrollees have similar or larger value-cost ratios, though this finding reverses if sorting on WTP is strong enough (this happens under the "perfect unobserved sorting", and "exponential utility" specifications).

Robustness: Varying Social Preferences for Equity How do different social preferences for equity change the implications of these targeting findings? Figure 8 examines the net social welfare of different policies for varying values of the social welfare weight on enrollees, μ (on the x-axis). As noted, a higher μ indicates a stronger value for distributional equity, given that enrollees are low-income. The graph plots social welfare for three policies: (1) the ordeal, (2) full enrollment, and (3) no enrollment. If ordeals were optimal – i.e., if there were positive gains from targeting – the value of SW^{Ordeal} (dashed blue) would need to be higher than both $SW^{FullEnroll}$ (solid red) and $SW^{NoEnroll} = 0$ (solid black). However, this is never the case: the ordeal is dominated by full enrollment for $\mu > 1.3$, by no enrollment for $\mu < 1.0$, and by both policies for $\mu \in [1.0, 1.3]$.[64]

The figure illustrates the reasons why ordeals are non-optimal, as outlined in Section 2. When μ is sufficiently high (above 1.3), the ordeal is undesirable because society wants to cover *both* active and passive enrollees – i.e., this illustrates what we called "*optimal universality.*" This is likewise true for $\mu < 1.0$, where it is optimal to not enroll both actives and passives. For the small range $\mu \in [1.0, 1.3]$,

[64] Appendix Table A.11 reports sensitivity of this analysis across different demand and externality assumptions. Across most specifications, we find that the ordeal is never optimal at any value of μ. The exceptions are: (1) with perfect unobserved sorting, where the ordeal is assumed to sort extremely well on unobservables so is optimal for a wide range of μ, and (2) with the simulated exponential utility for a narrow range of $\mu \in (0.56, 0.73)$.

Table 3: Targeting Impact of Auto-Enrollment

Value or Cost Variable ($/month)	Baseline (Demand Estimates): Baseline ($\mu = 1.0$) Active Enrollees (1)	Baseline ($\mu = 1.0$) Passive Enrollees (2)	Higher Welfare Weight ($\mu = 3.0$) Active Enrollees (3)	Higher Welfare Weight ($\mu = 3.0$) Passive Enrollees (4)
Social Benefits				
WTP of Enrollees (demand estimate, W_i)	$128	$93	$384	$280
Spillovers: Private Uncomp. Care Savings (E_i)	$88	$49	$88	$49
Total Benefits	**$216**	**$143**	**$473**	**$329**
Public Costs				
Medical Spending (gross costs)	$408	$228	$408	$228
Fiscal Externality: Public Uncomp. Care Savings (FE_i)	-$154	-$86	-$154	-$86
Net Public Cost (C_i^{Net})	**$255**	**$142**	**$255**	**$142**
Value-Cost Ratio (R_i)	***0.85***	***1.00***	***1.86***	***2.31***
	(Backward Sorting)		(Enrolling Both Groups Optimal)	

Note: Columns 1 and 2 of the table show shows our baseline model estimates of the social benefits and costs of insurance for active vs. passive enrollees (or inframarginal vs. marginal enrollees due to auto-enrollment), while column 3 shows the estimates where enrollee private valuations have been scaled by a social welfare weight of $\mu = 3$.. The sample is our main 2008-09 new enrollee sample in the below-poverty group, just as in Table 1. See Section 6.1 for the model estimation method. Enrollee value comes from our demand estimates, using the specification with unobserved sorting equal to observed sorting on WTP.

it would in theory be desirable to exclude the active enrollees, while covering the passives, but ordeals do the opposite. Thus, this case illustrates *backward sorting.*

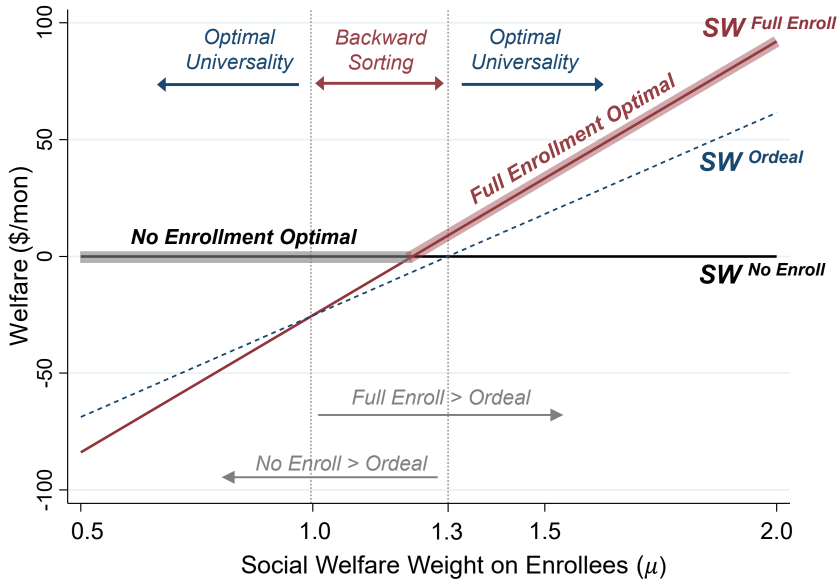
6.3 Policy Comparison: Auto-Enrollment vs. Subsidies

While the main focus of our paper is on the targeting properties of ordeals, we can also use our estimates to compare the tradeoffs of two different take-up policies: auto-enrollment vs. subsidies. We think of this as a guide for an insurance policymaker who has extra funds and can choose whether to expand coverage via auto-enrollment (for zero-premium enrollees) or larger subsidies (for higher-income groups). This analysis is relevant to understanding tradeoffs under the ACA today, in which 40-50% of the uninsured likely qualify for free coverage (Cox and McDermott, 2020) while many middle-income uninsured Americans owe premiums that could be reduced via larger subsidies. It also reflects (in reverse) Massachusetts' 2010 situation when it chose to eliminate auto-enrollment, rather than cutting subsidies.

For auto-enrollment, we use our model estimates, as just discussed. For subsidies, we use the results of Finkelstein, Hendren and Shepard (2019b). We consider the three subsidy changes in their analysis: reducing premiums from $39 per month to $0 (for enrollees at 150% of poverty), from $77 to $39 (at 200% of poverty), and $116 to $77 (at 250% of poverty).

This analysis yields two main results, shown in Table 4. First, all four take-up policies involve similar enrollment impacts of +32-36%. They also all enroll a similar set of low-cost marginal en-

Figure 8: Optimal Policy under Varying Social Values of Equity (μ)



Note: The figure plots net social welfare of ordeals (blue dashed line) vs. full enrollment (red solid) and no enrollment (black solid, which is normalized to 0) under different values for the social welfare weight μ (the x-axis). Social welfare is average net welfare ($= V^{Soc} - C^{Net}$) per eligible person per month. The graph shows that the ordeal is not optimal for any value of μ; it is dominated by no enrollment for lower values ($\mu < 1.3$) and by full enrollment for higher values ($\mu > 1.0$) and by both policies for $\mu \in [1.0, 1.3]$ which is the region of backward sorting.

rollees, with medical costs of $196-281 per month (well below the market average of $370). Indeed, after subtracting premiums paid, the "gross subsidy" for marginal enrollees is remarkably similar across policies, ranging from $196 to $229. The same is true of the net public cost, after subtracting uncompensated care savings. Overall, this suggests that auto-enrollment and the three subsidy expansions have relatively similar take-up impacts and targeting properties.

Second, however, the two policies differ markedly in their expenditures on inframarginal enrollees. Auto-enrollment spends nothing on inframarginal (active) enrollees, while the subsidies all spend >$100 per marginal enrollee on transfers (the $38-39 monthly subsidy increase times the ≈ 3 inframarginals per marginal enrollee). As a result, auto-enrollment is a much more *cost-effective* policy for expanding take-up. Auto-enrollment's net public cost per newly insured is 36-40% lower than for subsidies. This implies that each $1 million in public spending covers 55-66% more people if used for auto-enrollment rather than subsidies. Therefore, a budget constrained government wishing to maximize take-up would want to prioritize auto-enrollment over subsidies.

On the other hand, if the government wishes to implement the highest-MVPF policy, the analysis also depends on the relative MVPF of insurance versus cash transfers, since subsidies combine the two.[65] Cash transfers have an MVPF of 1 in our model (since we do not include labor supply

[65] MVPFs are calculated as follows. For auto-enrollment, we assume (conservatively) that the ordeal involves no real welfare costs ($L(\sigma) = 0$), so its MVPF is simply the social value-cost ratio of marginal (passive) enrollees, as in Table 3. For subsidies, the MVPF combines the social value of insurance (for the ΔD_S marginal enrollees) plus the value of cash

Table 4: Policy Comparison: Auto-Enrollment vs. Subsidies

	Auto Enrollment	Subsidy Increase (↓ premiums)		
		\$39 to \$0	\$77 to \$39	\$116 to \$77
	0-100% FPL	*150% FPL*	*200% FPL*	*250% FPL*
	(1)	(2)	(3)	(4)
Panel A: Marginal Enrollees				
Enrollment Impact	**32%**	**34%**	**36%**	**32%**
Social Benefit ($W_i + E_i$)	\$143	\$62	\$116	\$157
Medical Costs	\$228	\$196	\$268	\$281
Gross Subsidy *(= costs - premiums paid)*	\$228	\$196	\$229	\$204
Net Public Cost *(= gross subsidy - FE)*	\$142	\$122	\$128	\$98
Value-Cost Ratio (Marginals)	***1.00***	***0.51***	***0.90***	***1.60***
Panel B: Transfers to Inframarginals				
Premium Discount (\$/month)	--	\$39	\$38	\$39
x Inframarginals per marginal	3.12	2.92	2.80	3.14
= Transfer Spending per marginal	\$0	\$114	\$106	\$123
Value-Cost Ratio (Inframarginals)	**--**	***1.00***	***1.00***	***1.00***
Panel C: Cost-Effectiveness and MVPF				
Cost-Effectiveness				
Net Public Cost per Newly Insured	\$142	\$236	\$235	\$221
ΔInsured per \$1 million	**7,024**	**4,238**	**4,261**	**4,530**
Overall MVPF of Policy	***1.00***	***0.74***	***0.95***	***1.27***

Note: The table compares auto-enrollment with three subsidy changes generated by premium RDs at three income thresholds: a premium decrease from \$39 to \$0 per month at 150% of poverty (FPL) (column 2), from \$77 to \$39 at 200% of FPL (column 3), and from \$116 to \$77 at 250% of FPL (column 4). For auto-enrollment, results come from our model estimates (Section (6.1)) using the reduced form variation studied in this paper. For subsidies, estimates come from our calculations using the WTP and cost results reported in Finkelstein, Hendren and Shepard (2019b). Demand for marginal enrollees is assumed to equal the midpoint of the higher and lower premium amounts, and uncompensated care estimates come from applying our model in Section 6.1 to marginal enrollees' costs. Cash transfers are assumed to have an MVPF of 1.0.

distortions), while the social value-cost ratio of insurance for marginal enrollees (with $\mu = 1$) ranges from 0.51 to 1.60 for subsidies and is (coincidentally) 1.00 for auto-enrollment. As a result, we find that auto-enrollment's MVPF (= 1.00) lies within the range of the three subsidy changes (from 0.74 to 1.24).

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discounts to inframarginals (= ΔS times D_0 inframarginals), divided by the total fiscal cost, or:

$$MVPF_S = \frac{\overbrace{\Delta D_S \overline{V}_S^{Soc}}^{\text{Insurance for marginals}} + \overbrace{D_0 \Delta S}^{\text{Cash for marginals}}}{\Delta D_S \overline{C}_S^{Net} + D_0 \Delta S} = \underbrace{\kappa_M \times \left(\frac{\overline{V}_S^{Soc} + \overline{E}_S}{\overline{C}_S^{Net}}\right)}_{\text{MVPF of marginals}} + \underbrace{(1 - \kappa_M) \times 1}_{\text{Transfer to inframarginals}} \tag{16}$$

where $\overline{X}_S$ is the average of each variable X for subsidy-marginals, and $\kappa_M \equiv \frac{\Delta D_S \overline{C}_S^{Net}}{\Delta D_S \overline{C}_S^{Net} + D_0 \Delta S}$ is the share of extra spending on marginal enrollees. The equation shows that the MVPF of a subsidy is a weighted average of the MVPF of covering marginal enrollees and the MVPF of a cash transfer to inframarginals (which is 1.0).

7 Conclusion

Enrollment ordeals are a pervasive and controversial feature of many public programs, especially safety net programs for the poor. There is a longstanding debate and tension between two views. On the one hand, ordeals are barriers to poverty alleviation programs, which may undermine their goal of helping the poor. In this view, ordeals are inherently harmful – and particularly so when they reduce take-up a lot.

On the other hand, the classic economic ideas of Nichols and Zeckhauser (1982) shows how ordeals can *target* public assistance towards those who need or value it most, saving money that can be redeployed toward those in greatest need. In this view, ordeals are harmful only if they fail to target well. Because the "self-targeting" case for ordeals relies on revealed preferences, standard critiques have largely focused on *behavioral frictions* as the main reason ordeals may not target well (Bertrand et al., 2004; Finkelstein and Notowidigdo, 2019).

This paper argues that there is another big-picture reason ordeals self-targeting may not work well: *adverse selection*. We start by observing that in many public programs, enrollees vary in not just their *value* of assistance but also their *cost*. In other words, many programs – including but not limited to those providing insurance – share the key feature of "selection markets" that have been widely studied in the economics literature (Einav, Finkelstein and Mahoney, 2021). We then show that adverse selection tends to undermine the classic self-targeting logic for ordeals. When low-value types – those who ordeals are designed to screen out – also have low costs (e.g., because they are lower-risk types), targeting gains from excluding them may be minimal, or even negative. The key question in selection markets is not whether ordeals screen on value, but whether they screen *more strongly* on value than on costs.

We develop a general framework to formalize this idea, visualized using the graphical selection markets model of Einav, Finkelstein and Cullen (2010) and measured using a parameter we call the "adverse selection tax." We then test it empirically using a natural experiment in a subsidized health insurance program in Massachusetts. We find that eliminating auto-enrollment and adding a small ordeal leads to major 33% declines in enrollment. Ordeals differentially exclude precisely the young, healthy, and low-risk types one would expect under adverse selection. These individuals have lower value for insurance (consistent with self-targeting) but they are also much lower cost. Our model estimates suggest that they are not less efficient, implying that ordeals induced "backward sorting" into insurance, analogous to the findings of Marone and Sabety (2022) for price-based sorting. This occurs because adverse selection is very strong, with a "tax" exceeding 100% in our baseline estimates. With distributional equity concerns, health insurance is socially optimal, but it is optimal for all enrollees, including passive types screened out by ordeals – consistent with our idea of "optimal universality."

These findings have broader implications for how policymakers think about enrollment ordeals in social programs. In terms of *take-up* impact, our results suggest that ordeals are a first-order important barrier in health insurance. Even when coverage is free, a large share of people do not enroll when doing so is a hassle. Completely removing ordeals via auto-enrollment has an order of magnitude larger take-up impact than lower-touch "nudges" like reminders and outreach (Domurat et al., 2021;

Goldin et al., 2021; Ericson et al., 2023; Banerjee et al., 2021). Reaching universal coverage in the U.S., therefore, may require automatic enrollment in some form.

In terms of *targeting*, our results suggest that the standard case for ordeals is less likely to work well in settings with adverse selection – i.e., strongly correlated value and costs. This is clearly relevant for insurance programs, but it may also be relevant more broadly in transfer programs that pay varying benefit amounts to different groups. Fundamentally, adverse selection (like behavioral biases) interrupts the revealed preference link between demand and efficiency that is key to self-targeting. While ordeals are useful tools in some settings, they may not be well suited to health insurance and other adversely selected markets.

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Appendix for Online Publication:
"Do Ordeals Work for Selection Markets?"

Mark Shepard and Myles Wagner

A Data Construction and Summary Statistics

A.1 American Community Survey (ACS) Dataset

To provide context for our estimated impacts of auto-enrollment, we use the ACS to construct estimates of the number of CommCare-eligible uninsured people in Massachusetts. We begin with ACS data from 2009 (the year when our policy change occurred), derived from the IPUMS website (Ruggles et al., 2015). A key variable for our analysis is family income as a share of the poverty line, analogous to the measure used by CommCare. To define this in the ACS, we sum total personal income across all members of an individual's "health insurance unit" (HIU), a variable defined by the University of Minnesota's SHADAC to approximate family unit definitions used by public insurance programs. We divide this total income by the FPL defined by the year and the HIU size.

We then define people as *CommCare-eligible uninsured in the below-poverty group* if they are:

1. Massachusetts residents in the relevant age range (19-64) and income range (0-100% FPL),[66]
2. Not enrolled in any health insurance,
3. Not eligible for Medicaid, based on income and demographics (see below), and
4. U.S. citizens.

We restrict the sample to U.S. citizens because most non-citizens are ineligible for CommCare. Further, we drop non-citizen immigrant enrollees from our main CommCare sample, so this makes the two datasets more comparable.

Implementing item #3 (Medicaid eligibility) requires some care because we do not observe all variables needed to perfectly measure Medicaid eligibility in the ACS.[67] Instead, we approximate it by excluding the largest groups we know are Medicaid eligible: (1) Children up to age 18 (already excluded above), (2) Parents with income below 133% of FPL, and (3) People with disabilities (proxied by under 65 and SSI receipt). This leaves just non-disabled childless adults, who are the only CommCare-eligible group among below-poverty individuals.

[66] Massachusetts children (up to age 18) with incomes up to 300% of poverty are eligible for Medicaid. Seniors age 65+ are eligible for Medicare.

[67] We cannot even reliably measure Medicaid enrollment; the ACS does not distinguish between Medicaid and CommCare (both are coded as "Medicaid/other public insurance").

We use this final sample, along with ACS population weights supplied in the IPUMS extract, to estimate the number of CommCare-eligible uninsured people with incomes below 100% of poverty in our main year (2009).

A.2 Additional Summary Statistics

Monthly New Enrollment Data Our main enrollment analysis is conducted at a bimonthly level, averaging the number of new enrollees into CommCare across pairs of months. We do this because of several cases where auto-enrollment appears to have been suspended for a month, followed by a larger number of auto-enrollees the next month. Averaging across pairs of months smooths over this noise in the data and lets us improve the precision of our estimates. For completeness, Figure A.2 shows the monthly count of new enrollees in the 0-100% of poverty group (analogous to the similar Figure 3 in the main text).

Summary Statistics Table A.1 shows summary statistics on CommCare enrollment for the 0-100% of poverty treatment and 100-200% of poverty control groups over the fiscal year 2007-2011 period, broken down into: (1) the initial auto-enrollment period in 2007 when participants in the state's Uncompensated Care Pool were auto-converted into CommCare, (2) the main auto-enrollment period of 2008-2010m1, and (3) the no auto-enrollment period (2010m2-2011), excluding the three months at the end of 2010 when it was temporarily reinstated. See Section 3.3 for a description of the sample construction. Table A.2 shows additional statistics on enrollee attributes among new enrollees in our 0-100% of FPL group.

B Additional Analyses on Enrollment Impacts

B.1 Robustness: Alternate Specifications for New Enrollment Impacts

Table A.3 shows robustness checks on the estimated impacts on new enrollment of the end of auto-enrollment in FY 2010. Column (1) reports the estimates from the baseline specification from the body text, as shown in Figure 4. This uses a control group of 100-200% of FPL income enrollees, and it excludes the "temporary reinstatement" period at the end of 2010 when auto-enrollment was reinstated. Column (2)-(5) report results for alternate income groups as controls, including no control group in column (5) – i.e., a simple pre/post-2010 difference for the treatment group. Column (6) uses the baseline 100-200% FPL control group but adds the temporary reinstatement period to the sample, with this period coded as having auto-enrollment turned on. (Because we lack the data flag to observe active vs. passive enrollment in this period, we cannot show estimates for the outcome of active enrollment.) All of the estimates are extremely similar to the baseline estimates, suggesting that they are not sensitive to the control group or sample period.

B.2 Impacts on Re-Enrollment

In the main results, we limited our analysis to new (first-time) enrollees, and exclude re-enrollees who are returning to the market after some period of not being enrolled. Including re-enrollees would complicate our main analysis, since re-enrollees faced different auto-enrollment rules and the number of re-enrollees mechanically increased over time with the age of the marketplace. In this Appendix section, we present results testing the robustness of our main finding to including those re-enrollees.

Re-enrollees were auto enrolled one of two ways, depending on the length of their absence prior to re-enrolling: (1) those with a gap in enrollment of 13+ months were treated as new enrollees, while (2) those who had been away for ≤12 months were immediately enrolled in their previous plan without having to take any action (though they could switch ex-post). Further, among ≤12 month re-enrollees, auto-enrollment was used for a broader set of income groups prior to 2009, including for enrollees above 100% of poverty that were part of our control group for the main analysis. We therefore cannot perform the same difference-in-difference analysis for ≤12 month re-enrollees.

In what follows, we first analyze the robustness of adding 13+ re-enrollees to our main sample of new enrollees, since both groups faced similar auto-enrollment rules and there is a valid control group for both. We then show changes in the flow of ≤12 month re-enrollees around the policy change for 0-100% of poverty enrollees without a control group.

Including 13+ Month Re-enrollees in Main Estimates

We start by adding re-enrollees returning after an absence of at least 13 months into our main estimates including new enrollees. This is straightforward, since they were subject to the same auto-enrollment rules as new enrollees. We directly reproduce the analysis in Section 4 combining the number of new enrollees plus the 13+ month re-enrollees into both treatment and control groups.[68] The flow of 13+ month re-enrollees is initially quite small relative to the number of new enrollees, since the marketplace itself was less than two years old at the start of fiscal year 2009. The number of returning below-poverty enrollees steadily increases over time but never rises above 600 re-enrollees per month by the end of fiscal year 2012 – less than one-sixth of the average flow of new enrollees.

The results are shown in Figure A.4. Due to the relatively small share of 13+ month re-enrollees in the population, it is perhaps unsurprising that our results do not change dramatically with their inclusion. The main DD estimate for the impact on enrollment is a 39.1% decrease, slightly larger than the 32.6% decrease found for new enrollees alone in Figure 4. As in the main results, the DD estimate for active enrollment is still close to zero and statistically insignificant. The point estimate, which is slightly negative, continues to be inconsistent with the presence of "purposely passive" types – which would predict an *increase* in active enrollees after the policy change.

Re-enrollees with ≤12 month gap in coverage

We now analyze impacts for re-enrollees returning to CommCare within 12 months of the end of their last spell – who we call "short-gap re-enrollees." The monthly numbers of short-gap re-enrollees are substantial, averaging around 1,450 re-enrollees per month during the pre-period (the first 11 months of fiscal year 2009) – roughly a third of the number of new enrollees. Our analysis for short-gap re-enrollees needs to differ for three reasons. First, short-gap re-enrollees were *automatically* re-enrolled in their previous plan, without being given a chance to actively choose. Hence, we cannot distinguish active vs. passive types – essentially all are passive enrollees in the data – and instead focus solely on the effects on the *total* number of short-gap re-enrollees joining the market each month. Second, we do not have a valid control group for this analysis because auto-enrollment also applied to higher-income re-enrollees above 100% of poverty. As a result, we show results based on a simple pre/post enrollment change, without a control group. Third, the timing of the policy change is slightly different: auto-enrollment for short-gap re-enrollees ended in FY 2009m11, two months prior to the end for new enrollees (in 2010m1).

Figure A.5 shows the flow of short-gap re-enrollees, with the units rescaled so that the pre-period mean is 1.0. This flow drops sharply when auto-enrollment is suspended. The overall pre/post estimate is -35.3%. This is roughly similar to the main estimate on new enrollees (-32.6%). This similarity again suggests that our main estimates in the paper are a faithful representation of the overall effect of suspending auto-enrollment.

[68] As in the main analysis, above-poverty 13+ month re-enrollees were never subject to auto-enrollment.

B.3 Impact on Steady-State Enrollment

The goal of this section is to translate our estimated impacts of auto-enrollment on the *flow* of new enrollment into estimates for the *stock* of exchange enrollment in steady state. To do so, we start with a simple stock-flow model framework for the calculation.

Suppose that there are $g \in \{1, ..., G\}$ types of enrollees, each of which has a constant enrollment inflow into the market of E_g people per month and an exit rate of x_g. Total enrollment among type-g enrollees at time t is determined by the stock/flow equation: $N_{g,t} = (1 - x_g) N_{g,t-1} + E_g$. In steady state ($N_{g,t} = N_{g,t-1}$) total type-$g$ enrollment is $N_g^{SS} = E_g / x_g$. Total steady-state market enrollment is $N^{SS} = \sum_g (E_g / x_g)$.

Now apply the framework to the CommCare market. For simplicity, we use just two g types: passive (P) and active (A) enrollees; though not shown, we find the results are similar if we interact these with age-gender groups. Figure 3 shows that constant enrollment inflow (separately for actives and passives) is a reasonable approximation from 2009 on, and Appendix Figure A.6 suggests the same is true for the exit rate. Using the final six months auto-enrollment is in place as the estimation period, we estimate $\{E_A, E_P\} = \{3013, 1366\}$ and $\{x_A, x_P\} = \{0.0648, 0.0917\}$ (see Appendix Figure A.6). These imply that $N_A^{SS} \approx 46,500$ and $N_P^{SS} \approx 14,900$. This suggests that ending auto-enrollment decreases steady-state market size by about 32% of steady state active enrollment ($= 14,900 \,/\, 46,500$).

Figure A.7 compares this calculation to data on actual CommCare enrollment for the relevant 0-100% poverty group. The plot shows the total stock of enrollment over time, both overall (green) and separately by whether each enrollee initially joined the market actively (blue) or passively (red).[69] The estimates from the steady-state calculation above are indicated with horizontal dashed lines. Both active and passive enrollment rise quickly during the first year of the market (up to mid-2008). Active enrollment then stabilizes near the steady-state value of 46,500 and remains remarkably stable over the next five years. Passive enrollment declines in 2008-09 – consistent with the gradual exit of the 2007 surge in auto-enrollees – but starts to stabilize in late 2009 near the steady state level. It then declines towards zero once auto-enrollment is suspended. Overall, these enrollment trends are remarkably consistent with the simple back-of-the-envelope calculation, suggesting that the estimate of a 32% effect on steady-state enrollment is reasonable.

[69]Consistent with our analysis of new enrollees, we restrict the count to people in their first enrollment spells; we analyze re-enrollees separately in Appendix B.2.

C Additional Analyses of Targeting and Mechanisms

C.1 Robustness of Health Differences to Measurement Period

Our main targeting analysis in Section 5 found that passive enrollees were healthier than active enrollees. However, the key health variables – including chronic illnesses and risk scores – are measured based on diagnoses coded in insurance claims, which are measured only when individuals use care. While this is standard empirical practice in the health economics literature, it raises a specific concern in our setting. We showed that passive enrollees were enrolled for *shorter durations* than active enrollees. Therefore, it is possible that we would spuriously measure passive enrollees as healthier simply because they have fewer opportunities for diagnoses to be observed, rather than due to true health differences.

To address this concern, we test the robustness of our analysis to using a constant measurement period: the *monthly rate* of observed diagnoses in each of months 1-12 of individuals' enrollment spell. Recall that in our main specification, health variables are based on observed diagnoses during the first (up to) 12 months of each individual's enrollment spell. This robustness check shows month-by-month values for the variables underlying the main analysis.[70]

Figure A.8 shows results comparing active vs. passive enrollees on our four main health-related variables: (A) medical spending per month, (B) rate of chronic illness diagnoses, (C) rate of severe chronic illness diagnoses, and (D) medical risk score. In all panels, the x-axis is the month of an individual's enrollment spell, and the plots show the monthly rate for each variable. Note that the *monthly* rate of observed illnesses is mechanically lower than the *annual* rate (as reported in Table 1) because the latter codes people as chronically ill if they are *ever* observed with a relevant diagnosis during the whole year.[71] The key issue is not the level of the variable but the relative comparison for active vs. passive enrollees. We show results for three sub-samples: (1) the unbalanced panel of all enrollees still enrolled through month t (solid line with markers), (2) a balanced panel of people enrolled ≥ 6 months (dashed line), and (3) the balanced panel of people enrolled ≥ 12 months (solid line, no markers). These three series help show whether the results are sensitive to compositional changes in the sample over time.

We find persistent differences in monthly illness rates, with passive enrollees consistently healthier than active. The differences in any/severe chronic illness rates (panels B-C) and risk scores (panel D) are remarkably consistent over time, and they are steady even when conditioning on a balanced panel. These findings suggest differences in observed health between active and passive enrollees primarily reflect differences in underlying health and/or health care usage, rather than differences in length of time enrolled in CommCare. The spending differences (panel A) are also fairly stable, though the active/passive gap is somewhat larger in the first few months of an enrollment spell before narrowing somewhat. This difference may indicate delays in passive enrollees seeking care, perhaps because

[70] CommCare enrollment occurs at monthly intervals (no partial month coverage), so once we condition on the people enrolled in a given month, there is no difference in the observation period.

[71] For instance, 43% of passive enrollees are coded with a chronic illness diagnosis at some point over their first year enrolled vs. a monthly rate of ˜10% of passive enrollees being observed with a diagnosis.

they do not initially know they have been auto-enrolled. Nonetheless, monthly spending for passive enrollees is consistently more than 30% below that of active enrollees, implying that the active vs. passive spending gap is not simply a function of the different observation periods for the two groups.

In Table A.4, we report sensitivity of the targeting results on health measures (reported in Table 1) to changes in the measurement period and to adding a balanced panel restriction. Consistent with the differences in monthly diagnoses and healthcare use described above, we find consistently large differences in health measures between active and passive enrollees across all measurement periods and sample restrictions. Because differences between active and passive enrollees in diagnoses and utilization are somewhat larger in the earliest months of an enrollment spell, the differences in overall measured health are larger when using shorter measurement periods and when not restricting to a balanced panel. Even when restricting to the balanced panel of enrollees whose spells last at least 12 months (reported in the final column), passive enrollees are substantially healthier than active enrollees, and the proportional differences in health measures are similar to the baseline estimates reported in column 1 and in Panel B of Table 1.

C.2 Understanding Shorter Enrollment Durations for Passive Enrollees

Our main analysis shows that passive individuals are enrolled for shorter periods. To explore the reasons for this difference, Figure A.9 plots the exit hazard rate from CommCare after each month in an enrollment spell. It compares active (blue) vs. passive auto-enrollees (red). Passive enrollees have higher exit hazards in nearly all months over the first two years of a spell, but the (level and proportional) differences are largest at two times. First, passive enrollees are much more likely to exit after months 1-2 of a spell, consistent with a brief need for health insurance coverage (e.g., between jobs). Second, passive enrollees are much more likely to exit after months 12-14 of a spell. We do not directly observe the reason for this spike, but we know that this is is coincident with the timing of annual eligibility redetermination. Exit rates spike for both active and passive enrollees at this time, but the spike is larger for passive enrollees. This may be consistent with passive enrollees' failure to complete redetermination paperwork – a major reason for termination – just as they did not respond to the initial CommCare approval letter.

C.3 Active vs. Passive Use of Standard Sources of Charity Care

Figure A.10 shows several pieces of evidence that passive enrollees, though healthier (and therefore lower spending), obtain a *larger share* of their care from standard sources of charity care (or "uncompensated care"). The left two sets of bars show patterns of use of physician office visits (a form of elective care less likely to be available via charity care) versus emergency room use (a classic source of charity care). Consistent with being healthier, passive enrollees are less likely than actives to use both measures, but the ratios are quite different. They visit a physician's office 51% as frequently as active enrollees, but use emergency rooms 90% as frequently as active enrollees. Consequently, a greater share of passive enrollees' total medical spending is due to hospital-based emergency care (34%; this measure includes both the ER visit and any subsequent admissions) than for active enrollees (23%).

The final panel breaks down the source of hospital care for active vs. passive enrollees (weighted by cost). It shows the share of care that occurs at safety net hospitals, a designation of the state of Massachusetts based on a hospital having a high public-payer and uninsured share. Consistent with living closer to safety net hospitals, passive enrollees obtain 46% of their hospital care from these hospitals, versus 39% for active enrollees.

Overall, this evidence is consistent with passive enrollees obtaining a larger share of their care while insured from standard sources of charity care, including emergency rooms and safety net hospitals. However, it is worth adding caveats to this point. First, we only observe care utilization while *insured* in CommCare. We cannot directly observe individuals' (counterfactual) charity care use had they been uninsured. Our expectation is that sources of care while insured and uninsured would be correlated, but this is not a certainty. Second, because of these limitations, we do not use this evidence directly in estimating our model of uncompensated care costs in Section 2. Instead, we rely on a simple model that assumes a *constant* ratio between uncompensated care and observed costs while insured for active and passive enrollees. In this sense, the model is likely conservative in its conclusion that covering passive enrollees improves targeting, since it may understate passive enrollees' fiscal and social externalities relative to insured costs.

C.4 Robustness: Using Policy Change to Infer Marginal Enrollee Characteristics

In this appendix, we discuss results of a validation exercise for our main targeting results in the body text Section 5.1. Our main analysis uses a direct comparison of active vs. passive new enrollees during the 2008-09 period, assuming that all *observed* passive enrollees are marginal enrollees due to the auto-enrollment policy (and all active enrollees are inframarginal). Essentially, our main analysis assumes that passive behavior is exogenous to the auto-enrollment policy. Although we provide evidence in support of that assumption in the text, in this appendix we implement a robustness analysis that does not rely on this assumption. Specifically, we use *changes in the composition* of all new enrollees after the suspension of auto-enrollment in 2010 to infer the characteristics of marginal enrollees.

To do so, we run difference-in-difference (DD) regressions comparing average new enrollee characteristics for the treatment group (0-100% poverty enrollees, for whom auto-enrollment ends in 2010) vs. the control group (100-200% of FPL, for whom there is no auto-enrollment throughout). The DD regression specification is analogous to what we used for our main enrollment estimates (equation (13)):

$$Y_{i,g,t} = \alpha_g + \beta_t + \gamma \cdot 1\{g = Treat, t \geq 2010\} + \varepsilon_{i,g,t} \tag{17}$$

where $Y_{i,g,t}$ is the characteristic/outcome for new enrollee i in group g (treatment or control) who joins the exchange at time t (in bimonths). As with our main enrollment analysis, we run (17) on data from 2009-2011, excluding the period of auto-enrollment's temporary reinstatement at the end of 2010. Unlike the enrollment analysis, regressions are run at the enrollee level, and we cluster standard errors at the group (g) x time period level. The coefficient of interest is γ, which is the DD estimate of the compositional impact (the change in average characteristics) of turning off auto-enrollment.

Because for some variables we see signs of differential pre-trends, we also run a DD specification with group-specific linear time trends:

$$Y_{i,g,t} = \alpha_g + \beta_t + \gamma \cdot 1\{g = Treat, t \geq 2010\} + \delta_g \times t + \varepsilon_{i,g,t} \tag{18}$$

Appendix Table A.5 reports estimates of γ from these regressions, with the simple DD estimates (equation (17)) shown in columns (3)-(4) and the DD model with linear time trends (equation (18)) shown in columns (6)-(7).[72] To compare these estimates to the main targeting results in the paper, columns (1)-(2) report the implied compositional change ($\Delta\overline{Y}$) that would occur using the estimates in body text Table 1 and the assumption that passive behavior is exogenous. Specifically, define $\overline{Y}_P$ as the mean for passive enrollees, $\overline{Y}_A$ as the mean for active enrollees, and s_P as the share of enrollees who are passive. After auto-enrollment ends, only actives enroll, so $\overline{Y}_{Post2010} = \overline{Y}_A$. While auto-enrollment was in place, both groups enrolled, so $\overline{Y}_{Pre2010} = s_P\overline{Y}_P + (1 - s_P)\overline{Y}_A$. Therefore, the compositional change at the start of 2010 is:

$$\Delta\overline{Y} = \overline{Y}_{Post2010} - \overline{Y}_{Pre2010} = s_P \cdot \left(\overline{Y}_A - \overline{Y}_P\right) \tag{19}$$

Columns (1)-(2) of Table A.5 report the estimate of $\Delta\overline{Y}$ using the values of $\overline{Y}_A$ and $\overline{Y}_P$ from body text Table 1 and our main enrollment estimate that $s_P = 0.326$. Confidence intervals (and implied standard errors) are calculated using the bounds implied by the confidence intervals of $\overline{Y}_A$ and $\overline{Y}_P$, implicitly assuming independence.

Comparing the implications of our main targeting analysis (columns (1)-(2)) with the DD estimates in the remaining columns yields several conclusions. First, for *all the variables*, both our main analysis and the DD estimates are of the same sign (positive or negative). This implies that the methods align directionally in terms of how marginal enrollees compare to inframarginals – e.g., marginals are younger, healthier, lower-cost, and economically more disadvantaged.

Second, for most variables the estimates are similar enough that the confidence intervals of the main method and the DD estimates overlap – see columns (5) and (8), which indicate whether this holds. This is true for 10 of the 20 variables for the simple DD model and 16 of 20 variables in the DD model with trends; 17 of 20 variables match for at least one of the two methods.[73] Where the two DD estimates differ, it appears to be because of non-parallel pre-trends for treatment and controls – something we have verified in plots of the raw data (not shown). Differential pre-trends were not a major problem with our main enrollment analysis and for the risk score and cost outcomes shown in Figure 6. However, they are a larger concern for other variables – especially age, income, and duration enrolled. This suggests a challenge with the DD approach and a reason we prefer the main method (a simple comparison of actives vs. passives) for our main targeting estimates.

[72] Note that the DD estimates for risk score and costs reported in body text Figure 6 are based on the simple DD model in equation (17).

[73] In the few cases where CIs do not overlap, the estimates are qualitatively similar. For instance, the share with any positive spending increases by 0.060 in the main method vs. 0.084 in the simple DD method. The average income increases 1.56% of FPL in the main method vs. 5.44% of FPL using the simple DD.

C.5 Targeting during 2007 Auto-Enrollment of Uncompensated Care Pool

As discussed in Section 3.2, the nature of auto-enrollment was different in early 2007 when the state auto-converted individuals from its Uncompensated Care Pool (UCP), leading to a major spike in passive enrollment (see Appendix Figure A.2). Many of these individuals had enrolled in the UCP months beforehand, which is quite different than the very short lags (max of a few weeks) between application and auto-enrollment in our main period. A natural question is whether this different nature of the auto-enrollment policy led to different targeting properties.

Table A.6 replicates the comparison of active vs. passive enrollees (from Table 1 in the body text) using enrollees during the auto-conversion of the UCP, which occurs during December 2006 to February 2007 (or FY 2007, months 6-8). Columns (1)-(2) show active vs. passive enrollee mean characteristics/outcomes, and columns (3)-(4) report the active-passive differences and percent differences. (Standard errors are omitted, but all differences are significant at a $p < 0.001$ level.) The differences for our main sample are shown in columns (5)-(6), replicating what is shown in Table 1.

All of the active vs. passive differences go in the same direction as the differences for the main 2008-09 sample. Moreover, the magnitudes are similar across all variables. The UCP period passive enrollees may be slightly younger and healthier – for instance, there are 14.2% points more people age 19-34 (vs. 11.8% points more in the main sample) and passives' medical spending is 51% below actives (vs. 44% lower in the main sample) – but the qualitative patterns are not too different. We conclude that the targeting properties of auto-enrollment are robust to the somewhat different policy environment during 2007.

C.6 Medical Shocks for Active vs. Passive Enrollees

In the body text, we refer to the fact that passive enrollees experience meaningful rates of medical shocks, suggesting that they are likely to benefit from health insurance. This appendix presents the analysis underlying that statement.

Figure A.11 compares active vs. passive enrollees on their rates of experiencing (various measures of) medical shocks during their first year enrolled. The first bar shows the probability of any medical spending, for context. The next three bars show the probability of experiencing a *high-cost month*, defined as spending exceeding \$500, \$1000, or \$2000. These are large spending amounts relative to the very low incomes of the below-poverty CommCare enrollees (e.g., the 2009 poverty line for an individual was \$903 per month, and the average income of passive enrollees is 20% of poverty). The final bar shows the probability of an emergency inpatient hospital admission.

Across all of these measures, passive enrollees are less likely than active enrollees to experience the shock, but they still experience these shocks at meaningful rates – about 61-78% as frequently as active enrollees. This is comparable to the passive enrollees' risk scores, which are 63% as large as for actives (see Table 1). Our overall conclusion is that while passive enrollees are healthier on average, they do experience meaningful medical shocks for which insurance is likely to be valuable.

C.7 Evidence Against Choice Overload

In Section 5.3, we discussed evidence that individuals' passive behavior when asked to select a health plan is unlikely to be due to choice overload. Recall that choice overload is the propensity to become passive or forgo making a decision in the face of "too many choices" (Iyengar and Kamenica, 2010). To test this, we examine how the rate of passive behavior varies with the *number of plan choices* available to different enrollees across areas and over time.

Panel A of Table A.7 reports the passive rate during 2008-09 across areas of the state that have different number of available plans. This variation arises from selective insurer entry across areas. There is essentially no cross-sectional relationship between choice set size and the passive rate, which varies from 33-35%. Notably, the passive enrollment rate is 33.9% even in areas with just a *single plan* available. In these areas, the requirement to "choose" a plan is a pure ordeal, as the state gains no information from this step.

Panel B of Table A.7 shows a panel version of this test, running a simple DD regression to test whether area-level *changes* in the number of available plans (due to insurer entry/exit between 2008 and 2009) lead to changes in the passive rate. The regression specification is:

$$PassiveRate_{a,t} = \alpha_a + \beta_t + \gamma \cdot \Delta NPlans_a * 1\{t \geq 2009\} + \epsilon_{a,t} \tag{20}$$

where a are "service areas" (the level at which insurer entry occurs) and t are months (in the 2008-09 sample period). The coefficient of interest is γ, which is identified off of 6 service areas (out of 38 total) that experience a change in number of plans between 2008-09 – five areas with a one-plan increase of and one area with one-plan decrease. Adding an extra plan leads to a small and insignificant change in the passive rate (with a slightly negative point estimate) – again suggesting that passive enrollment is not driven by choice overload.

C.8 Analysis of Factors Related to Inattention or Misunderstanding

Table A.8 reports analysis of how the passive enrollment rate varies by characteristics plausibly related to inattention or misunderstanding of program rules, as discussed in body text Section 5.3. We show analysis for three types of variables, which we discuss in turn. For each variable, the table shows the number and share of enrollees each subgroup in columns (1)-(2) and the subgroup's raw passive enrollment rate in column (3). Columns (4)-(5) show adjusted passive enrollment rates, after controlling for the covariates indicated in the bottom panel of the table.

Address Mismatch This analysis tests the idea that people may be passive because of address errors that may lead to not receiving the approval letter with instructions on how to actively enroll. Although we cannot observe address errors directly, we construct a variable proxying for possible "address mismatches." We do so by asking whether we observe *different zipcodes* (the most detailed variable we observe) in CommCare's enrollment file (based on the address used in administrative mailings) vs. on the enrollee's first observed medical claim (which is submitted by the medical provider, often based on paperwork filled out at a visit). Starting from our main sample, we further limit the sample to enrollees with a medical claim observed during their first 6 months enrolled.[74]

The top panel of Table A.8 shows that address mismatches by this measure are surprisingly common, occurring for 36% of enrollees. Moreover, mismatch is predictive of passivity. The passive rate is 28% for enrollees with mismatched addresses vs. 25% for enrollees with matching zipcodes, a 3% point (or 13%) difference. Moreover, this pattern is robust to controlling for demographics (column 4) and for health factors and the timing of the first observed claim (column 5).

Immigration Status The next panel shows passive rates for immigrant vs. non-immigrant enrollees. Recall that our main sample in the paper *excluded* immigrants because of an eligibility change for them that occurred in early FY 2010, shortly after the suspension of auto-enrollment. For this analysis, we add immigrants back to the main sample of below-poverty new enrollees during the 2008-09 period. (The sample size is therefore larger than in our main analysis, and than in the first panel where we limited to people with a claim observed in the first 6 months.) Immigrant enrollees represent 12% of new enrollees, and they are more likely to passively enroll. Their passive rate is 41%, about 7% points (or 21%) higher than for non-immigrants (34% rate). This pattern is robust to controlling for demographics and health variables.

Cross-Program Transitions This analysis tests the idea that there may be greater confusion or inattentiveness for enrollees who transition between programs. The first analysis focuses on people who transitioned from the state's pre-2007 Uncompensated Care Pool (UCP) to CommCare in early 2007. The sample is limited to new enrollees during the beginning of FY 2007 (December 2006 to February 2007) when this UCP group was auto-converted to CommCare eligibility. UCP transitioners represent a high share of enrollees (77%) during this period. Further, UCP transitioners are much more likely to be passive – with a 60% passive rate vs. a 40% rate for other new enrollees at the same time.

The second type of cross-program transition are enrollees shifting from Medicaid to CommCare. We use a variable in the data that captures whether a new CommCare enrollee was enrolled in Medicaid during the prior 12 months. This may slightly overstate the share of people *directly* transitioning between programs (as opposed to joining after a gap), but it is a decent proxy for transitions. This analysis returns to the main sample for the paper (below-poverty new enrollees during 2008-09), so people transitioning from the UCP are not relevant for this analysis. About 35% of new enrollees are transitioning from Medicaid to CommCare, and this group has a higher passive enrollment rate of 39% versus 31% for all other new enrollees. Part of this gap is driven by a very high passive enrollment rate (49%) for people transitioning out of Medicaid at age 19. But columns (4)-(5) show that this pattern is robust to controlling for age-sex cells (including a dummy for age 19, interacted with gender).

[74]This ensures that mismatches are not simply driven by failure to observe a claim and reduces the chance that mismatches are driven by enrollees moving between their initial enrollment and first observed claim. The results are robust to using more or less stringent periods when the first claim must be observed.

D Duplication of Coverage Analysis (using APCD)

A question of interest is whether auto-enrollment leads to duplicate coverage for people get auto-enrolled in CommCare despite also having outside private insurance. To assess coverage duplication, we draw on information from the Massachusetts All-Payer Claims Database (APCD) (Mass. CHIA, 2014).[75] The APCD lets us observe coverage in CommCare as well as nearly all other health insurance plans in the state. The sole exception is traditional Medicare, which is unlikely to to be relevant for the under-65, non-disabled population in CommCare. (Additionally, anyone enrolled in Medicare should be ineligible for CommCare.) The APCD includes a synthetic ID that follows individuals across insurers, letting us observe duplicate coverage.

D.1 Data Construction Method

Using the APCD's member eligibility (ME) file, we construct an enrollment history dataset for people ever enrolled in CommCare that also includes their coverage history in other insurance. The data construction requires some care. Each record in the ME file describes a member's enrollment spell in a particular health plan, with variables describing the characteristics of the health plan (such as the plan's carrier), and the start- and end-dates of the spell. We use the variables "Insurance Type Code" (ME003) and "Special Coverage" (ME031) to define indicators for CommCare plans. Both variables include a category for CommCare enrollment; however, since they do not always coincide, we define our sample based on whether either variable indicates CommCare.

An additional challenge is that many records for BMC HealthNet (a large CommCare plan) enrollments have missing values for the end-date, specifically coded as "12/31/2099" or "12/31/2199." We find that these are often (in about 98% of cases) accompanied by another record with an identical start-date and a non-missing end-date. In these cases, we disregard the record with the missing end-date in the construction of our panel. In the remaining 2% of cases, we truncate the end-date to be 12/31/YYYY, where YYYY is the year of the report ("eligibility year", given by the variable ME004).

We validate the construction of this dataset by comparing it to the true counts in the administrative CommCare enrollment data. The numbers line up quite closely. The member-month counts in the APCD data match to within 3% the counts in the admin CommCare data for fiscal years 2009-2013 (10.7 million in the APCD compared to 10.4 million true CommCare member-months). Enrollment across plans and over time also line up quite closely. Figure A.12 shows that the flow of incoming enrollees into CommCare (either as a new or re-enrollee) matches quite well in the APCD and CommCare datasets.[76]

With this panel dataset in hand, we turn to enrollment spells in other (non-CommCare) plans in the APCD. We restrict the analysis to enrollment in private coverage only – which includes employer-

[75] We use the APCD version 3.0, which includes calendar years 2009-2013. The APCD, which is not linked to the CommCare data, was obtained under a separate data use agreement with Massachusetts' Center for Health Information and Analysis.

[76] Note that Figure A.12 includes all new and re-enrollees in all income groups, which is why the counts differ from what is shown in Figure 3 for 0-100% of poverty new enrollees only. We include both because we the APCD data on CommCare start only in January 2009, so we cannot tell an incoming enrollee had been enrolled prior to this.

based, individual market, and Medicare Advantage plans but excludes Medicaid plans. We do this for two reasons. First, Medicaid and CommCare used a unified eligibility and enrollment system, meaning that inappropriate duplicate coverage mechanically should not occur. Second, most Medicaid managed care plans also participate in CommCare, and we expect there may be some measurement error in labeling Medicaid vs. CommCare coverage. This would create the potential for false positives in measuring duplicate coverage. Therefore, we exclude Medicaid coverage and focus on duplication between CommCare and private insurance. We do not have an external dataset to validate the enrollment numbers for private coverage, so we take the spell descriptors in the APCD at face-value. We define dual enrollment as a month in which a CommCare member is also enrolled in non-CommCare private health insurance.

A limitation of the APCD is that we are unable to distinguish member income levels or whether the member actively selected their plan at enrollment, meaning we cannot directly measure the duplication rate in the target auto-enrollment population. We present two lines of evidence: the first is that overall duplication for CommCare enrollees is low, and follows patterns consistent with members gaining outside insurance as they are leaving CommCare. The second examines the *change* in the duplication rate for enrollees who join CommCare just before and after the suspension of auto-enrollment. Sharp changes in duplication rates around this time would suggest that the people who stop enrolling (passive enrollees) differ in their duplication rate.

D.2 Duplicate Coverage: Results

Overall, we find that the average duplicate coverage rate is low, around 3.1% of member-months over the 2009-2013 period observed in the APCD. Figure A.13 examines the rate of duplication over the course of an enrollment spell. Duplication rates are lowest at the start of the spell and rise slightly over time. Interestingly, the probability of duplicate coverage drops in the 15th and again through the 27th-30th months of enrollment spells (Panel A), which is consistent with the timing of CommCare's re-certification of eligibility. This suggests that re-certification catches and disenrolls some members with outside insurance. However, these changes are modest – suggesting, again, that duplication is relatively rare. Figure A.13B shows the duplication rate in months relative to the end of a member's CommCare spell. The probability of duplicate coverage is highest in the 1-3 months before the member leaves CommCare. This is consistent with members leaving due to acquiring outside insurance and there being a short overlap in some cases. Nonetheless, duplication rates are never high: even in the final month of enrollment, they are below 6%.

Figure A.14 examines whether duplication rates change when auto-enrollment stops at the beginning of 2010. The figure shows duplication rates for each monthly cohort entering CommCare (the x-axis), with duplication measured over enrollees' first (up to) 12 months in CommCare . The population entering CommCare before the suspension of auto-enrollment at the start of fiscal year 2010 contains both active and passive enrollees, while post-suspension enrollees consist entirely of active enrollees. Since we cannot observe income level in the APCD, these averages also include enrollees above poverty who are unaffected by the policy. We see that the average duplication rate *rises* slightly around the end of auto-enrollment. This suggests that, if anything, passive enrollees are *less* likely to have duplicate coverage than the remaining CommCare population. However, the differences are modest – suggesting again that duplication is relatively rare in all groups.

The pattern of reverses when we focus on the temporary reinstatement of auto-enrollment in the final three months of fiscal year 2010. Overall duplication rates among incoming CommCare enrollees spike to 5-6% during this period, suggesting elevated duplication rates for passive enrollees joining in this window. This stands in contrast to the evidence (based on the early 2010 change) that passive enrollees likely had *lower* duplication rates. There are two possible explanations for this discrepancy. First, it may be a coincidence. The duplication rate stays elevated for early-2011 entering cohorts despite auto-enrollment having ended, suggesting that other factors may explain the trends. Second, if not a coincidence, passive enrollees during the temporary reinstatement period may have differed from pre-2010 auto-enrollees. During 2010, a stock of "eligible but not enrolled" people had accumulated, many of whom had applied for coverage weeks or months beforehand. When this group was auto-enrolled in late 2010, a higher than usual share may have already obtained duplicate insurance. This suggests that the specifics of auto-enrollment may matter for how serious a concern duplicate coverage is.

E Uncompensated Care Estimates from Health Safety Net Claims

A key fiscal and social (positive) externality of providing formal health insurance in our model is reduced uncompensated care costs. In our main model (Section 2), we use a simple formula that maps (observed) insured costs to (counterfactual) uncompensated care costs, using parameters from prior research. In this appendix, we provide an alternate measure that uses data from Massachusetts' Health Safety Net (HSN) program, an official uncompensated care program run by the state. The HSN was established to help pay for medical costs of the remaining low-income (below 200% of poverty) uninsured after Romneycare's enactment in 2006. Unlike most other sources of uncompensated care financing, the HSN pays out based on *specific medical claims* submitted by providers when they care for the uninsured, and these claims are included in the state's APCD. This provides a unique source of claims data on uninsured medical care use in Massachusetts that we leverage for our estimates.

Unfortunately, the data have several key limitations, which require us to make assumptions in our estimates – and also led us to prefer the simple formula for our main estimates. First, the data only include reliable information on billed "charges," not true payments by the HSN program. We can tell this is the case because the total charges are substantially higher than the paid amounts reported in the HSN program's (publicly available) annual reports. To convert charges to uncompensated care costs (C_i^{UC} in our model), we assume that costs are a fixed multiple of observed charges, applying a cost-to-charge ratio derived from HSN annual reports.

Second, the HSN claims data are only available in the APCD starting in 2013.[77] We calculate C_i^{UC} estimates for 2013 and discount them back to 2010 (the year auto-enrollment is canceled) based on overall growth in HSN spending, which we can observe in the program's annual reports.

Third, HSN payments are limited to care delivered in participating hospitals and community health centers. They therefore represent a subset of total uncompensated care costs, which will tend to make our estimates using this method somewhat conservative.

Finally, we cannot directly estimate C_i^{UC} for actual active vs. passive CommCare enrollees – both because active/passive flag is not observed in the APCD and because the HSN data are not available until 2013. Instead, we assume C_i^{UC} is constant within demographic (age-sex) cells.[78] We sum up total HSN-paid costs in a cell and divide by the total number of low-income Massachusetts uninsured individuals in the cell, estimated from the 2013 American Community Survey data (see Appendix A.1). This procedure lets us calculate HSN costs per uninsured person-month within an age-sex cell (g), or $\overline{C}_g^{UC}$. We then impute this to enrollees in our CommCare data using the appropriate age-sex cell for each person.

Uncompensated Care Estimates We use this HSN-based estimate of uncompensated care in a robustness check for our main targeting results, shown in in row (6) of Appendix Table A.10. Underlying the statistics shown there is an estimate that C^{UC} equal \$173 per month uninsured for active enrollees and \$144 per month for passive enrollees. By comparison, our baseline method (using the simple

[77] There is some limited data in 2012, but this appears to be incomplete.

[78] Ideally, we would let C_i^{UC} vary with richer health variables, but we are constrained by the (very limited) set of diagnoses available in the HSN claims.

formula based on past evidence) estimates C^{UC} as \$242 per month for active and \$135 for passive enrollees. Thus, the HSN-based estimates for passive enrollees are slightly (7%) higher than in our baseline estimates, and they are quite a bit (29%) lower for active enrollees. Our sense is that this is likely to reflect unmeasured health differences – recall that our imputation from the HSN data is based only on age-sex cells – but it may also reflect the fact that passive enrollees obtain a larger share of their care from charity care sources (see Appendix C.3). Nonetheless, it is reassuring that these estimates are in the same ballpark, despite the very different methods. We find that our main targeting results – that passive enrollees have lower value but higher value-cost ratios – are robust to using this alternate HSN-based measure for C^{UC}.

F Enrollee Value of Insurance (Demand) Model

Our model in Section 2 requires estimates of the value of insurance to enrollees. To implement this, we draw on premium variation used in prior work by Finkelstein, Hendren and Shepard (2019b) (hereafter, "FHS") to estimate enrollee willingness to pay (WTP), or demand, for insurance in the Massachusetts CommCare program. This appendix explains the method we use for this demand estimation, which builds on and extends the method of FHS.[79]

F.1 Subsidy Variation and Enrollment RD Estimates

CommCare features three income thresholds at which there are discrete changes in subsidies – and therefore subsidized enrollee premiums for insurance – which we use to estimate points on a demand curve. Panel A of Appendix Figure A.15 shows this premium variation, plotting the income as a percent of the federal poverty level (FPL) versus the enrollee premium for the cheapest plan during the FY 2009-2011 period we analyze. Subsidies were set so that enrollee premiums for the lowest-price plan (which FHS call "P_L") always equaled a specified "affordable amount," which varied by income in discrete bins. During the period we study, this amount was: (1) \$0 for enrollees with incomes below 150% of FPL, (2) \$39 per month for enrollees with incomes from 150-200% of FPL, (3) \$77 per month for incomes 200-250% of FPL, and (4) \$116 per month for incomes 250-300% of FPL. This subsidy structure implies discrete jumps in P_L of \$38-39 per month at 150%, 200%, and 250% of FPL, as shown.[80]

FHS's basic strategy, which we follow, is to use a regression discontinuity (RD) design to estimate how total market enrollment changes in response to these discrete changes in the cheapest plan's premium. This generates points on a demand curve that can be used for further analysis. We estimate a simple linear RD in which the slope and intercept are allowed to vary on each side of each threshold. We run the following regression across income bins (b) collapsed at the 2% of FPL level:

$$Enr_b = \alpha_{s(b)} + \beta_{s(b)} Inc_b + \epsilon_b \tag{21}$$

where Enr_b is market enrollment in income bin b, Inc_b is income (as a % of FPL) at the midpoint of the bin, and $s(b)$ is the income segment on which bin b lies (either 135-150%, 150-200%, 200-250%, or 250-300% FPL). All regressions are run on collapsed bin-level data. Following FHS, the income range starts at 135% of FPL because of an eligibility change (for low-income parents) at 133% of FPL. Above 133%, program eligibility is relatively constant – and importantly, does not change meaningfully at the RD thresholds.[81]

[79] Portions of the following writeup closely follow the description in Finkelstein, Hendren and Shepard (2019b).

[80] The market also includes higher-price plans, with premiums on average \$24 per month higher than the cheapest plan (with a 10th and 90th percentile of \$10-36 higher). However, for our demand estimates, we use only the premium of the *cheapest* plan (P_L), as this is most likely to matter for the (extensive margin) demand for insurance vs. uninsurance. FHS show that this assumption that the cheapest plan's price is what matters holds exactly in a vertical model of insurance demand, in which plans are clearly ranked on quality and price. In a richer model, they show that using the price of the cheapest plan for demand generates a *lower bound* on the demand curve for insurance (see Appendix E of their paper).

[81] The one minor exception is at 200% of FPL, above which pregnant women and HIV-positive people lose Medicaid

A key assumption for the RD design's validity is that enrollees do not strategically manipulate their incomes around thresholds to try to get a lower premium. FHS argue both that this strategic manipulation is institutionally unlikely and that there is little evidence of manipulation occurring in practice.[82]

Panel B of Appendix Figure A.15 shows results of this RD analysis for CommCare enrollment during 2009-2011, the relevant period for our study.[83] The graph shows average monthly enrollment by 5% of FPL income bins, along with linear RD best-fit lines and RD estimates based on (21). Enrollment falls sharply at each of the thresholds where premiums rise by $38-39 per month – falling by 23-32% at each threshold. The main RD estimates are visually clear and highly statistically significant.

F.2 Incorporating Heterogeneity and Translating RDs to Demand Estimates

For our model, we are interested in not just the overall demand for insurance but in how demand *varies* across different types of people. This heterogeneity is important for estimating targeting implications of increasing take-up among different groups. To incorporate heterogeneity, we adapt the FHS method in several ways. Our approach follows three steps.

1) Estimate RDs by Age-Sex-Risk Cells: First, we use our micro-data to estimate enrollment RDs separately by cells of $g = \{\text{age, sex, risk score}\}$. For age, we use nine bins in roughly five year intervals (19-24, 25-29, ..., 55-59, 60+). For risk score, we use quintiles of the HCC risk score, with an additional bin splitting out the top 5% highest-risk enrollees. We define risk score quantiles within each age-sex group to avoid generating very small cells. In each cell g, we collapse the data to enrollment counts by income bin level and run the RD regression in (21). We use the regression function to predict enrollment just to the left and right of each RD threshold $k \in \{150\%, 200\%, 250\%\}$, or $\hat{Enr}_g^{below(k)}$ and $\hat{Enr}_g^{above(k)}$. This then implies a percent change in enrollment at each cutoff, or:

$$dEnr_g^k \equiv (\hat{Enr}_g^{above(k)} / \hat{Enr}_g^{below(k)}) - 1 \tag{22}$$

2) Construct Implied Demand Points: Second, given these RD estimates, we generate implied points on an insurance demand curve for each g cell. To fix notation, let $s \in [0, 1]$ index people by declining WTP, and let $D_g(s)$ be WTP for insurance at the sth percentile of the potential enrollee population

and become eligible for CommCare. In practice, these comprise a very small share of enrollees, and FHS show that their estimates are robust to excluding the 200% of FPL discontinuity. To the extent that this change creates bias, our analysis will tend to *understate* the RD enrollment decline at 200% of FPL, as the eligible population grows slightly above this threshold.

[82]It is institutionally unlikely because enrollee report their monthly *dollar* income on eligibility forms, which then gets translated into income as a % of FPL (the running variable) using a formula that is not salient to enrollees. FHS further show that there is little evidence of spikes in the enrollee distribution just to the left of RD thresholds (and "holes" just to the right), which one would expect under strategic manipulation.

[83]We cannot use this strategy before 2009, since this is the first year when the continuous income measure is available. Our study period is slightly different than FHS, who study the full 2009-2013 period and also conduct an in-depth analysis of 2011.

in cell g. At any price P_L, the share who buy insurance is $s_g(P_L) = \{s : D_g(s) = P_L\}$ – i.e., the share with WTP $\geq P_L$ (or the inverse demand at P_L). From our RDs, we can infer how this share *changes* when P_L increases at each cutoff. For instance, at $k = 150\%$ FPL where P_L rises from \$0 to \$39, we infer that:

$$\frac{s_g(\$39)}{s_g(\$0)} = 1 + dEnr_g^{150\%} \tag{23}$$

To construct the level of (inverse) demand at each P_L, we start by normalizing demand to 1.0 for all g groups at $P_L = \$0$, i.e., $s_g(\$0) \equiv 1.0$.[84] We then iteratively construct inverse demand at higher prices (P_L = \$39, \$77, \$116) using the RD estimates:

$$\begin{aligned} s_g(\$39) &= s_g(\$0) \times \left(1 + dEnr_g^{150\%}\right) = 1.0 \times \left(1 + dEnr_g^{150\%}\right) \\ s_g(\$77) &= s_g(\$39) \times \left(1 + dEnr_g^{200\%}\right) \\ s_g(\$116) &= s_g(\$77) \times \left(1 + dEnr_g^{250\%}\right) \end{aligned} \tag{24}$$

Recall that $dEnr_g^k$ is generally negative because take-up declines at higher prices. However, in a small subset of cases (13 of the 324 g cell x cutoff RDs), we find positive raw estimates of $dEnr_g^k$, likely due to noise with small samples. In these cases, we enforce non-upward sloping demand by using $dEnr_g^k = 0$ in (24) instead of the raw estimate, and we also do not use these segments for any extrapolation (as discussed next).

3) Connect Points to Estimate Demand Curve: Finally, given these estimates of $s_g(P_L)$ at $P_L \in \{\$0, \$39, \$77, \$116\}$, we plot them in standard quantity-price space as $(s_g(P_L),\ P_L)$, where recall that $P_L = D_g(s_g(P_L))$ in our notation. We then connect these points linearly to generate an interpolated demand curve, $D_g(s)$ for each g over the range $s \in [s_g(\$116), 1.0]$. Note that $s_g(\$116)$ is typically around 0.25-0.50 (with a median of 0.34), so this captures the majority of the relevant population. However, for our targeting analysis we need to estimate average WTP across *all* individuals of a given g type – i.e., over the full $s \in [0, 1]$ range. We therefore linearly extrapolate the final valid demand segment leftward until we reach $s = 0$. This generates a full demand curve $D_g(s)$ for each g type.

Demand Results To illustrate the results of this exercise, Appendix Figure A.16 plots average demand curves, focusing just on the "interpolated" portion of demand (up to \$116 per month). Panel A shows the overall average demand curve, and Panels B-D show breakdowns by gender, age group, and medical risk score quantiles (defined, as noted above, within age-sex bins). For visibility, we show age and risk variation in more aggregated categories than are used in the underlying model, with the curves shown reflecting (sample-weighted) averages of the underlying curves. Demand varies in the expected

[84] This departs somewhat from FHS's method of calculating share insured at $P_L = \$0$ by taking observed CommCare enrollment and dividing by an estimated eligible population size using American Community Survey (ACS) data. Their approach would not be feasible within our age-sex-risk (g) cells, both because of limited ACS sample sizes in small cells and because risk scores are unobserved in the ACS. In practice FHS find that insurance take-up is 94% at $P_L = \$0$, so our normalization of $s_g(\$0) = 1.0$ is likely a reasonable approximation.

directions – with higher WTP curves for females (vs. males), older enrollees (vs. younger), and higher medical risk enrollees (vs. lower-risk) within age-sex groups.

The graphs also show that the interpolated parts of demand capture most but not all of the overall curves – typically from $s = 1.0$ down to $s = 0.25$-0.50 (though not as far for older and sicker groups). Our linear extrapolation strategy ensures that we use a full demand curve for each type. However, one might be concerned about the reliance of our estimates on this extrapolation. As a sensitivity analysis, we also consider enrollee value estimates based on the median ($s = 0.50$) and 75th percentile ($s = 0.75$) of declining WTP on each curve (see Appendix Table A.10, row (3)). These points are much less likely to be extrapolated, and when they are, the extrapolation is more limited. Because of the convexity of demand, these estimates are naturally lower than our baseline method using average WTP. However, our main directional targeting results for active vs. passive enrollees are similar whether we use average WTP or the 50th or 75th percentiles.

F.3 Projecting Demand Estimates onto Active/Passive Enrollees

The method so far has estimated insurance demand curves, $D_g(s)$, for a full set of age-sex-risk cells (g), following a modified version of the FHS method. Given demand curves, $D_g(s)$, we now use them to estimate WTP for active and passive enrollees in our below-poverty sample relevant for auto-enrollment. We set WTP for a given individual i to be the average demand for that person's g cell, i.e.,

$$V_i = E_s\left[D_{g(i)}(s)\right] \tag{25}$$

By projecting at the g-cell level, this method allows for WTP heterogeneity by observable factors that enter g – i.e., age, sex, and medical risk score. These are likely important drivers of demand and are factors on which passive enrollees differed from active (as we showed, passives are younger and healthier).

However, a key question is whether there is further *unobserved* WTP sorting between active vs. passive enrollee. One might expect that even conditional on age, sex, and risk, people who fail to actively enroll may have lower demand for insurance. To capture this possibility, we model three assumptions for unobserved sorting. Operationally, these determine the *range of s* over which we average $D_g(s)$ for active vs. passive enrollees in equation (25). The three assumptions we consider are:

1. **No unobserved sorting:** We average over the same range, $s \sim [0, 1]$, for both active and passive enrollees. Thus, WTP is *equal* for both groups within a given age-sex-risk cell.

2. **Perfect sorting:** We assume that within each g cell, actives comprise the highest 67% WTP types (i.e., $s_A \sim [0, 0.67]$), while passives comprise the lowest 33% (i.e., $s_P \sim [0.67, 1.0]$), where 33% is the share of passive enrollees in the data.

3. **Unobserved sorting "equal to" observed sorting (baseline model):** We assume that the probability an active enrollee has higher *unobserved* WTP type than a passive type is equal to the probability

they have higher *observed* WTP type. Specifically, we first calculate the probability that a random active enrollee is in a g cell with higher estimated WTP than a random passive enrollee. This would be 50% with zero sorting on observables and 100% with perfect sorting. Empirically, it is 56% – consistent with partial (but incomplete) sorting. We then set the averaging ranges of s so that the analogous probability is also 56% *within* each g cell based on a simple sorting model. This, as we show next, is consistent with $s_A \sim U[0, 0.96]$ for actives and $s \sim U[0.08, 1.00]$ for passives.

Unobserved Sorting Model Our model for unobserved sorting is the following. We assume that within each g cell, active enrollees are uniformly distributed over the range $s_A \sim U[0, 1 - 0.33\omega]$, while passives are uniformly distributed over $s_P \sim U[0.67\omega, 1.0]$. In this model, $\omega \in [0, 1]$ is a parameter that indexes the degree of sorting. If $\omega = 0$ (zero sorting), both s_A and $s_P \sim U[0, 1]$, and the model collapses to the "no unobserved sorting" case. If $\omega = 1$ (perfect sorting), the ranges are $s_A \sim U[0, 0.67]$ and $s_P \sim U[0.67, 1.0]$, and the model collapses to the "perfect sorting" case. Thus, this model nests cases #1 and #2 above, and it allows for intermediate sorting via $\omega \in (0, 1)$.

We next solve for the ω that implies that the probability that $s_A < s_P$ – the probability a random active type has higher WTP type (i.e., lower s) – equals 56% given the uniform distributions $s_A \sim U[0, 1 - 0.33\omega]$ and $s_P \sim U[0.67\omega, 1.0]$. To do so, we derive:

$$\begin{aligned}
Pr(s_A < s_P) &= 1 - Pr(s_P < s_A) \\
&= 1 - E_{s_A}\left[Pr(s_P < s_A \mid s_A)\right] \\
&= 1 - E_{s_A}\left[\max\left\{0, \tfrac{s_A - 0.67\omega}{1 - 0.67\omega}\right\}\right] \\
&= 1 - E_{s_A}\left[\tfrac{s_A - 0.67\omega}{1 - 0.67\omega} \mid s_A \geq 0.67\omega\right] Pr(s_A \geq 0.67\omega) \\
&= 1 - \left(\tfrac{\frac{1}{2}(0.67\omega + 1 - 0.33\omega) - 0.67\omega}{1 - 0.67\omega}\right)\left(\tfrac{1 - 0.33\omega - 0.67\omega}{1 - 0.33\omega}\right) \\
&= 1 - \frac{1}{2}\left(\tfrac{(1-\omega)^2}{(1 - 0.67\omega)(1 - 0.33\omega)}\right)
\end{aligned}$$

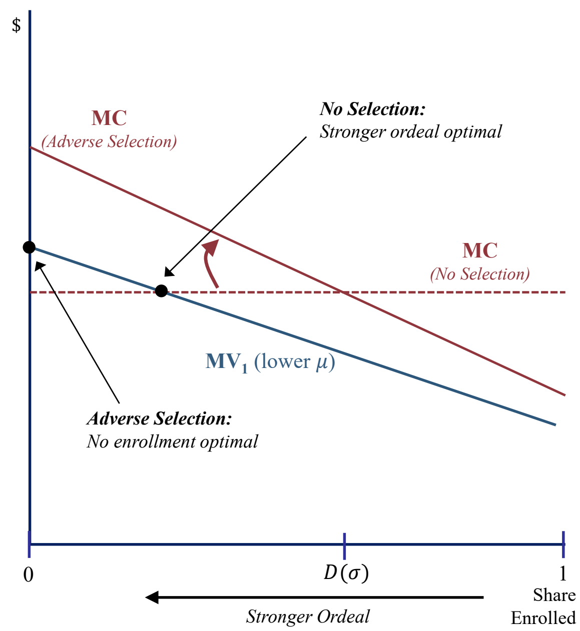
Solving (numerically) for the value of ω that makes this expression equal 0.56 yields $\omega = 0.12$. This in turn implies $s_A \sim U[0, 0.96]$ and $s_P \sim U[0.08, 1.0]$, just as noted above.[85]

[85] More precisely, we use 0.326 for the share passive and 0.674 for the share active, which yields $\omega = 0.117$ and $s_A \sim U[0, 0.962]$ and $s_P \sim U[0.079, 1.0]$.

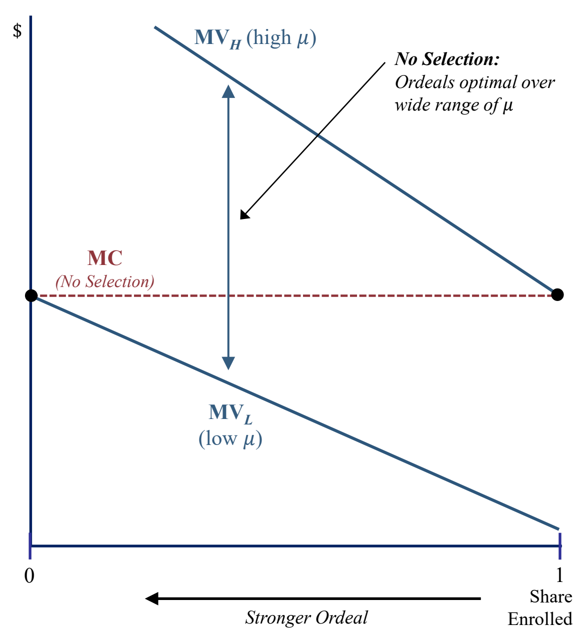
G Appendix Figures and Tables

Figure A.1: Optimal Universality with Adverse Selection: Additional Figures

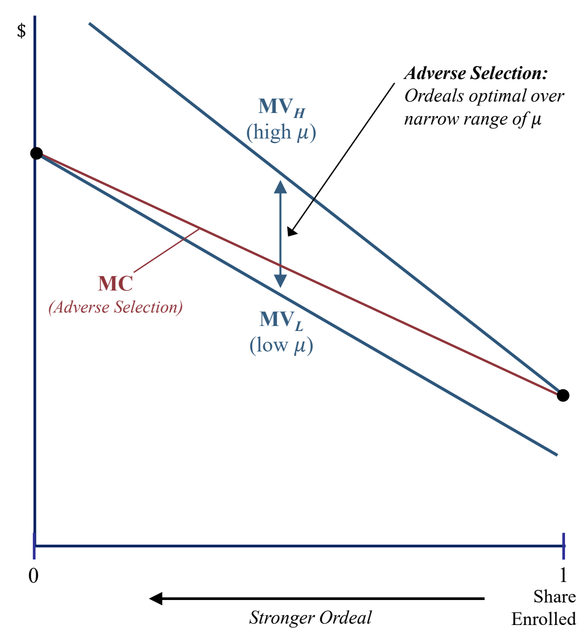
A. Optimal Universality: Lower Social Value Curve



B. Ordeals Optimality Range: No Selection

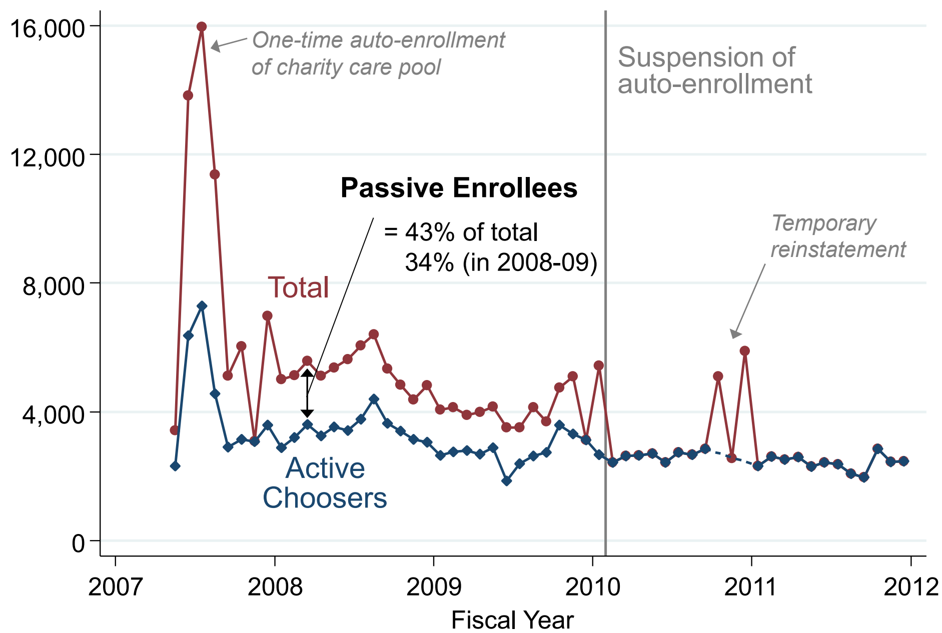


C. Ordeals Optimality Range: Adverse Selection



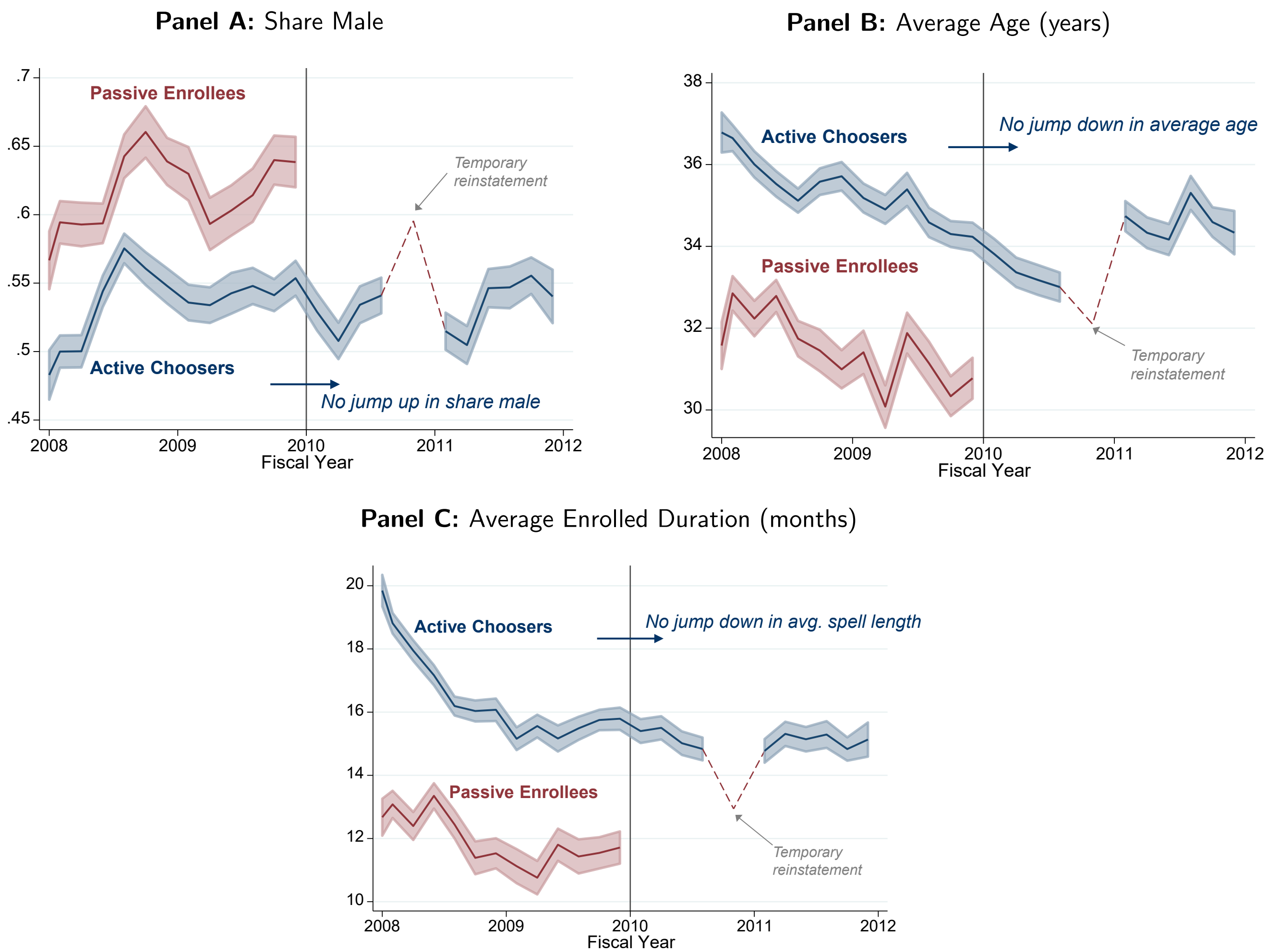
Note: The figure shows further analysis of how adverse selection leads to optimal universality. Panel A shows how a lower social value curve (MV) (via a lower social welfare weight, μ) leads to no enrollment being optimal, analogous to Figure 1C for a higher μ. Panels B-C illustrate the formal argument for why the range of social preferences μ over which ordeals yield targeting gains is *wider under no selection* (panel B) *than under adverse selection* (panel C).

Figure A.2: Count of New Enrollees per Month (0-100% of poverty)



Note: The graph shows counts of new enrollees per month into the CommCare market for the <100% of poverty group subject to the auto-enrollment policy. This graph shows the monthly raw data underlying the bimonthly averages shown in Figure 3 and used in our empirical analysis. The CommCare market starts in fiscal year 2007, and auto-enrollment is in place from 2007 to the end of 2009, plus a temporary reinstatement period at the end of 2010. The red series shows total new enrollment, and the blue series shows the count of "active choosers" who actively chose a plan when newly enrolling. The gap between these series represents the number of passive auto enrollees. The large spike in passive enrollment in 2007 comes from a one-time auto-enrollment of charity care pool enrollees (see discussion in Section 3.1). The dashed line for the blue series at the end of 2010 indicates that we lack data separating active vs. passive enrollees during this period.

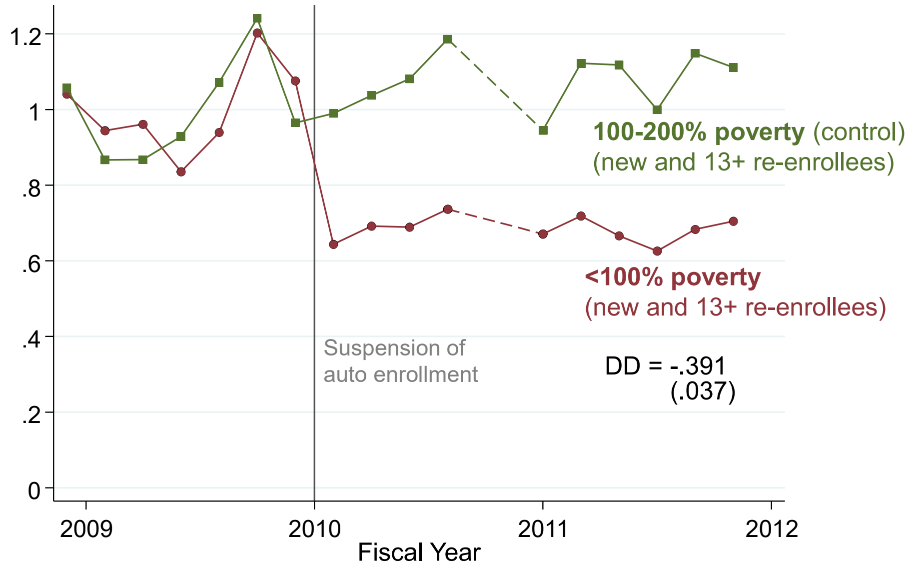
Figure A.3: Evidence against Purposely Passive: No Jumps in Active Enrollee Characteristics



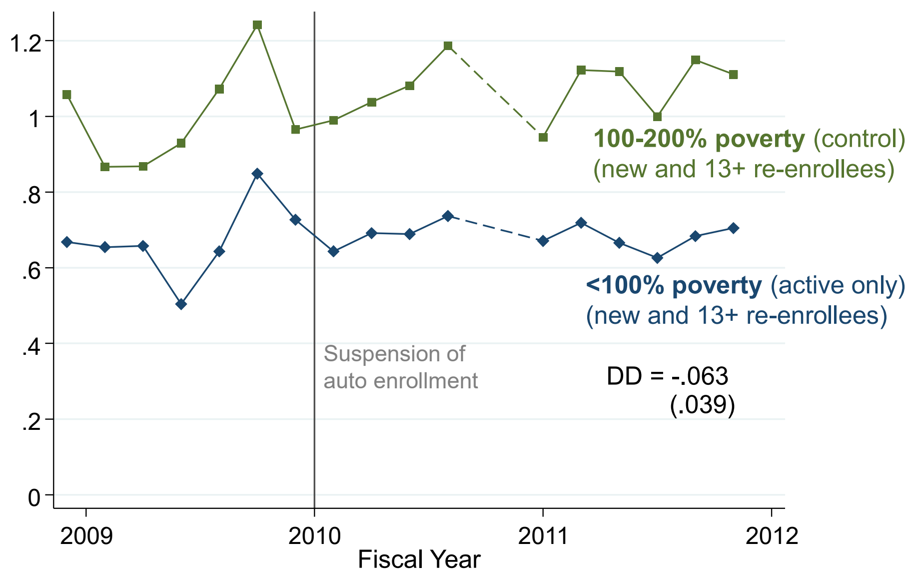
Note: The figure examines whether there are changes in the average characteristics of active new enrollees at the suspension of auto-enrollment (start of 2010), which could indicate the presence of "purposely passive" types (see Section 4.1 for a definition). If there were purposely passive types, we would expect a jump in the mean for active enrollees towards the mean for the passive enrollees, as some people switch from being passive to active without auto-enrollment. We see no evidence of this for three key characteristics: share male (panel A), average age (panel B), and average enrollment spell length (panel C). Along with the absence of increase in active new enrollment (see Figure 4B), this suggests that passive behavior is largely exogenous to the auto-enrollment policy. Note that the dashed red lines indicate the auto-enrollment temporary reinstatement period during which our data are missing the indicator for passive status, so the data point reflects the average of passive and active enrollees.

Figure A.4: Enrollment Impacts: New Enrollees plus Re-enrollees with 13+ Month Gap

Panel A: Total New + 13+ Month Re-enrollees per Month *(scaled, 1.0 = pre-period mean)*

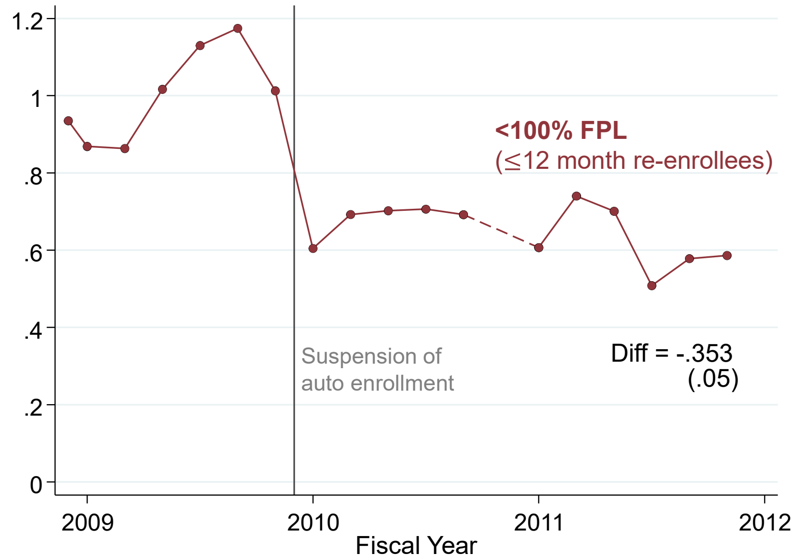


Panel B: Active New + 13+ Mon. Re-enrollees per Month *(scaled, 1.0 = pre-period mean)*



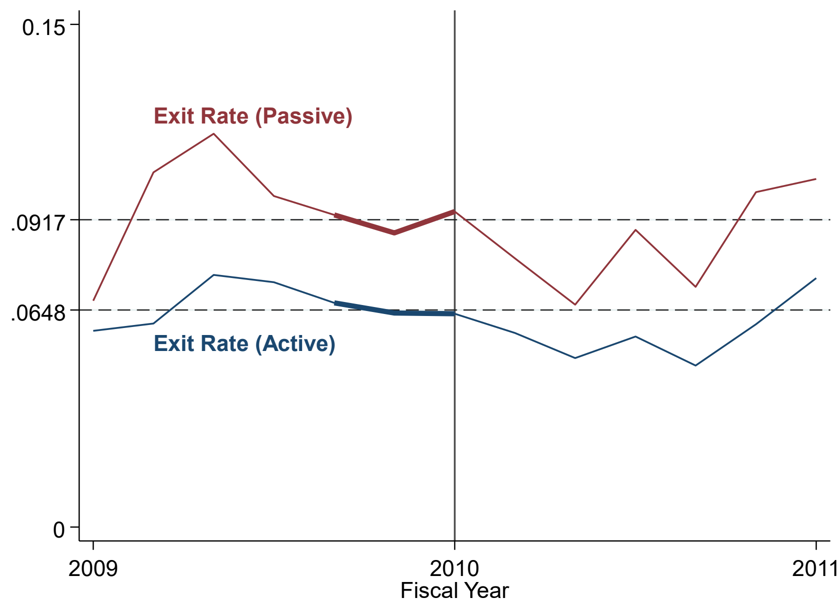
Note: The figure shows data on the scaled sum of new enrollment and 13+ month re-enrollment per month into the CommCare market and estimates of the difference-in-difference specification (13) for estimating the causal effect of the suspension of auto-enrollment at the start of 2010. Each panel compares trends for below-poverty enrollees (the treatment group subject to auto-enrollment pre-2010) versus 100-200% of poverty enrollees (the control group not auto enrolled). Each income group's series is rescaled by dividing by the group's pre-period mean new enrollment, which makes DD estimates interpretable as a percent change. Panel A shows that *total* new amd re-enrollment falls sharply (by 39.1%) for the treatment group at the start of 2010, consistent with a causal effect of the policy. Panel B shows that the number of *active* new and re-enrollees is flat through the policy change, consistent with there being few if any "purposely passive" types (see Section 4.1 for a definition). These results are qualitatively similar to the results reported in the main paper excluding all re-enrollees.

Figure A.5: Change in Enrollment of Short-Gap Re-enrollees (with $\leq$ 12 month gap)



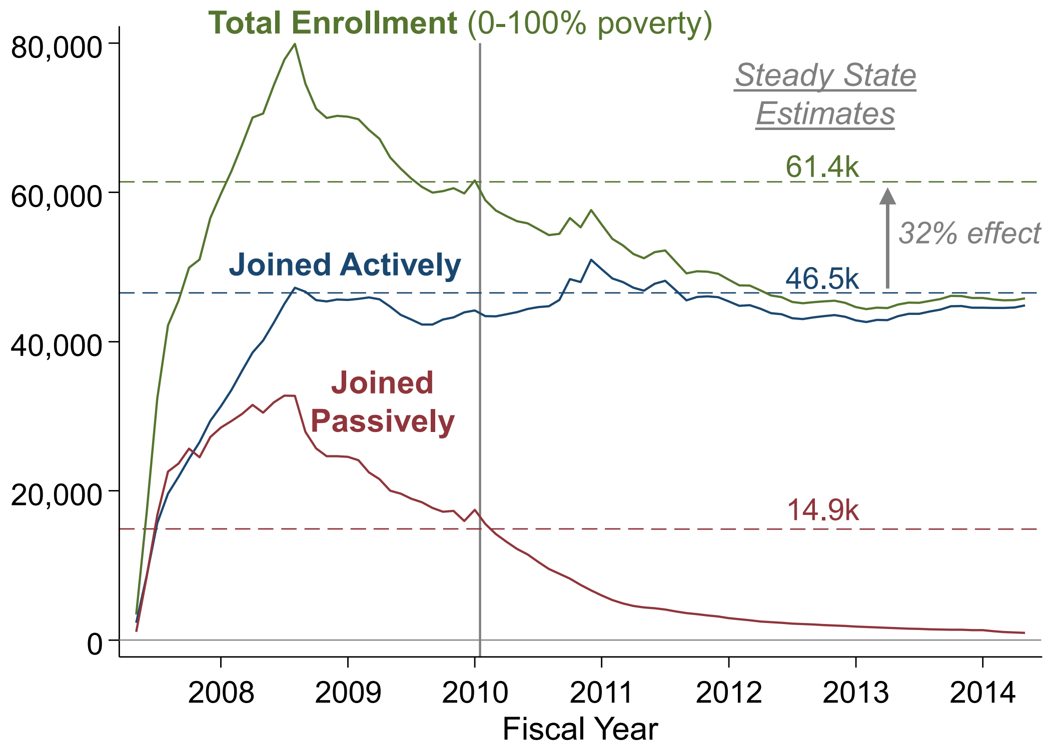
Note: The figure shows data on the scaled number of below-poverty $\leq$ 12 month re-enrollees per month, with the pre-period mean scaled to be 1.0. It shows estimates of the pre/post difference after the suspension of auto enrollment at the end of 2009. As noted in the text, we cannot implement a difference-in-difference analysis because the control group (enrollees with incomes > 100% of poverty) is also subject to the auto-enrollment policy.

Figure A.6: Exit Rate per Month for Active vs. Passive Enrollees



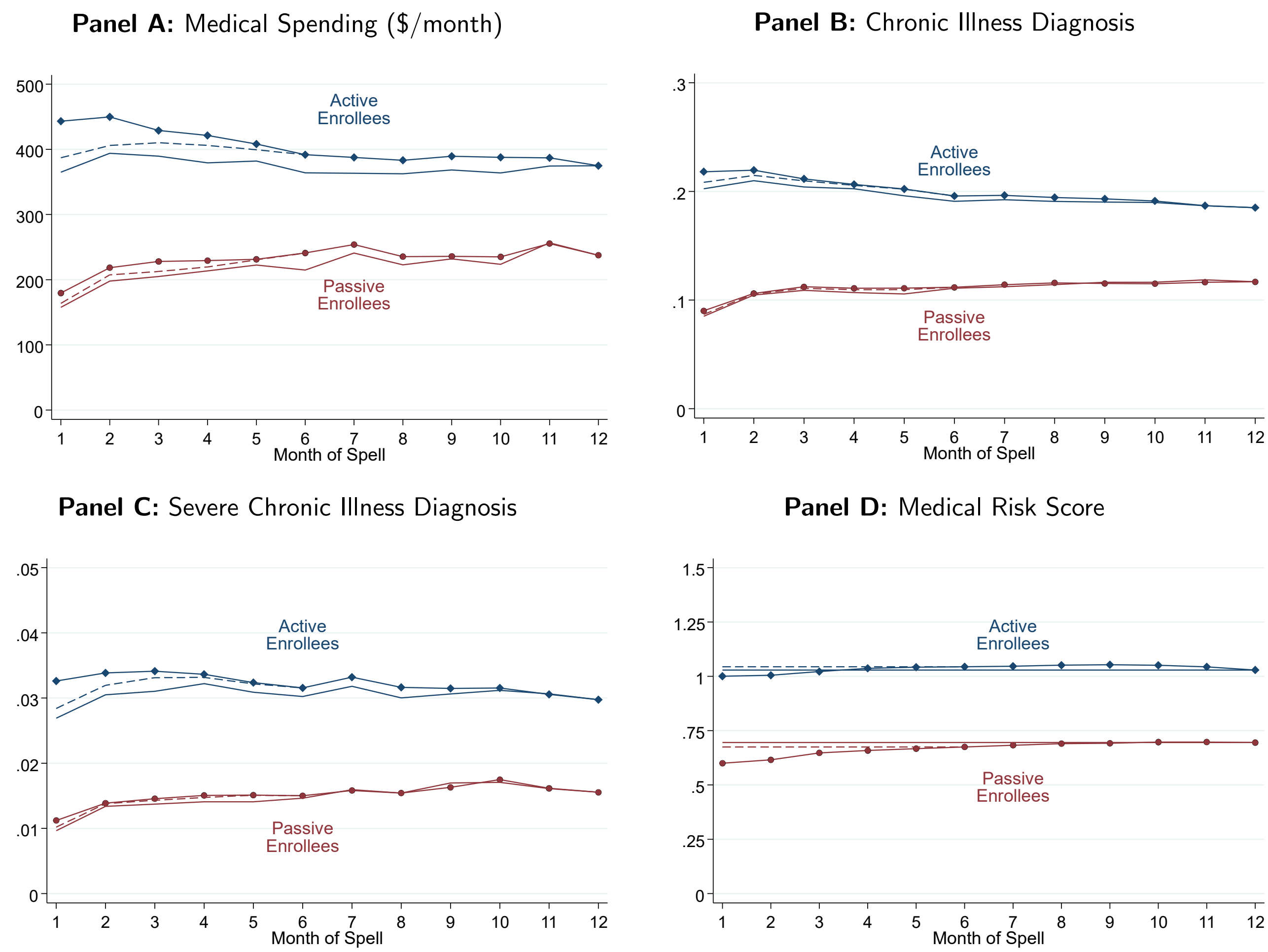
Note: The figure plots the exit rates in bi-monthly bins for active (blue) vs. passive (red) enrollees as an input to our steady state market size categories. The segments of each curve shown in bold are the samples used to estimate the average exit rates for each category, corresponding to the final six months auto-enrollment is in place.

Figure A.7: Total CommCare Enrollment (0-100% poverty), by Whether Joined Actively/Passively



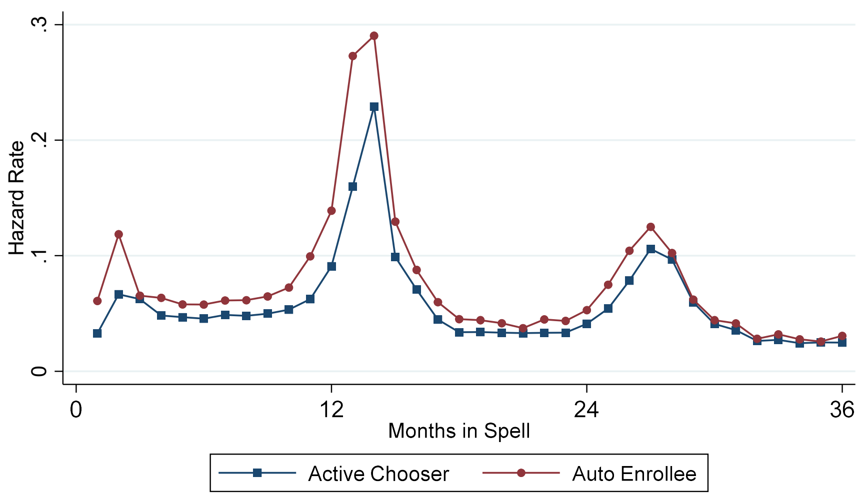
Note: The figure plots the stock of CommCare enrollment over time in the 0-100% of poverty group subject to auto-enrollment, both overall (green) and separately by whether each enrollee initially joined the market by actively choosing (blue) or passively (red). The enrollment counts are restricted to individuals during their first enrollment spell to be consistent with our empirical analysis of new enrollees (since rules differed for re-enrollment). The horizontal dashed lines indicate the steady-state enrollment estimates (for total, active, and passive enrollment) from the back-of-the-envelope calculation described in the text. The vertical gray line indicates the suspension of auto-enrollment. Because of incomplete data, we label all enrollees during the temporary reinstatement period (final three months of 2010) as active; this may account for the active enrollment uptick during this period. Overall, both the steady state calculation and analysis of the raw data indicate that passive enrollees represented about 32% of steady-state active enrollment.

Figure A.8: Active vs. Passive Health Differences Observed in Claims by Month of Enrollment Spell



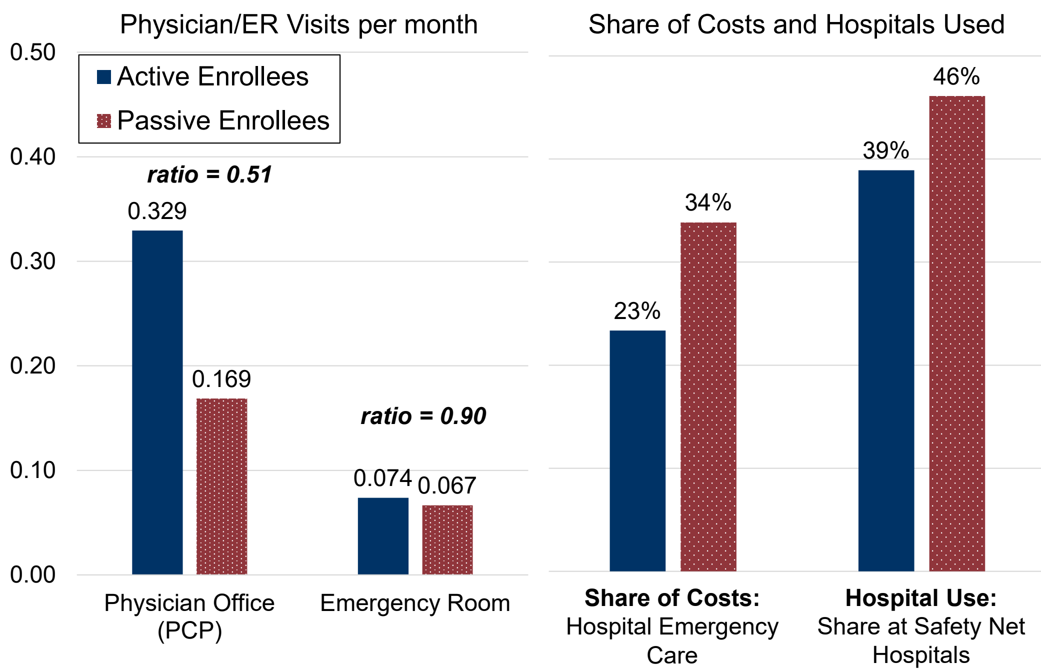
Note: The figures show the monthly rate of health measures separately for active vs. passive enrollees over the first 12 months of the enrollment spell. The solid line with markers plots the unconditional mean for all enrollees still enrolled as of that month of their enrollment (which is an unbalanced panel across points in the series, as enrollees drop out over time). The solid line without markers gives the mean in each month of the spell, only for the balanced panel of enrollees whose spell lasts ≥12 months; the dashed line without markers does the same for the balanced panel enrolled for ≥ 6 months. The four panels show: (A) medical spending ($ per month), (B) any chronic illness, (C) severe chronic illness, and (D) HCC medical risk score. See the note to Table 1 for further information on these variables.

Figure A.9: Exit Hazard Rate from CommCare: Active vs. Passive Enrollees



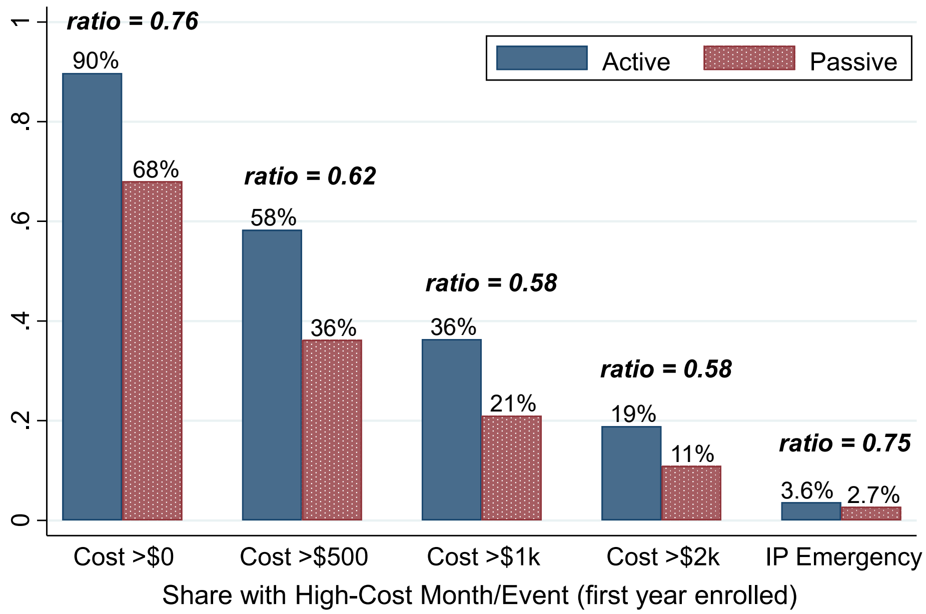
Note: To understand the reasons for passive enrollees' shorter enrollment durations, the graph shows the hazard rate of spell endings for active choosers (blue) vs. passive enrollees (red) by month since the start of their CommCare spell. The hazard rate is the share of enrollees whose spells end just after month t as a share of enrollees remaining through month t. Hazard rates are higher for passive enrollees in most months, but the gap is largest in two periods: (1) in months 1-2 of the enrollment spell, and (2) in months 12-14, which coincides with the timing of annual eligibility recertification. The former may be consistent with either mistaken enrollment (which are quickly rectified) or with passive enrollees needing coverage for very short periods. The latter is consistent with passive enrollees being less likely to respond to recertification paperwork – just as they failed to respond to the initial approval letter asking them to choose a plan.

Figure A.10: Utilization of Common Sources of Charity Care



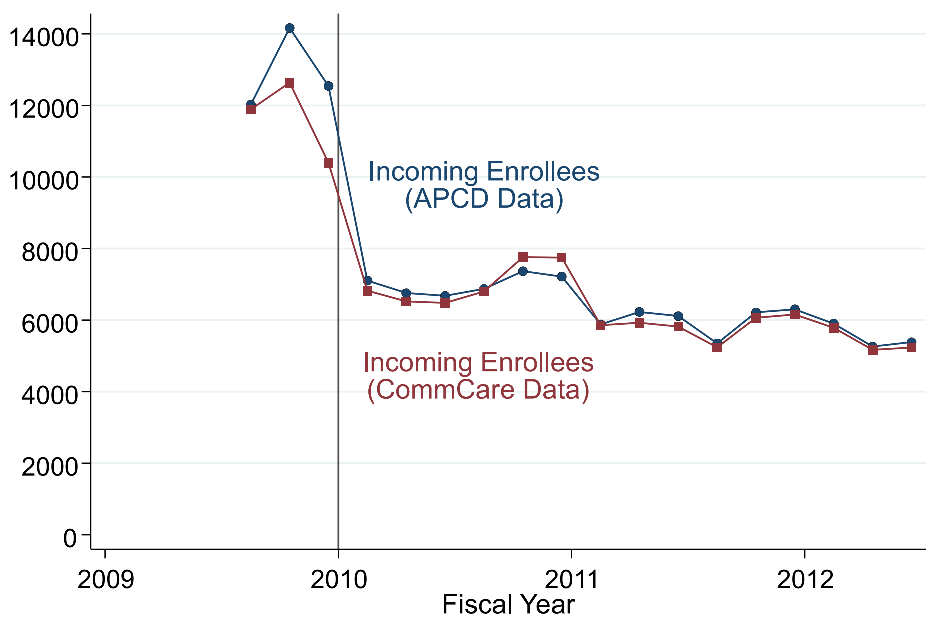
Note: The graph compares active and passive enrollees on several measures of use of common sources of charity care. The first two sets of bars show monthly rates of physician office visits (less likely to be obtained via charity care) versus emergency room visits (the classic source of charity care). The third bars show the share of enrollees' total costs that occur through emergency hospital care, including both the ER visit and any subsequent admission. The fourth bar shows the share of hospital use (weighted by cost) that occurs at safety net hospitals, a state-designated category based on having high public-payer and uninsured shares.

Figure A.11: Rates of Medical Shocks for Active vs. Passive Enrollees



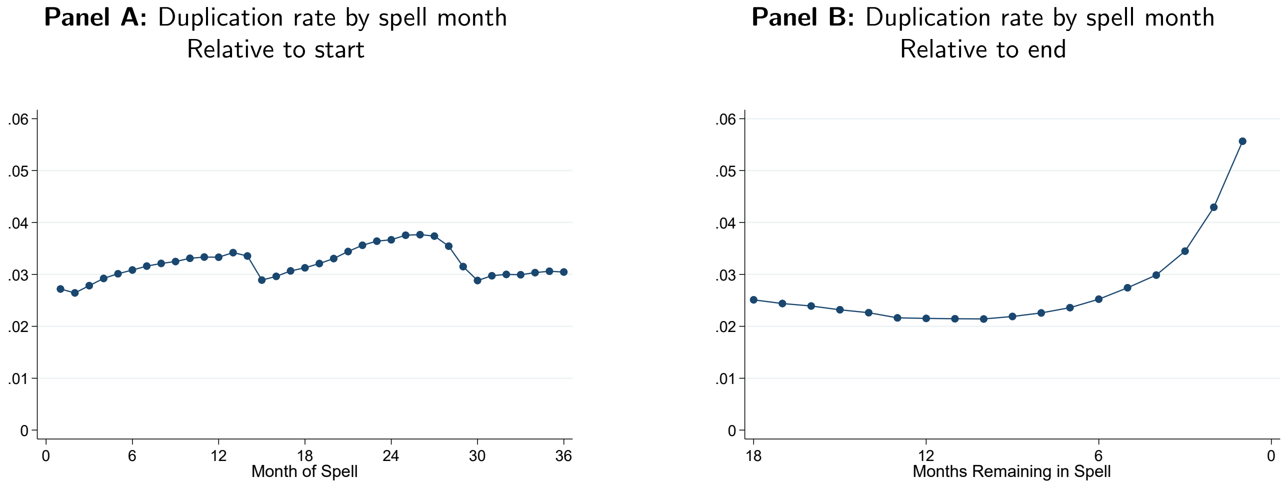
Note: The graph shows active and passive enrollees' rates of various expensive medical shocks during their first year enrolled, along with the risk ratio for passives / actives (shown above each set of vars). The first four bars are the likelihood of experiencing a single month with spending exceeding $0, $500, $1000 and $2000. The final bar is the probability of an emergency inpatient (IP) hospitalization, defined as a hospital admission that originated in the emergency department.

Figure A.12: Validation of APCD Data on CommCare: Incoming Enrollees per Month



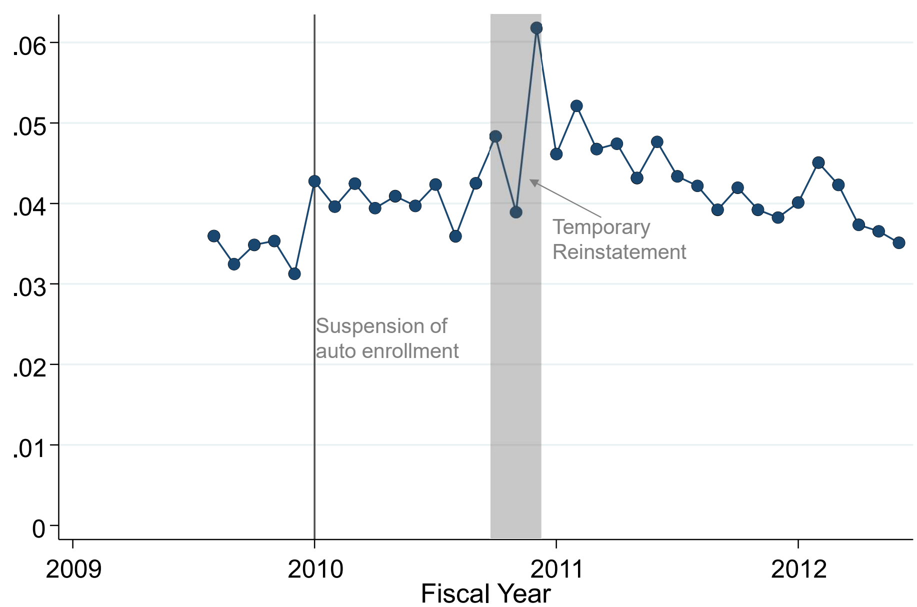
Note: The figure plots the number of incoming CommCare members in bi-monthly bins in both the administrative CommCare data (red squares) and in the APCD (blue circles). Incoming enrollees include new enrollees and re-enrollees, since we cannot distinguish these two in the APCD.

Figure A.13: CommCare Duplicate Coverage Rate over Enrollment Spells



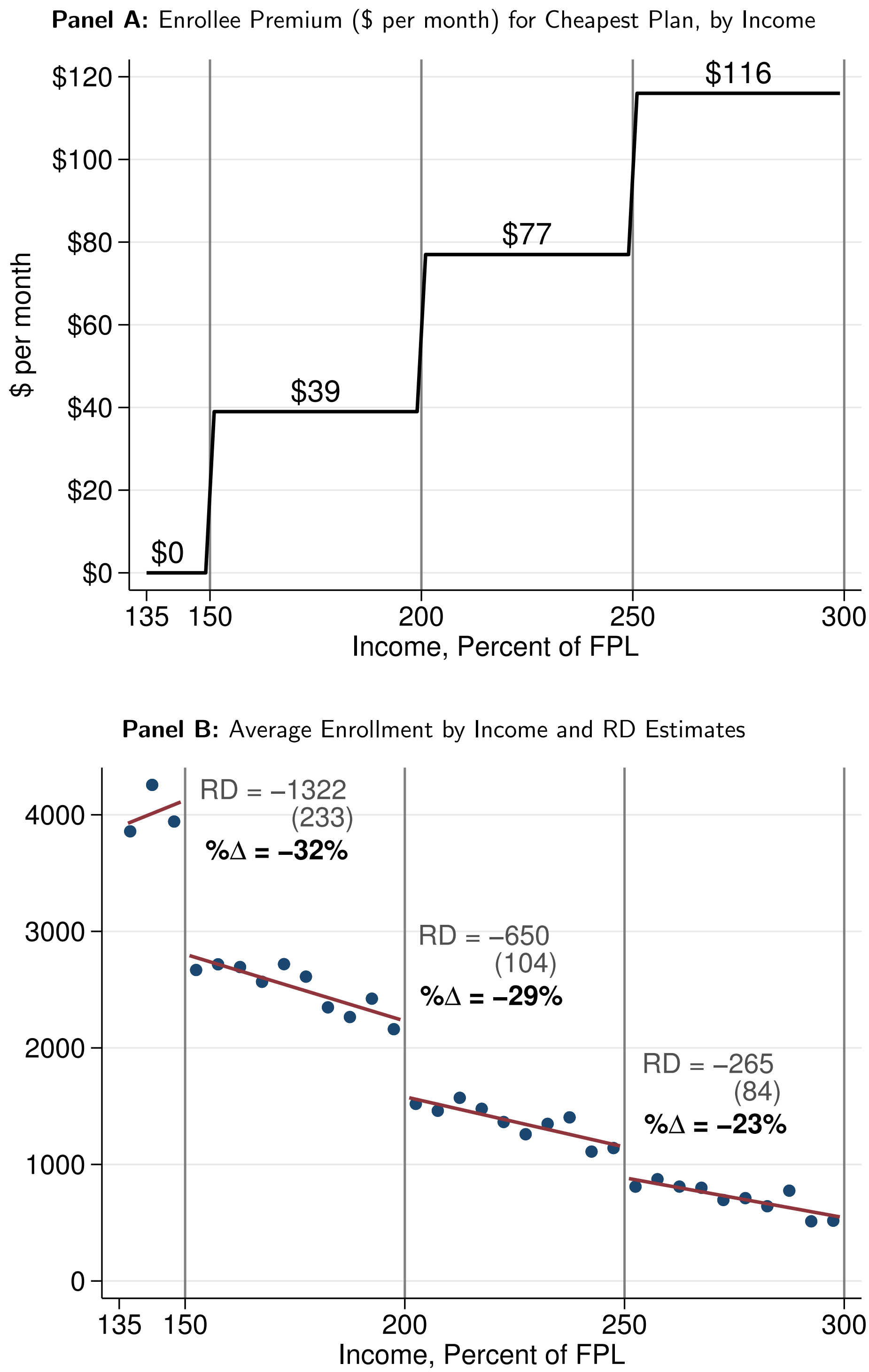
Note: The figures show the average rate of duplicate private insurance across all observed CommCare enrollment months in spells that begin in February 2009 and later, by the month of the spell (Panel A) and by the number of months remaining in the spell (Panel B). The APCD does not include enrollment prior to January 2009, so month of spell is not known for spells that start in or before January 2009.

Figure A.14: CommCare Duplication Rate by Date of Entry into Market



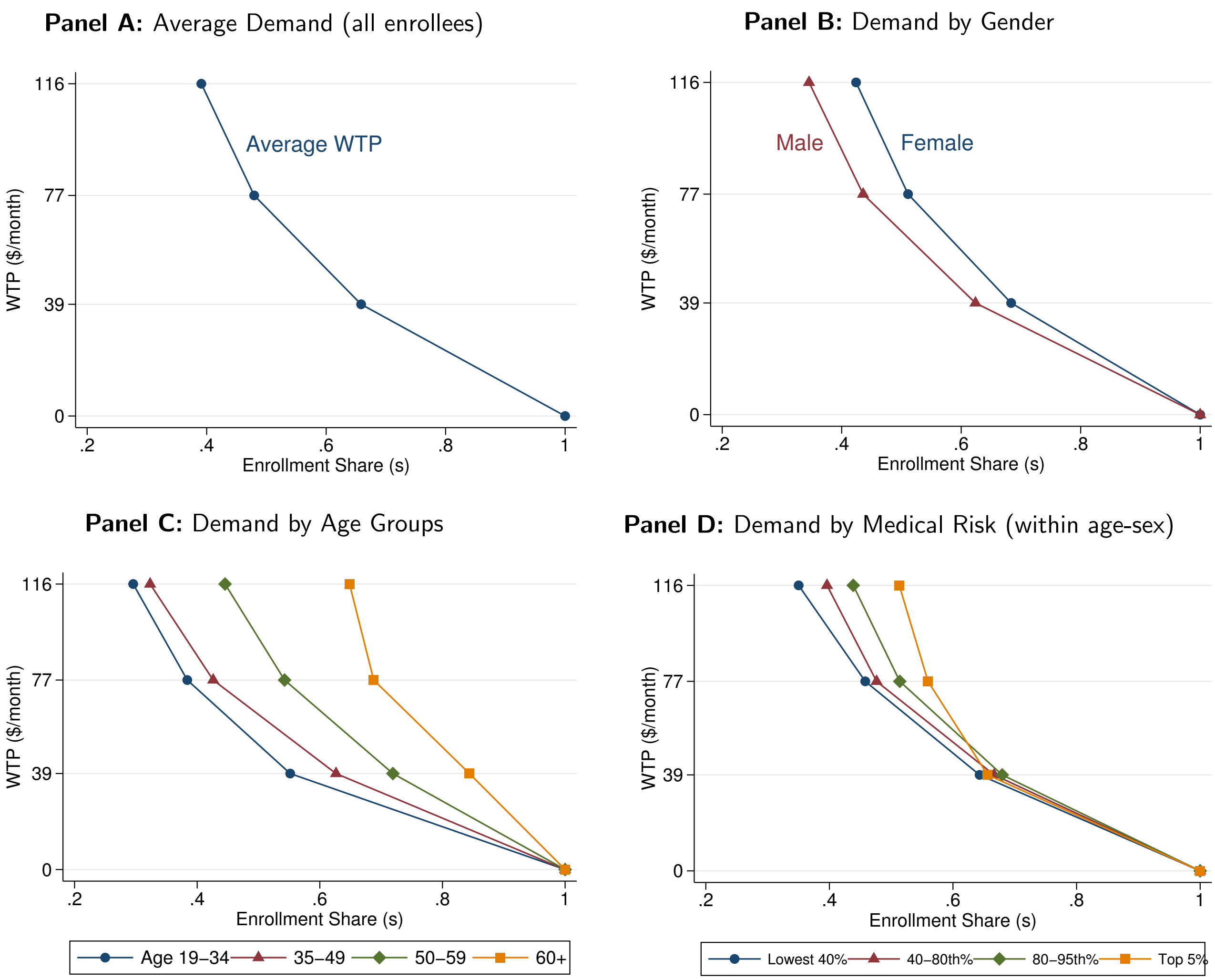
Note: The figure uses the APCD data to plot the duplication rates (over the first 12 months of the spell) for each monthly cohort of individuals entering CommCare (as new or re-enrollees, which we cannot distinguish). The first data point is February 2009 (or FY 2009m8), which is the earliest we can construct using the APCD data. The vertical line marks the time when auto-enrollment is suspended at the start of FY 2010, and the shaded bar indicates the temporary reinstatement of auto-enrollment.

Figure A.15: Demand Model: Premium Variation and Enrollment RDs



Note: Panel A plots enrollee premiums for the cheapest plan by income as a percent of the federal poverty level (FPL), noting the thresholds (150%, 200%, and 250% of FPL) where the amount increases discretely. Panel B shows our RD estimates of how average CommCare enrollment (pooled over 2009-2011) changes at these premium thresholds. The dots are averages in 5% of FPL bins, and the red lines show the best-fit line estimates. The on-graph text reports the estimated enrollment RD at each threshold, its standard error (in parentheses), and the implied percent change in enrollment.

Figure A.16: Demand Model: Summary of Estimated Demand Curves



Note: The figures show average estimated demand curves from our enrollee value model (see Appendix F). Each curve plots demand (or willingness-to-pay, WTP) curves with WTP in dollars per month on the y-axis and enrollment share on the x-axis, as in a standard demand curve. Panel A shows the overall average demand curve, while Panels B-D show demand variation by groups of gender, age, and medical risk (based on the HCC risk score). As discussed in text, medical risk score groups in Panel D are defined *within* 5-year age x gender bins, so they reflect high vs. low risk enrollees within these groups. In all cases, we show just the interpolated portion of demand, up to the maximum price observed in our data ($116 per month).

Table A.1: Sample Summary Statistics: Enrollment

	Initial Period with Auto Enr *2007* (1)	**Main Period with Auto Enr** *2008-2010m1* (2)	**No Auto Enr Period** *2010m2-2011* (3)
Total market enrollment (monthly avg.)			
0-100% FPL	37,059	73,304	69,706
100-200% FPL	4,799	55,014	62,762
New enrollees per month			
0-100% FPL	8,231	4,691	2,429
Share Active	51%	66%	100%
Share Passive	49%	34%	0%
100-200% FPL	1,823	3,959	1,996
Re-enrollees per month			
0-100% FPL: Total	144	1,181	1,529
Active	15	206	1,529
Passive	129	975	0
100-200% FPL	10	826	1,663

Note: The table shows CommCare enrollment patterns for the 0-100% of FPL treatment group and 100-200% of FPL control group over fiscal years 2007-2011. Column (1) shows statistics for the initial FY 2007 period with auto-enrollment in place, during which the exchange was just starting and there was a large auto-enrollment of Uncompensated Care Pool enrollees. Column (2) shows the main 2008-2010m1 period with auto-enrollment in place, and column (3) shows the 2010m2-2011 post-period when auto-enrollment was canceled. Column (3) excludes the three months at the end of 2010 when auto-enrollment was temporarily reinstated.

Table A.2: Sample Summary Statistics: Enrollee Attributes

A. Demographics (new enrollees, 0-100% FPL)	
Share Male	0.557
Age (mean)	34.2
Share Age 19-34	0.581
Share Age 35-54	0.308
Share Age 55+	0.111
Income (% of FPL)	26.4
B. Health Measures (new enrollees, 0-100% FPL)	
Any Chronic Illness	0.597
Risk Score (HCC)	0.926
C. Cost Measures (new enrollees, 0-100% FPL)	
Avg monthly spending	$369.3
Enrollment duration (months)	14.9

Note: The table reports means for new enrollees entering over the period 2008-11 in the 0-100% of FPL "treatment" group subject to auto-enrollment. All variables with the exception of enrollment duration are weighted by number of months enrolled (capped at 12 total).

Table A.3: Robustness: Alternate Specifications for Impact on New Enrollment

	Baseline: *(100-200% Controls)*	**Alternate Control Group (% of FPL)**				**Include Temp. Reinstatement Period**
		100-150%	**150-200%**	**100-300%**	**None**	
	(1)	(2)	(3)	(4)	(5)	(6)
Effect on Total New Enrollment	-0.326	-0.324	-0.329	-0.343	-0.399	-0.339
	(0.034)	(0.034)	(0.045)	(0.032)	(0.036)	(0.035)
Effect on Active New Enrollment	0.003	0.005	0.000	-0.014	-0.070	-
	(0.037)	(0.037)	(0.046)	(0.034)	(0.032)	

Note: The table shows robustness checks on the analysis of the impact on new enrollment of ending auto-enrollment at the start of FY 2010. The baseline results reported in body text Figure 4 are reported in column (1). These are based on the DD regression in equation (13), with outcome variables of total new enrollment (top row) and active new enrollment (bottom row). The baseline control group is enrollees with incomes 100-200% of FPL, and the sample excludes observations from the "temporary reinstatement" of auto-enrollment period in the final three months of 2010. Columns (2)-(5) start from the baseline specification, but uses alternate control groups: (2) 100-150% of FPL, (3) 150-200% of FPL, (4) 100-300% of FPL (all above-poverty enrollees in the program), and (5) no control group, which is a simple pre/post difference for the treatment group. Column (6) uses the baseline control group but includes in the sample the temporary reinstatement period, which is coded as a period with auto-enrollment in place (i.e., the dummy multiplying γ in equation (13) is turned off). For column (6), we cannot estimate impacts on active new enrollment because we cannot observe active vs. passive status in the data during the temporary reinstatement period.

Table A.4: Sensitivity of Health Targeting to Measurement Period

		Baseline (12 months)	Shorter Measurement Periods			Balanced Panel + Diff. Meas. Period		
		[replicates Table 1]	1 month	3 months	6 months	3 months	6 months	12 months
Share of Sample	Active	0.68	0.66	0.67	0.68	0.68	0.69	0.71
	Passive	0.32	0.34	0.33	0.32	0.32	0.31	0.29
Health Status Measures (means)								
A. Medical Spending	Active	$408	$443	$441	$425	$424	$400	$373
	Passive	$228	$182	$207	$218	$207	$212	$220
	%Diff	**-44%**	**-59%**	**-53%**	**-49%**	**-51%**	**-47%**	**-41%**
B. Chronic Illness	Active	0.641	0.218	0.399	0.524	0.402	0.537	0.668
	Passive	0.427	0.091	0.207	0.308	0.216	0.325	0.470
	%Diff	**-33%**	**-58%**	**-48%**	**-41%**	**-46%**	**-39%**	**-30%**
C. Severe Chronic Illness	Active	0.158	0.033	0.069	0.106	0.069	0.109	0.170
	Passive	0.081	0.011	0.029	0.049	0.030	0.052	0.092
	%Diff	**-49%**	**-66%**	**-58%**	**-54%**	**-56%**	**-52%**	**-46%**
D. HCC Risk Score	Active	1.011	0.979	0.987	1.002	1.000	1.022	1.008
	Passive	0.644	0.583	0.602	0.623	0.630	0.658	0.682
	%Diff	**-36%**	**-40%**	**-39%**	**-38%**	**-37%**	**-36%**	**-32%**

Note: The table shows the sensitivity of measured health for active vs. passive enrollees to changes in the measurement period. Each row of the first column replicates the baseline results in Table 1, Panel B: row (A) shows medical spending ($ per month), (B) any chronic illness, (C) severe chronic illness, and (D) HCC medical risk score. See the note to Table 1 for further information on these variables. Each of the subsequent columns reports estimated active and passive means and the percent difference in means for different measurement periods from the start of an enrollment spell (for $t = 1$, 3, 6, and 12 months) and balanced-panel restrictions. As in Table 1, estimates control for entry cohort fixed effects and are weighted averages by months enrolled (capped at t months for measurement period t). Columns 2-4 report estimates restricting to measurement periods of 1, 3, and 6 months, but do not restrict to a balanced panel of enrollees. Columns 5-7 report estimates using measurement periods of 3, 6, and 12 months, restricting to the balanced panel of enrollees who are enrolled for the entire measurement period in each case. The baseline results reported in Column 1 and in Table 1 use a measurement period of 12 months and do not restrict to a balanced panel.

Table A.5: Robustness: DD Estimates of Change in Enrollment Composition

Variable	Implied ΔAverage (from Table 1 ests.) Est. (1)	Std. Err. (2)	Simple DD Estimate Est. (3)	Std. Err. (4)	Overlap CIs? (5)	DD with Linear Trends Est. (6)	Std. Err. (7)	Overlap CIs? (8)
A. Age and Sex								
Average Age (years)	1.223	(0.028)	0.575	(0.225)		1.470	(0.414)	X
Age 19-34	-0.037	(0.001)	-0.013	(0.007)		-0.032	(0.016)	X
Age 35-54	0.022	(0.001)	0.005	(0.005)		0.001	(0.01)	X
Age 55+	0.016	(0.001)	0.008	(0.004)	X	0.031	(0.007)	X
Share Male	-0.027	(0.001)	-0.035	(0.006)	X	-0.061	(0.006)	
Male Age 19-34	-0.040	(0.001)	-0.033	(0.006)	X	-0.055	(0.01)	X
B. Health Status and Medical Spending								
Any Chronic Illness	0.069	(0.001)	0.081	(0.005)	X	0.077	(0.008)	X
Severe Chronic Illness	0.025	(0.001)	0.018	(0.003)	X	0.026	(0.004)	X
Risk Score (HCC)	0.119	(0.005)	0.146	(0.019)	X	0.082	(0.022)	X
Average Cost ($/month)	58.778	(2.08)	57.610	(7.25)	X	57.209	(17.446)	X
Any Spending (>$0)	0.060	(0.001)	0.084	(0.005)		0.091	(0.006)	
C. Income & Area Disadvantage								
Income / Poverty Line	1.561	(0.121)	5.436	(0.967)		5.158	(1.47)	
High-Disadvantage Area	-0.026	(0.001)	-0.019	(0.005)	X	-0.035	(0.008)	X
Share Black (zipcode)	-0.008	(0.0003)	-0.007	(0.001)	X	-0.011	(0.002)	X
Share Hispanic (zipcode)	-0.008	(0.0004)	-0.005	(0.001)	X	-0.008	(0.003)	X
Near Safety Net Hosp/CHC	-0.028	(0.001)	-0.014	(0.005)		-0.027	(0.009)	X
D. Duration Enrolled								
Average (months)	1.444	(0.03)	2.635	(0.163)		1.697	(0.122)	X
Share 1-3 months	-0.024	(0.001)	-0.064	(0.007)		-0.034	(0.006)	X
Share 12+ months	0.038	(0.001)	0.093	(0.01)		0.029	(0.008)	X
Share 16+ months	0.041	(0.001)	0.059	(0.004)		0.071	(0.006)	

Note: The table shows robustness checks on the targeting analysis in the body text Section (5.1). See the text of Appendix (C.4) for a detailed description of the method. Columns (1)-(2) show the change in average enrollee characteristics (for each variable listed in the first column) after auto-enrollment ends implied by the main targeting analysis shown in body text Table (1). Columns (3)-(4) report estimates from simple DD regressions following equation (17) capturing the actual change in average characteristics for the treatment group relative to controls (with separate regressions for each variable). Columns (6)-(7) report estimates from DD regressions with group-specific linear time trends, as shown in equation (18). Columns (5) and (8) report whether the confidence intervals from each DD estimate overlap with the implied change from the main method shown in column (1). Confidence intervals overlap for 10 of 20 variables with the simple DD and 16 of 20 variables for the DD with trends.

Table A.6: Active vs. Passive Enrollees during 2007 Auto-Enrollment of Uncompensated Care Pool

	Uncompensated Care Pool Auto-Conversion (2007)				Main Sample 2008-09 (from Table 2)	
Outcome	**Active** (1)	**Passive** (2)	**Diff.** (3)	**%Diff** (4)	**Diff.** (5)	**%Diff** (6)
A. Demographics						
Average Age (years)	38.1	33.4	-4.7	*-12%*	-3.8	*-11%*
Age 19-34	0.456	0.598	+0.142	*31%*	+0.118	*22%*
Age 35-54	0.365	0.299	-0.066	*-18%*	-0.068	*-20%*
Age 55+	0.179	0.103	-0.076	*-42%*	-0.049	*-39%*
Share Male	0.466	0.545	+0.080	*17%*	+0.087	*16%*
Male Age 19-34	0.215	0.334	+0.119	*55%*	+0.125	*44%*
B. Health Status and Medical Spending						
Any Chronic Illness	0.677	0.391	-0.286	*-42%*	-0.215	*-33%*
Severe Chronic Illness	0.163	0.072	-0.091	*-56%*	-0.077	*-49%*
Risk Score (HCC)	1.024	0.640	-0.384	*-38%*	-0.367	*-36%*
Average Cost ($/month)	$373.4	$183.8	-$189.6	*-51%*	-$180.5	*-44%*
Any Spending (>$0)	0.901	0.637	-0.264	*-29%*	-0.185	*-21%*
C. Duration Enrolled						
Average (months)	21.5	16.0	-5.5	*-26%*	-4.6	*-28%*
Share 1-3 months	0.103	0.137	+0.034	*33%*	+0.075	*49%*
Share 12+ months	0.730	0.668	-0.062	*-8%*	-0.119	*-21%*
Share 16+ months	0.476	0.352	-0.125	*-26%*	-0.129	*-43%*
D. Income & Neighborhood						
High-Disadvantage Area	0.375	0.428	+0.054	*14%*	+0.082	*26%*
Share Black (in zipcode)	0.107	0.119	+0.012	*11%*	+0.024	*29%*
Share Hispanic (in zipcode)	0.150	0.172	+0.022	*15%*	+0.025	*18%*
Near Safety Net Hosp/CHC	0.455	0.506	+0.051	*11%*	+0.087	*23%*

Note: The table replicates the comparison of active vs. passive enrollees characteristics/outcomes from Table 1, applied to the Uncompensated Care Pool auto-conversion period during FY 2007 months 6-8. The enrollment process differed during this period, so this tests whether the targeting properties of auto-enrollment are robust to these different institutions. Columns (1)-(2) show means for active vs. passive enrollees, after adjusting for cohort-of-entry fixed effects. Column (3)-(4) show the difference and percent difference in each variable for the 2007 UCP period. Column (5)-(6) show similar difference and percent difference for the main 2008-09 sample. These active vs. passive differences are qualitatively and quantitatively similar across all variables. See Table 1's note for a description of the variables. Note that we exclude family income as share of poverty from this table since it is unavailable in the 2007 data.

Table A.7: Tests of Choice Overload: Passive Rate vs. Choice Set Size

Panel A: Cross-Area Relationship		
# of Plans Available	Passive Rate	Sample Size (new enrollees)
1	33.9%	6,696
2	34.5%	5,009
3	35.2%	50,886
4	32.9%	46,103
Avg	*34.1%*	*108,694*

Panel B: Diff-in-Diff	
	Outcome: Passive Rate
ΔPlans*Post	-0.014
	(0.011)
Num Obs. (area-months)	874

NOTE: The table shows the relationship between the passive enrollment rate and the choice set size for the 2008-09 period, as a way of testing "choice overload" as an explanation for passive behavior. The choice set varies across areas and over time because of insurer participation decisions. Each of four insurers operating in CommCare offers a single plan, but they can choose whether the plan is available in 38 "service areas" of the state. Panel A shows the cross-sectional relationship between number of plans available and the passive rate. Panel B shows a difference-in-difference regression capturing how the passive rate changes when the number of plans changes. Both analyses suggest little relationship between passivity and the choice set size.

Table A.8: Passive Rates by Factors related to Inattention or Misunderstanding

	Sample Statistics		Passive Enrollment Rate		
	Number	Share	Raw	Adjusted for Controls	
	(1)	(2)	(3)	(4)	(5)
Address Mismatch					
Mismatched Zipcode	31,010	36%	0.282	0.280	0.284
			(0.002)	(0.002)	(0.002)
No Mismatch	54,869	64%	0.249	0.252	0.250
			(0.002)	(0.002)	(0.002)
Immigration Status (language barriers)					
Immigrant Enrollee	16,247	12%	0.412	0.434	0.421
			(0.004)	(0.004)	(0.004)
All Others	117,269	88%	0.340	0.337	0.340
			(0.001)	(0.001)	(0.001)
Cross-Program Transitions					
Uncompensated Care Pool (early 2007 only)					
Transiton UCP to CommCare	31,820	77%	0.603	0.605	0.603
			(0.003)	(0.003)	(0.003)
All Other New Enrollees	9,366	23%	0.403	0.395	0.400
			(0.005)	(0.005)	(0.005)
Medicaid Transitions (main 2008-09 sample)					
Transiton Medicaid to CommCare	41,339	35%	0.388	0.372	0.379
			(0.002)	(0.002)	(0.002)
All Other New Enrollees	75,930	65%	0.313	0.323	0.320
			(0.002)	(0.002)	(0.002)
Controls Included:					
Age and Sex			---	X	X
Timing of first claim *(address mismatch analysis only)*			---	---	X
Health Status, Risk Score			---	---	X

Note: The table shows variation in the passive enrollment rate by factors related to inattention or misunderstanding. See the discussion in the appendix text for a description of the analysis, the samples, and the variable definitions. Columns (1)-(2) report sample statistics for each variable. Column (3) reports the raw passive enrollment rate by the categories of the variable (e.g., mismatched zipcode vs. no mismatch). Column (4)-(5) show adjusted means from a regression that controls for the indicated variables, with adjusted means output using Stata's "margins" command. Age-sex variables include gender dummies x age categories (19, 20, 21-24, 25-29, 30-34, ..., 60-64, 65+). Timing of the first claim are dummies for the first month of the enrollment spell when a claim is observed; this is used only in the address mismatch analysis. Health status variables are dummies for chronic illness and severe chronic illness, and deciles of the HCC medical risk score.

Table A.9: Incidence of Auto-Enrollment

		Actual Payment System (2008-09)				Counterfactual Risk Adj. (HCC)		
	Enrollees	Revenue	Cost	Gross Margin		Revenue	Gross Margin	
				$	%		$	%
Active	74780	$373.2	$403.8	-$30.5	*-8.2%*	$402.0	-$1.8	*-0.5%*
Passive	37051	$344.0	$223.8	$120.2	*34.9%*	$251.6	$27.7	*8.1%*
Total	**111831**	**$364.3**	**$348.4**	**$15.9**	***4.4%***	**$355.7**	**$7.3**	***2.0%***

Note: The table shows average monthly insurer revenues, paid medical costs, and gross profit margins (not accounting for any administrative costs) for active, passive, and all enrollees in our main sample. The first columns show actual data given CommCare's payment rules. Although insurers were overpaid for passive enrollees, they were underpaid for active enrollees, implying roughly zero profit margins for the pooled group. The final columns show a counterfactual analysis with improved risk adjustment, using the HCC system used in the ACA. This method better matches revenues to costs for passive and active enrollees, but overall insurer revenues and margins are little affected. Figures are computed using the first 12 months of the enrollment spell for all new below-poverty CommCare enrollees over fiscal years 2008-09.

Table A.10: Robustness: Targeting Impact of Auto-Enrollment

Robustness Specification	Active Enrollees				Passive Enrollees			
	Enrollee Value (1)	Social Value (2)	Net Cost (3)	*V/C Ratio* (4)	Enrollee Value (5)	Social Value (6)	Net Cost (7)	*V/C Ratio* (8)
Baseline Estimates *(see Table 2)*	**$128**	**$216**	**$255**	***0.85***	**$93**	**$143**	**$142**	***1.00***
Alternate Estimates: Enrollee Value								
(1) Demand: No unobserved sorting	$123	$212	$255	*0.83*	$110	$159	$142	*1.12*
(2) Demand: Perfect unobs. sorting	$169	$258	$255	*1.01*	$18	$67	$142	*0.47*
(3) Demand: Median WTP	$80	$168	$255	*0.66*	$71	$120	$142	*0.84*
75th percentile WTP	$31	$119	$255	*0.47*	$28	$77	$142	*0.54*
(4) Using FHL Estimates: Low-End	$82	$170	$255	*0.67*	$46	$95	$142	*0.67*
High-End	$196	$284	$255	*1.12*	$110	$159	$142	*1.12*
(5) Value = Uninsured OOP Costs	$64	$153	$255	*0.60*	$36	$85	$142	*0.60*
(6) Exponential Utility (Direct Loss)	$735	$735	$408	*1.80*	$312	$312	$228	*1.37*
Alternate Estimates: Uncompensated Care								
(7) Mass. HSN Data Estimate	$128	$191	$299	*0.64*	$93	$146	$137	*1.07*
(8) Zero Uncomp. Care *(LB)*	$128	$128	$408	*0.31*	$93	$93	$228	*0.41*
(9) Full Uncomp. Care *(UB)*	$128	$240	$214	*1.12*	$93	$156	$120	*1.30*

Note: The table shows robustness checks on the targeting analysis reported in Table 3 of the body text. It reports enrollee/social value and cost statistics for active enrollees (columns 1-4) and passive enrollees (columns 5-8) based on alternate assumptions. The top row (in bold) replicates the baseline estimates from Table 3. Rows (1)-(6) show alternate assumptions for enrollee value (demand), including: (1-2) no and perfect unobserved sorting (see Appendix F), (3-4) using median or 75th percentile WTP instead of average WTP, (4-5) simple value estimates based on results in Finkelstein, Hendren and Luttmer (2019a) ("FHL"), and (6) implied demand from a simple model of exponential utility using a coefficient of absolute risk aversion of 8.6×10^{-5} from Handel and Kolstad (2015), assuming individuals pay the full cost of observed medical care when uninsured (i.e., no moral hazard or uncompensated care). Rows (7)-(9) show alternate assumptions for the uncompensated care estimates. Row (7) uses estimates based on the Massachusetts Health Safety Net (HSN) data (see Appendix E), and rows (8)-(9) report a lower and upper bound of zero and full uncompensated care. See Sections 6.1-6.3 for further description of the model and these sensitivity analyses.

Table A.11: Robustness: Optimal Policy Simulations

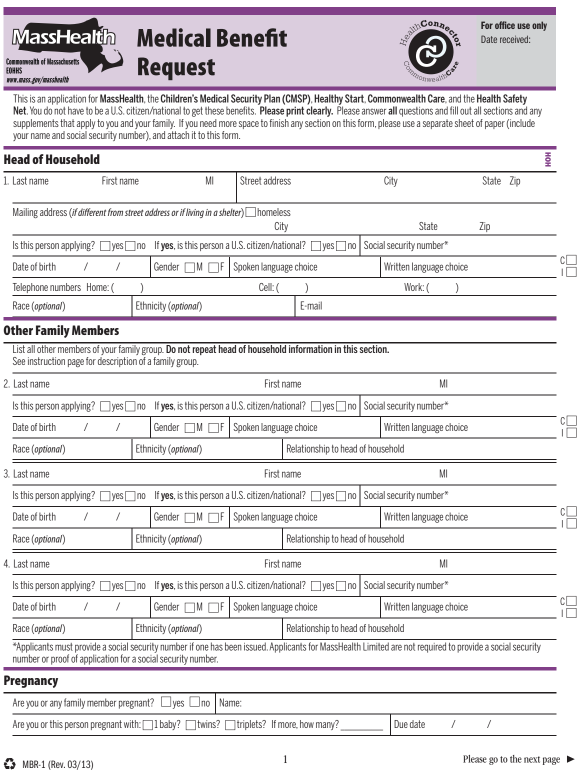
Specification	Ordeals Ever Optimal? (1)	Range of μ where Optimal (2)	Gains from Targeting ($ per enrollee-month) μ = 1.0 (3)	μ = 2.0 (4)	μ = 3.0 (5)
Baseline Estimates	**No**	**--**	**-$26**	**-$31**	**-$61**
Alternate Estimates: Enrollee Value					
1. Demand: No unobs. Sorting	No	--	-$29	-$41	-$77
2. Demand: Perfect unobs. Sorting	Yes	(0.98, 5.14)	$2	$19	$13
3. Demand: Median WTP	No	--	-$58	-$16	-$39
75th percentile WTP	No	--	-$91	-$70	-$50
4. Using FHL Estimates: Low-End	No	--	-$57	-$2	-$14
High-End	No	--	-$5	-$41	-$77
5. Value = Uninsured OOP Costs	No	--	-$69	-$25	-$5
6. Exponential Utility (Direct Loss)	Yes	(0.56, 0.73)	-$27	-$129	-$231
Alternate Estimates: Uncompensated Care					
7. Mass. HSN Data Estimate	No	--	-$73	-$33	-$64
8. Zero Uncomp. Care (LB)	No	--	-$189	-$103	-$17
9. Full Uncomp. Care (UB)	No	--	-$12	-$42	-$73

Note: The table shows robustness checks on the optimal targeting policy simulations presented in Figure 8 in the body text. Column (1) reports whether ordeals targeting is optimal (relative to no/full enrollment) at any value of social welfare weight (μ). Column (2) reports the values of the social welfare weight for which targeting is optimal. Columns (3)-(5) report the gains from targeting at various values of the social welfare weight. The top row (in bold) shows results for the baseline specification displayed in Figure 8. Rows (1)-(6) show alternate assumptions for enrollee value (demand), including: (1-2) no and perfect unobserved sorting (see Appendix F), (3-4) using median or 75th percentile WTP instead of average WTP, (4-5) simple value estimates based on results in Finkelstein, Hendren and Luttmer (2019a) ("FHL"), and (6) implied demand from a simple model of exponential utility using a coefficient of absolute risk aversion of 8.6×10^{-5} from Handel and Kolstad (2015), which assumes individuals pay the full cost of observed medical care when uninsured (i.e., no moral hazard or uncompensated care). Rows (7)-(9) show alternate assumptions for the uncompensated care estimates. Row (7) uses estimates based on the Massachusetts Health Safety Net (HSN) data (see Appendix E), and rows (8)-(9) report a lower and upper bound of zero and full uncompensated care. See Sections 6.1-6.3 for further description of the model and these sensitivity analyses.

H Massachusetts Exchange (CommCare) Enrollment Forms

Application Form for CommCare

The following shows the application form that must be submitted to apply for CommCare (step #1 of the two-step process). This form collects information on income, family status, and other sources of health insurance. The state uses this form to determine whether a person was eligible for CommCare, Medicaid (MassHealth) or neither. In addition to the main six pages below, there is a signature page and five pages of "supplements" that certain groups of applicants need to fill out.



MassHealth
Commonwealth of Massachusetts
EOHHS
www.mass.gov/masshealth

Medical Benefit Request

HealthConnector CommonwealthCare

For office use only
Date received:

This is an application for **MassHealth**, the **Children's Medical Security Plan (CMSP)**, **Healthy Start**, **Commonwealth Care**, and the **Health Safety Net**. You do not have to be a U.S. citizen/national to get these benefits. **Please print clearly.** Please answer **all** questions and fill out all sections and any supplements that apply to you and your family. If you need more space to finish any section on this form, please use a separate sheet of paper (include your name and social security number), and attach it to this form.

Head of Household HOH

1. Last name | First name | MI | Street address | City | State | Zip

Mailing address (*if different from street address or if living in a shelter*) ☐ homeless | City | State | Zip

Is this person applying? ☐ yes ☐ no | If **yes**, is this person a U.S. citizen/national? ☐ yes ☐ no | Social security number*

Date of birth / / | Gender ☐ M ☐ F | Spoken language choice | Written language choice | C ☐ I ☐

Telephone numbers Home: ( ) | Cell: ( ) | Work: ( )

Race (*optional*) | Ethnicity (*optional*) | E-mail

Other Family Members

List all other members of your family group. **Do not repeat head of household information in this section.**
See instruction page for description of a family group.

2. Last name | First name | MI

Is this person applying? ☐ yes ☐ no | If **yes**, is this person a U.S. citizen/national? ☐ yes ☐ no | Social security number*

Date of birth / / | Gender ☐ M ☐ F | Spoken language choice | Written language choice | C ☐ I ☐

Race (*optional*) | Ethnicity (*optional*) | Relationship to head of household

3. Last name | First name | MI

Is this person applying? ☐ yes ☐ no | If **yes**, is this person a U.S. citizen/national? ☐ yes ☐ no | Social security number*

Date of birth / / | Gender ☐ M ☐ F | Spoken language choice | Written language choice | C ☐ I ☐

Race (*optional*) | Ethnicity (*optional*) | Relationship to head of household

4. Last name | First name | MI

Is this person applying? ☐ yes ☐ no | If **yes**, is this person a U.S. citizen/national? ☐ yes ☐ no | Social security number*

Date of birth / / | Gender ☐ M ☐ F | Spoken language choice | Written language choice | C ☐ I ☐

Race (*optional*) | Ethnicity (*optional*) | Relationship to head of household

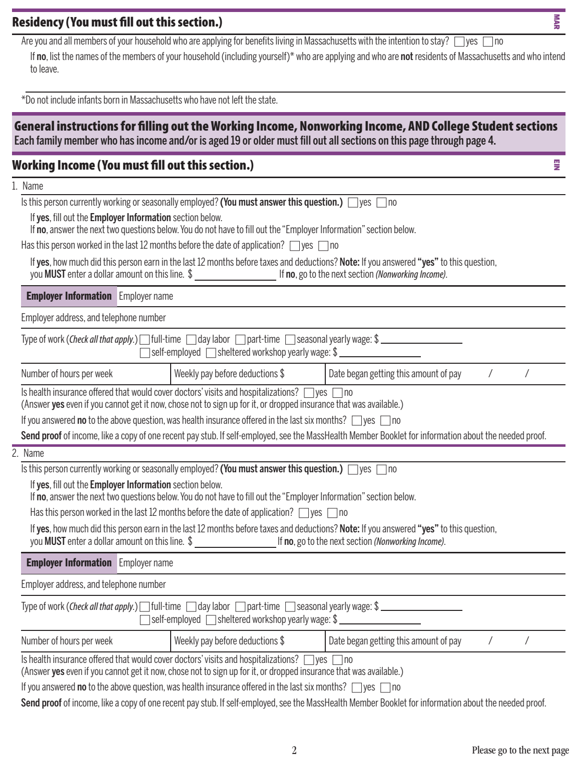
*Applicants must provide a social security number if one has been issued. Applicants for MassHealth Limited are not required to provide a social security number or proof of application for a social security number.

Pregnancy

Are you or any family member pregnant? ☐ yes ☐ no | Name:

Are you or this person pregnant with: ☐ 1 baby? ☐ twins? ☐ triplets? If more, how many? ________ | Due date / /

MBR-1 (Rev. 03/13) 1 Please go to the next page ►



Residency (You must fill out this section.) MAR

Are you and all members of your household who are applying for benefits living in Massachusetts with the intention to stay? ☐ yes ☐ no

If **no**, list the names of the members of your household (including yourself)* who are applying and who are **not** residents of Massachusetts and who intend to leave.

*Do not include infants born in Massachusetts who have not left the state.

General instructions for filling out the Working Income, Nonworking Income, AND College Student sections
Each family member who has income and/or is aged 19 or older must fill out all sections on this page through page 4.

Working Income (You must fill out this section.) EIN

1. Name

Is this person currently working or seasonally employed? **(You must answer this question.)** ☐ yes ☐ no

If **yes**, fill out the **Employer Information** section below.
If **no**, answer the next two questions below. You do not have to fill out the "Employer Information" section below.

Has this person worked in the last 12 months before the date of application? ☐ yes ☐ no

If **yes**, how much did this person earn in the last 12 months before taxes and deductions? **Note:** If you answered **"yes"** to this question, you **MUST** enter a dollar amount on this line. $ ________ If **no**, go to the next section *(Nonworking Income)*.

Employer Information Employer name

Employer address, and telephone number

Type of work (*Check all that apply.*) ☐ full-time ☐ day labor ☐ part-time ☐ seasonal yearly wage: $ ________
☐ self-employed ☐ sheltered workshop yearly wage: $ ________

Number of hours per week | Weekly pay before deductions $ | Date began getting this amount of pay / /

Is health insurance offered that would cover doctors' visits and hospitalizations? ☐ yes ☐ no
(Answer **yes** even if you cannot get it now, chose not to sign up for it, or dropped insurance that was available.)

If you answered **no** to the above question, was health insurance offered in the last six months? ☐ yes ☐ no

Send proof of income, like a copy of one recent pay stub. If self-employed, see the MassHealth Member Booklet for information about the needed proof.

2. Name

Is this person currently working or seasonally employed? **(You must answer this question.)** ☐ yes ☐ no

If **yes**, fill out the **Employer Information** section below.
If **no**, answer the next two questions below. You do not have to fill out the "Employer Information" section below.

Has this person worked in the last 12 months before the date of application? ☐ yes ☐ no

If **yes**, how much did this person earn in the last 12 months before taxes and deductions? **Note:** If you answered **"yes"** to this question, you **MUST** enter a dollar amount on this line. $ ________ If **no**, go to the next section *(Nonworking Income)*.

Employer Information Employer name

Employer address, and telephone number

Type of work (*Check all that apply.*) ☐ full-time ☐ day labor ☐ part-time ☐ seasonal yearly wage: $ ________
☐ self-employed ☐ sheltered workshop yearly wage: $ ________

Number of hours per week | Weekly pay before deductions $ | Date began getting this amount of pay / /

Is health insurance offered that would cover doctors' visits and hospitalizations? ☐ yes ☐ no
(Answer **yes** even if you cannot get it now, chose not to sign up for it, or dropped insurance that was available.)

If you answered **no** to the above question, was health insurance offered in the last six months? ☐ yes ☐ no

Send proof of income, like a copy of one recent pay stub. If self-employed, see the MassHealth Member Booklet for information about the needed proof.

2 Please go to the next page ►

Nonworking Income (You must fill out this section.)

Rental Income Do you or any family member get rental income? (**You must answer this question.**) ☐ yes ☐ no REN

If **yes**, enter the monthly amount of rental income (before taxes and deductions) on this line. $ ______________

Name of person getting rental income

If **no**, go to the next section (*Unemployment Benefits*).

Send proof of rental income.

Unemployment Benefits Are you or any family member getting an unemployment check? **(You must answer this question.)** ☐ yes ☐ no UIN

If **yes**, fill out this section and answer all questions. **Send proof** of unemployment benefits.
If **no**, go to the next section (*Other Nonworking Income*).

Name of person getting unemployment benefits

Is this check from the Commonwealth of Massachusetts? ☐ yes ☐ no

If **yes**, in the 12 months before this person became unemployed, did this person work for an employer in Massachusetts? ☐ yes ☐ no
(Do not include federal employers, like the U.S. Postal Service.)

Enter the monthly amount of unemployment benefits (before taxes and deductions). $ ______________

Name of person getting unemployment benefits

Is this check from the Commonwealth of Massachusetts? ☐ yes ☐ no

If **yes**, in the 12 months before this person became unemployed, did this person work for an employer in Massachusetts? ☐ yes ☐ no
(Do not include federal employers, like the U.S. Postal Service.)

Enter the monthly amount of unemployment benefits (before taxes and deductions). $ ______________

Other Nonworking Income Do you or any family member have any other income? **(You must answer this question.)** ☐ yes ☐ no UIN

If **yes**, fill out this section.
If **no**, go to the next section (*College Student*).

Please describe the source of the income (where it comes from) for each family member. If anyone has more than one source, list on separate lines.

Send proof. Some types of other income are: (You do not have to send proof of social security or SSI income.)

- alimony
- annuities
- child support
- dividends or interest
- pensions
- retirement
- social security
- SSI
- trusts
- veterans' benefits (federal, state, or city)
- workers' compensation
- other (*Please describe below.*)

Name	Type of income (all that apply from list above)
Source (where the income comes from)	Monthly amount before taxes $
Name	Type of income (all that apply from list above)
Source (where the income comes from)	Monthly amount before taxes $
Name	Type of income (all that apply from list above)
Source (where the income comes from)	Monthly amount before taxes $

College Student (You must fill out this section.)

Are you or any family member a college student? **(You must answer this question.)** ☐ yes ☐ no

If **yes**, fill out this section and answer all questions.
If **no**, go to the next section (*Health Insurance You Have Now and Subsidized Health Insurance You May Be Eligible For*).

1. Name of college student

Is this person eligible for health insurance from college? ☐ yes ☐ no

Is this person a college student in Massachusetts with at least 75% of a full-time schedule? ☐ yes ☐ no
(**Note:** If you are not sure that this person has 75% of a full-time schedule, contact the school to find out if the number of credits the student is taking would require the student to get the health insurance the school offers to students.)

If yes, is this student planning to get health-insurance coverage from the school, but is waiting for coverage to start? ☐ yes ☐ no

If yes, what is the date that the school health-insurance coverage starts? ___ / ___ / ___

2. Name of college student

Is this person eligible for health insurance from college? ☐ yes ☐ no

Is this person a college student in Massachusetts with at least 75% of a full-time schedule? ☐ yes ☐ no
(**Note:** If you are not sure that this person has 75% of a full-time schedule, contact the school to find out if the number of credits the student is taking would require the student to get the health insurance the school offers to students.)

If yes, is this student planning to get health-insurance coverage from the school, but is waiting for coverage to start? ☐ yes ☐ no

If yes, what is the date that the school health-insurance coverage starts? ___ / ___ / ___

Health Insurance You Have Now and Subsidized Health Insurance You May Be Eligible For

Even if you or any family member have other health insurance, MassHealth may be able to help you pay your premiums. Health insurance can be from an employer, an absent parent, a union, a school, Medicare, or Medicare supplemental insurance, like Medex. **All applicants must fill out the health insurance section. Do not include MassHealth or any health plan you enrolled in through Commonwealth Care when answering the questions below.**

Do you or any family member get Medicare benefits? ☐ yes ☐ no

If **yes**, name(s):

Claim number(s):

Do you or any family member have health insurance other than Medicare? ☐ yes ☐ no

If **yes**, fill out both **Part A** below and **Part B** on the next page.
If **no**, fill out **Part B** on the next page.

Part A: Health Insurance You Have Now

1. Policyholder name | Date of birth / /

Social security number* | Insurance company name

Policy type (*Check one.*) ☐ individual ☐ couple (two adults) ☐ dual (one adult, one child) ☐ family | Policy start date ___ / ___ / ___

Policy number | Group number (if known)

Employer or union name

Policyholder contribution to premium costs (*Complete one.*) $ per week | $ per quarter | $ per month

Insurance type (*Check one.*) ☐ employer or union subsidized (employer or union pays some or all of the insurance cost) ☐ TRICARE
☐ other federal or state subsidized (government pays some or all of the insurance cost) ☐ student health insurance through school
☐ nonsubsidized, like self-employment or COBRA (policyholder pays total insurance cost) ☐ Medical Security Program

Names of covered family members

Insurance coverage (*Check all that apply.*) ☐ doctors' visits and hospitalizations ☐ catastrophic only ☐ vision only ☐ pharmacy only ☐ dental only

If you have long-term-care insurance, **send a copy** of the policy.

* Required, if obtainable and one has been issued, whether or not this person is applying.

2. Policyholder name Date of birth / /

Social security number* Insurance company name

Policy type (*Check one.*) individual couple (two adults) dual (one adult, one child) family Policy start date ___ / ___ / ___

Policy number Group number (if known)

Employer or union name

Policyholder contribution to premium costs (*Complete one.*) $ per week | $ per quarter | $ per month

Insurance type (*Check one.*) ☐ employer or union subsidized (employer or union pays some or all of the insurance cost) ☐ TRICARE
☐ other federal or state subsidized (government pays some or all of the insurance cost) ☐ student health insurance through school
☐ nonsubsidized, like self-employment or COBRA (policyholder pays total insurance cost) ☐ Medical Security Program

Names of covered family members

Insurance coverage (*Check all that apply.*) ☐ doctors' visits and hospitalizations ☐ catastrophic only ☐ vision only ☐ pharmacy only ☐ dental only

If you have long-term-care insurance, **send a copy** of the policy.
* Required, if obtainable and one has been issued, whether or not this person is applying.

Part B: Subsidized Health Insurance You May Be Eligible For

Are you or any member of your family in one of the uniformed services? ☐ yes ☐ no

If **yes**, fill out the section below. (The uniformed services are the Army, Navy, Air Force, Marine Corps, Coast Guard, Public Health Services, National Oceanic and Atmospheric Administration, and the National Guard or Reserves.)

1. Name:

Active Duty? ☐ yes ☐ no	Retiree? ☐ yes ☐ no	Reserves? ☐ yes ☐ no	Medal of Honor? ☐ yes ☐ no

2. Name:

Active Duty? ☐ yes ☐ no	Retiree? ☐ yes ☐ no	Reserves? ☐ yes ☐ no	Medal of Honor? ☐ yes ☐ no

Have you or any member of your family served in the U.S. military or can you be considered a dependent of someone who has served in the U.S. military?

☐ Yes, I have served. Name:

☐ Yes, I am a dependent of someone who has served. Name:

☐ No, I am neither a veteran nor a dependent.

American Indian/Alaska Native

Certain American Indians and Alaska Natives may not have to pay MassHealth premiums and copays.

Are you or any member of your family who is applying a federally recognized American Indian or Alaska Native who is eligible to receive or has received services from an Indian health-care provider or from a non-Indian health-care provider through referral from an Indian health-care provider? ☐ yes ☐ no

If **yes**, name of person(s):

General instructions for filling out the Injury, Illness, Disability, or Accommodation, Absent Parent, and U.S. Citizenship/National Status and Immigration Status sections below

The HIV section is optional. You must answer all questions in each of the three sections after the HIV section.

HIV Information (optional) HIV

MassHealth may give benefits to people who are HIV positive who might not otherwise be eligible.
Do you or any family member who is HIV positive want to apply for these benefits? ☐ yes ☐ no

If **yes**, fill out this section.

Send proof of income, U.S. citizenship/national status and identity, or qualified alien status to see if you can get benefits for up to 60 days while we wait for you to send us proof of your HIV-positive status. For more information, see the MassHealth Member Booklet.

Name(s):

Injury, Illness, Disability, or Accommodation

Do you or any family member have an injury, illness, or disability (including a disabling mental-health condition) that has lasted or is expected to last for at least 12 months? (If legally blind, answer **yes**.) ☐ yes ☐ no

Do you or any family member need health care because of an accident or injury? ☐ yes ☐ no

Do you or any family member applying for MassHealth require a reasonable accommodation because of a disability or injury? ☐ yes ☐ no

If you answered **yes** to any of these three questions, you must fill out **Supplement A** (the blue sheet).

Absent Parent

Has any child in the household been adopted by a single parent or has a parent who is deceased or unknown? ☐ yes ☐ no

Does any child in the family have a parent who does not live with you who is not included in the previous question? ☐ yes ☐ no

If you answered **yes** to either of these questions, you must fill out **Supplement B** (the yellow sheet).

U.S. Citizenship/National Status and Immigration Status

The U.S. citizenship/national status of parents does not affect the eligibility of their children.

U.S. Citizens

For applicants **born in Massachusetts** who want help getting proof of their U.S. citizenship, please fill out **Supplement D** (the red sheet).

For applicants **born outside Massachusetts** who want help getting proof of their U.S. citizenship, MassHealth may be able to help you. Please call MassHealth Customer Service at 1-800-841-2900 (TTY: 1-800-497-4648 for people who are deaf, hard of hearing, or speech disabled).

Persons who are not U.S. citizens/nationals

If you or any other family member applying for MassHealth or Commonwealth Care fits any of the immigration status codes on **Supplement C** (the orange sheet), numbered 1 through 17, you must fill out **Supplement C**.

If you or any other family member applying for benefits does not fit any of the immigration status codes on **Supplement C** (the orange sheet), numbered 1 through 17, you or that family member may get only one or more of the following: MassHealth Limited, Healthy Start, CMSP, or the Health Safety Net. You do not have to fill out **Supplement C**.

Note: A social security number is not required for approval for MassHealth Limited. We will not match the names of applicants for MassHealth Limited with any other agency including the Department of Homeland Security (DHS). You do not need to send proof of immigration status. MassHealth Limited pays for emergency services only. See the MassHealth Member Booklet for more information.

List below the names of family members who want to get only one or more of the following: MassHealth Limited, Healthy Start, CMSP, or the Health Safety Net.

Name(s):

Name(s):

CommCare Plan Choice Form

The next pages show the "plan choice form" received when they were accepted to CommCare (after submitting the application form shown above). The form is a letter that shows an enrollee their plan choice options and associated premiums and refers enrollees to a website for more information on plans (e.g., on provider networks). The form prompts enrollees to go online, call the Connector, or return the form by mail to choose a plan. For the 0-100% of poverty group we study, all plans have a premium of $0 (as shown), but for higher-income groups the correct premium amounts would be shown. Higher-income groups would also need to return the first month's premium payment when they choose a plan.

HealthConnector
CommonwealthCare

Your **connection** to good health

[Mail_date]
[Case_Name]
[Case_Street]
[Case_City], [Case_State] [Case_Zip]

Dear [Insert Name]

Welcome to Commonwealth Care. Here is the enrollment package you requested. This information will help you select and enroll in the health plan that is right for you. Your package includes:

- ***Getting Started***, a brochure about Commonwealth Care that explains the program and how to enroll.
- ***Health Benefits and Copays***, a chart that lists your health benefits and how much you pay for each health visit or service (copays).
- ***Health Plan Information***, descriptions of each health plan available to you and any special programs they offer. The health plans available to you depend on where you live, your plan type and in some cases, whether you've been previously enrolled with Commonwealth Care or MassHealth.
- ***Enroll Now***, information and instructions for selecting and enrolling in a health plan.

There are a lot of benefits to enrolling in Commonwealth Care: you get your own health care provider, regular checkups, care when you are sick or injured, prescriptions, treatment for alcohol, drug abuse and mental health problems, vision care and free glasses. Some members also receive dental benefits (Plan Type 1 only).

You can enroll in Commonwealth Care over the phone and online.*

1. **By phone**: Call the Commonwealth Care Member Service Center Monday - Friday, from 8:00 a.m. to 5:00 p.m. at 1-877 MA ENROLL (1-877-623-6765) TTY 1-877-623-7773 for people with partial or total hearing loss.
2. **Online**: Enroll using the Commonwealth Care website at www.MAhealthconnector.org. Read the instructions on the back of this letter to learn how to create an account and log in.

If you have any questions, call the Commonwealth Care Member Service Center Monday - Friday, from 8:00 a.m. to 5:00 p.m. at 1-877 MA ENROLL (1-877-623-6765) TTY 1-877-623-7773 for people with partial or total hearing loss.

We are pleased to offer you a full range of health benefits and be your connection to good health.

Commonwealth Care Member Service Center

Turn to review your health plan options

Member ID

Enroll Now! Select and Enroll in a Commonwealth Care health plan

Below are the Commonwealth Care health plans you can choose from. The dollar amount next to each health plan is what you must pay each month to stay enrolled in that plan. If you select a health plan with $0.00 next to it, you will not be charged a monthly premium. The premiums listed below are based on your plan type, which depends on your income and your family size. Based on the information you provided, you are eligible for **Plan Type X**.

1. Choose your health plan and premium. Choose only one.
These plans are available to you. Read each Health Plan Information description to learn about the Commonwealth Care health plans.

<BMC HealthNet Plan	$0.00	web address	Phone number>
<CeltiCare Health Plan	$0.00	web address	Phone number>
<Fallon Community Health Plan	$0.00	web address	Phone number>
<Neighborhood Health Plan	$0.00	web address	Phone number>
<Network Health	$0.00	web address	Phone number>

2. Choose your Primary Care Provider (PCP).
Tell us the name of your PCP when you select your health plan by phone or online.* When choosing a health plan, check to see if the doctors, hospitals or community health center you visit today are part of the plan you would like to select. To find out if a provider is in a certain health plan, look on our website or call the doctors, the health plans, or the Commonwealth Care Member Service Center.

You have selected____**as your Primary Care Provider (PCP).**
First Name Last name

3. Enroll by phone, or online.* Enroll by phone or on our website. Commonwealth Care will send you a bill if you need to pay a monthly premium. After you pay your first monthly premium, you will be in Commonwealth Care. If you do not need to pay a monthly premium, Commonwealth Care will enroll you in your selected health plan.

If this is your first time using the website, follow the instructions below.

Create an account

1. Log on to www.MAhealthconnector.org
2. Click **Register** for access to your account
3. Click **Create Login** then follow the instructions on each screen

* If you are unable to call or go online, circle the health plan of your choice,
write in the name of your PCP and mail this page to:
Commonwealth Care Member Service Center, 133 Portland St, 1st Floor, Boston MA 02114-1707.
DO NOT A SEND PAYMENT with your health plan selection.