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REDUCING ORDEALS THROUGH AUTOMATIC ENROLLMENT: EVIDENCE FROM A HEALTH INSURANCE EXCHANGE

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ABSTRACT

Incomplete health insurance enrollment is a persistent U.S. challenge despite large subsidies. We ask whether hassles built into enrollment systems matter for insurance take-up and targeting. Studying removal of an auto-enrollment policy, we find that a small hassle – a requirement to actively select a health plan to enroll – reduces take-up by 33%, a major impact equivalent to $470 (57%) higher enrollee premiums. Hassles differentially screen out younger, healthier, and poorer people – groups with both low value and costs of insurance. We show that this value-cost correlation – a standard feature of insurance, where risk drives both – may undermine the classic rationale for ordeals' favorable targeting.

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1 Introduction

Incomplete take-up of social safety net programs is a longstanding public policy concern and economic puzzle. Across a wide variety of programs – from food stamps (SNAP) to the Earned Income Tax Credit (EITC) to Pell Grants – sizable shares of eligible populations do not enroll (Currie, 2006). Incomplete take-up is particularly important in the context of U.S. health insurance, where universal coverage is a longstanding policy goal. Despite the Affordable Care Act's (ACA) enactment in 2014, almost 30 million people (or 11% of the non-elderly population) remained uninsured as of 2019, down by less than half from the pre-ACA uninsured rate of 17%.

In seeking to reduce uninsurance, the standard policy approach is to expand *financial subsidies* for insurance to make coverage more affordable. This approach, which formed the core of the ACA, is backed by a large body of research showing that financial incentives matter for take-up.[1] However, the persistence of uninsurance also suggests limits to their effectiveness. Indeed, about three-fifths of the remaining uninsured are already eligible for subsidized coverage, including 40-50% who qualify for completely free health insurance (Cox and McDermott, 2020; Rae et al., 2021).

In this paper, we study an alternate approach to coverage expansion: reducing the *hassles* or "*ordeals*" involved with insurance enrollment systems. Hassles are a pervasive feature of the U.S. health insurance system and its social safety net more broadly. Across a plethora of programs – each with distinct eligibility rules and application systems – low-income individuals themselves bear the onus (or "administrative burden") of enrolling to obtain support (Herd and Moynihan, 2019). In health insurance the problem is reinforced by the fragmented U.S. system, in which people frequently transition between eligibility for different sources of coverage, including job-based insurance, Medicaid, and ACA exchanges. At key transitional moments (e.g., job loss or family status change), individuals must actively enroll in a new program to stay insured. People who get lost in the process – or simply stop taking action – slide into uninsurance by default.

How much would a more streamlined and automatic enrollment process matter for health insurance take-up and coverage outcomes? Despite an influential literature on auto-enrollment in settings like 401(k) pensions (e.g., Madrian and Shea, 2001),[2] we are not aware of prior work measuring its causal impact in health insurance programs.[3]

Further, the auto-enrollment literature has largely focused on take-up impacts, without modeling the *tradeoffs* of higher enrollment – a key issue in taxpayer-subsidized programs. Although greater enrollment may seem desirable, it also involves higher public spending. The classic insights of Nichols

[1] For evidence on the impact of larger insurance subsidies (and eligibility for subsidized Medicaid), see Dague (2014); Frean et al. (2017); Finkelstein et al. (2019b). A parallel literature has shown that financial *penalties* on uninsurance also increase coverage; see Chandra et al. (2011); Lurie et al. (2019); Fiedler (2020).

[2] The impact of auto-enrollment has also been found in many other settings, including: savings outcomes (Beshears et al., 2009; Chetty et al., 2014), organ donation programs (Abadie and Gay, 2006), welfare program take-up (Alatas et al., 2016), and education technology adoption (Bergman et al., 2020).

[3] Although auto-enrollment is used in some health insurance programs (e.g., into Medicare when individuals turn age 65), past research has not evaluated its *causal impact*, likely because of a lack of policy variation. There is a small health policy literature providing observational evidence on the use of auto-enrollment policies in Medicaid (DeLeire et al., 2012; Hoag et al., 2013) and in the Indian health system (Sood and Wagner, 2018).

and Zeckhauser (1982) show that enrollment ordeals may be optimal if they induce people with little benefit from a program to "self-screen" out of taking it up. There is an ongoing debate on whether ordeals improve "targeting efficiency" in this way – with some papers finding positive effects (Alatas et al., 2016; Dupas et al., 2016; Finkelstein and Notowidigdo, 2019a) and others finding little or negative impacts (Deshpande and Li, 2019; Homonoff and Somerville, 2021). Importantly, these debates have been framed around whether ordeals screen out people with *low value* or *benefit* from a program (equated with "improved" targeting) or those with higher benefit/value ("worsened" targeting).

In this paper, we contribute to these debates in three ways. First, we provide new evidence on the importance of hassles for health insurance take-up, providing some of the first causal evidence on auto-enrollment in health insurance. Second, we argue that the classic ordeals targeting debate has missed a key issue of central relevance for insurance: the role of *correlation* between benefit and cost types. When people with low value for a program also have low costs – a standard feature of insurance, where risk drives both – screening out low-benefit enrollees need not improve targeting. It may, in fact, screen out *low-risk* (healthy) people who policymakers are often most eager to enroll in insurance markets. Finally, we develop and estimate a simple model to assess the targeting tradeoffs of auto-enrollment, both on its own and relative to financial subsidies.

To study these issues, we draw on evidence from Massachusetts' pre-ACA health insurance exchange, a program that offered subsidized insurance to low-income adults without access to other coverage. Unlike the ACA today, Massachusetts initially used a form of auto-enrollment for its poorest enrollees, who qualified for fully subsidized ($0) coverage. Auto-enrollment was used for individuals who had applied and qualified for coverage (in step 1 of the enrollment process) but failed to respond when asked to choose a health plan (step 2 of the process).[4] Rather than leaving these non-responsive (or "passive") individual without coverage, the auto-enrollment policy *defaulted* them into a state-selected plan. While they could opt out or switch plans afterwards, they would have insurance by default. In essence, this policy sought to use defaults or "choice architecture" (Thaler, 2018) to reduce hassles and prevent people from "falling through the cracks" of the system.

Our first contribution is to measure the causal impact of auto-enrollment using a 2010 natural experiment in which Massachusetts suspended auto-enrollment, effectively adding active plan choice as an extra step required to enroll. 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 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 the 2010 change.

We find that adding a small hassle by stopping auto-enrollment has a major impact on health insurance take-up. Prior to 2010, one-third of low-income new enrollees join the exchange passively

[4] This type of passive non-response is surprisingly common in many settings and explains the power of auto-enrollment. In health insurance, passive non-response occurs among 45% of new enrollees in Medicaid managed care programs (Kaiser Family Foundation, 2015) and 84% of new enrollees in Medicare Part D's low-income subsidy program (Brot-Goldberg et al., 2021). In both settings, passive individuals are auto-enrolled in a state-selected plan. There is not evidence on how much enrollment would fall if people were forced to actively choose to get coverage.

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 qualified individuals simply fail to take up health insurance.

These findings indicate that modest hassles can be a major deterrent to health insurance take-up among the poor. This is true even though (as we show) passive enrollees appear to get meaningful benefits from insurance, including risk protection and coverage of predictable costs.[6] Our results are in line with growing evidence that even modest barriers substantially reduce insurance coverage among the poor. While past work shows this for financial premiums (Dague, 2014; Finkelstein, Hendren and Shepard, 2019b), our findings suggest the same is true for hassle barriers. Further, we find that auto-enrollment has an extremely large impact relative to other take-up policies studied in past work. Our estimates are an order of magnitude larger than the 1-4 percentage point take-up impacts of lower-touch "nudges" like outreach and enrollment assistance (Goldin et al., 2021; Domurat et al., 2021; Banerjee et al., 2021; Ericson et al., 2019) and 1.25-2 times larger than Massachusetts' uninsurance penalty (Chandra, Gruber and McKnight, 2011). They are similar to the impact of a $470 (or 57%) annual premium subsidy in the same Massachusetts exchange (Finkelstein, Hendren and Shepard, 2019b), whose analysis we replicate and use to estimate our model. These magnitudes suggest that auto-enrollment – and hassle reduction in general – may be an important policy lever for covering the remaining uninsured in America.

Our second contribution is to apply our causal evidence to the classic public economics debate about whether ordeals improve the "targeting" of who enrolls in public programs. Although ordeals targeting has been studied in many social programs, we are not aware of prior work assessing the classic Nichols and Zeckhauser (1982) logic in health insurance, one of the largest areas of public spending.

We point out that insurance programs share a feature that poses a challenge for the standard "self-screening" logic of ordeals: enrollee benefit and cost types are often strongly correlated, since both are driven by an individual's risk. This is a classic feature of insurance markets in models of adverse selection going back to Akerlof (1970) and Rothschild and Stiglitz (1976). For instance, in health insurance, the people with lowest benefit (demand) for insurance are often the young and healthy (e.g., Tebaldi, 2020), but the same people also cost much less to insure. This poses a problem for self-screening: an ordeal that deters people with *low value* for insurance also excludes precisely the *low-cost* people policymakers are most eager to enroll to stabilize market risk pools.

[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.

[6] We estimate that the foregone benefits are likely significant. Drawing on recent work on the value of insurance to the poor (Finkelstein, Hendren and Luttmer, 2019a), we estimate an average value of $550 to $1,300 over a typical 12-month enrollment spell. This is comparable to foregone benefits from failure to take up the EITC or SNAP (Bhargava and Manoli, 2015; Finkelstein and Notowidigdo, 2019b) and to the cost of choice errors among health plans (e.g., Abaluck and Gruber, 2011; Bhargava et al., 2017).

Formally, we show that in settings (like insurance) with correlated benefits and costs, screening out low-benefit types may not indicate favorable targeting – as it would in a setting with constant costs across enrollees. Instead, a more appropriate targeting index is an enrollee's *value-cost ratio*, which we show links closely to the marginal value of public funds (MVPF) metric of Hendren (2016). This has important implications for when ordeals are desirable. Even if ordeals do lead to "self-screening" out by low-value types – as in standard rational take-up models – they have an ambiguous impact on targeting as measured by the MVPF. Correlated enrollee benefit and public cost is a feature not only of insurance, but of transfer programs in general. Therefore, our analysis suggests a general concern with ordeals that may apply in a wide variety of settings where they are used.[7]

The idea that hassles screen out low-value, but also low-cost and low-risk enrollees emerges clearly in our empirical results. We find that passive individuals – the marginal group that no longer enrolls after auto-enrollment stops – are younger (by 4 years on average) and healthier (e.g., 33% less likely to be chronically ill, and 49% less likely to be severely chronically ill), and especially likely to be young men age 19-34 (a group sometimes called "young invincibles"). Consistent with their youth and health, they incur 44% lower medical spending per month – most of which (36%) is predictable by a medical risk score measure. Because of their lower costs, excluding passive enrollees results in a higher-cost market risk pool, with 15% higher average costs.

We further find that passive enrollees are more likely to be very low-income, to live in high-minority and disadvantaged neighborhoods, and to live near safety net hospitals and clinics. These results suggest that hassles reduce the distributional equity of (formal) health insurance take-up and are consistent with ideas that ordeals differentially affect the poor (Bertrand et al., 2004; Mullainathan and Shafir, 2013). But to the extent the poor and those nearby safety net hospitals have better access to informal insurance from charity care when uninsured (Mahoney, 2015; Garthwaite, Gross and Notowidigdo, 2018), they are also consistent with our overall findings. People with better informal insurance will naturally have lower (private) value for formal insurance, and their *net* cost of coverage – after subtracting savings on charity care – will also be lower.

Our paper's third contribution is to evaluate the tradeoffs of ordeals reduction via auto-enrollment, especially compared to subsidies. We consider a situation where a program has extra funds and can choose whether to expand coverage via auto-enrollment (for zero-premium enrollees) or larger subsidies (for higher-income groups) – a relevant issue in the ACA today and the reverse of Massachusetts' 2010 situation when it chose to cut auto-enrollment in the face of a budget shortfall. To infer the impact of subsidies and estimate our model, we draw on and extend the analysis of RD-style subsidy variation by Finkelstein, Hendren and Shepard (2019b) in the Massachusetts exchange. We adapt the MVPF metric of Hendren (2016) to the ordeals/take-up setting and use it to evaluate each policy.

This exercise yields two main results. First, consistent with our descriptive findings, the marginal

[7] Although straightforward, this point appears to have been largely missed in the ordeals literature, which tends to equate screening out *low-benefit* types with improved targeting. For instance, Finkelstein and Notowidigdo (2019a) define favorable targeting as increasing "the share of enrollees who are high-benefit enrollees." Likewise, in recent work discussing ordeals in health care, Zeckhauser (2021) writes, "Ordeals are thus justified to limit *low-benefit* users from consuming highly subsidized medical resources" (italics added).

(passive) enrollees brought in by auto-enrollment have both low value and cost relative to inframarginal (active) enrollees. However, their value-cost ratio is similar to or (in our main specification) slightly higher than active enrollees. Thus, based on an MVPF metric, ordeals do *not* improve program targeting, despite screening out low-benefit types. This suggests the empirical relevance of our theoretical point about ordeals targeting and adds to findings that there can sometimes be "backward sorting" in insurance markets (Bundorf et al., 2012; Marone and Sabety, 2022).

Second, we find that auto-enrollment and subsidies have similar targeting properties – both enroll a similar young, healthy, and low-cost marginal population – but differ in their cost and impact on *inframarginal* enrollees. Subsidies involve a significant transfer to inframarginals (via lower premiums) – which benefits them but involves an equal cost to the government – while auto-enrollment involves no new costs (or benefits) for inframarginals.

As a result, auto-enrollment is a much more "cost-effective" policy, in the sense of maximizing enrollment expansions given a fixed budget. We find that each extra $1 million in public spending covers 55-66% more people if used for auto-enrollment rather than larger subsidies. Therefore, a budget constrained government wishing to maximize take-up would want to prioritize auto-enrollment (and hassle reduction) over subsidies – though in practice, with less tight budgets these policies may be complementary.[8] 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. Empirically, we find that auto-enrollment's MVPF lies within the range of the subsidy changes we examine.

Overall, our paper's results are important for two reasons. First, they illustrate the large impact of hassles built into the U.S.'s fragmented and non-automatic health insurance system, especially for poorer Americans. The system is built on a model of an active consumer whose strong demand for insurance means that they will seek out public assistance and pay meaningful premiums to enroll. Growing evidence suggests that this model is incorrect. Instead, willingness-to-pay for insurance among the poor is strikingly low, with modest premiums leading to major take-up reductions (Dague, 2014; Finkelstein et al., 2019b) and complexities in Medicaid enrollment systems likewise reducing take-up and recertification (Wu and Meyer, 2021; Arbogast, Chorniy and Currie, 2022). When enrolled, consumers are highly inertial, sticking with their default plan even when actively choosing would be beneficial (Handel, 2013; Ericson, 2014; Polyakova, 2016; Brot-Goldberg et al., 2021). Our paper shows, 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 findings suggest that as long as take-up is voluntary, getting to universal coverage will likely require some form of automatic enrollment. More directly, they illustrate the surprising power of a feasible form of auto-enrollment – using defaults to remove the barrier of active plan choice – that has recently been considered or implemented in several states' ACA exchanges.[9]

[8] For instance, auto-enrollment may be more administratively (and politically) feasible when insurance is more subsidized – especially when it is free. Our paper does not speak to the larger challenges of implementing auto-enrollment in settings like the ACA Marketplaces, an issue of active discussion (e.g., Dorn et al., 2018; Young, 2019; McIntyre and Shepard, 2022).

[9] This includes Massachusetts, which reinstated a similar form of auto-enrollment in April 2022 (MassLegalServices, 2022).

Second, our paper contributes to the growing literature on optimal ordeals in welfare 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, 2019b) 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). Our results are consistent with self-screening on low value of insurance, but they also show why enrollee benefit/value is an insufficient statistic for optimal targeting. Our results, therefore, shed light on the major impacts and tradeoffs involved with simplifying program complexity, an issue of great theoretical and practical interest (Kleven and Kopczuk, 2011).

Outline of Paper Section 2 discusses the setting, the auto-enrollment policy, and our data. Section 3 shows our main results on the impact on enrollment, and section 4 presents results on targeting. Section 5 presents a model and applies our evidence to the framework to understand the public economics of auto-enrollment versus subsidies. Section 6 concludes.

2 Setting, Auto-Enrollment Policy, and Data

2.1 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.[10]

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

[10] 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.

enrollment.[11] 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, 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 1. 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.

2.2 Auto-Enrollment Policy and Timeline

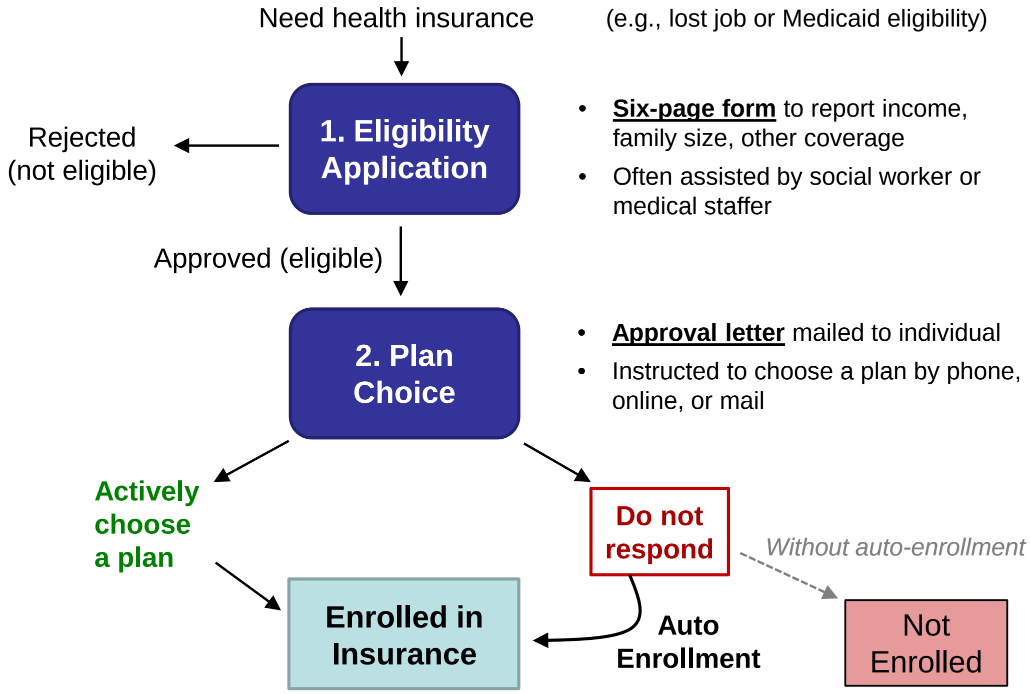
Auto-Enrollment Policy CommCare's auto-enrollment policy set the default outcome for people determined eligible (step #1 of the process) but who did not respond when asked to choose a plan (step #2; see Figure 1). The policy applied only to below-poverty enrollees, for whom all plans were free.[12] This 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, 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

[11] 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.

[12] 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).

Figure 1: 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.

respond. This lets us observe mode of enrollment (active vs. passive) directly in our administrative data. 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).

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.

Policy Timeline We use 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 at the start of 2009, and it was eager to avoid doing so again. Suspending auto-enrollment provided an alternative to reduce new 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 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. However, a potential concern is the possibility of other concurrent shocks or policy changes that affect enrollment in early 2010. Based on background research and discussions with the exchange administrator, there was only one other significant change around this time: 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 immigrant enrollees from our sample in all periods.[13] Aside from this, other enrollment-relevant variables did not change.[14] Nonetheless, to address any unobserved demand shocks, our method uses 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 largely 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

[13] 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.

[14] 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.

study; we therefore leave it for future work.

2.3 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 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 entering enrollee actively chooses a plan or is auto-enrolled. This lets us construct the key variables for our main analysis: monthly counts, characteristics, and outcomes for passive and active enrollees.[15]

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, excluding "re-enrollees" for whom auto-enrollment rules were different (see discussion above). Second, we limit the sample 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. Third, we exclude from our sample non-citizen enrollees who (as described above) faced an eligibility cutback in October 2009, to avoid conflating this with the effect of suspending auto-enrollment (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 do so for the following reasons. We exclude 2007 because of the different nature of auto-enrollment during the 2007 auto-conversion of the UCP (see discussion above). For DD regressions, we further exclude 2008 because of policy changes that affected the control group during mid-to-late 2008.[16] 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:

[15] 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.

[16] 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 (Chandra et al., 2011; Jaffe and Shepard, 2020). 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.

(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 described 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 duplicative private insurance, as a possible reason for failing to actively enroll. The APCD is well suited for this purpose because it lets us observe enrollment in 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.

Descriptive Statistics Figure 2 shows data on new enrollment per month in the treatment group (0-100% of poverty) over the main 2008-2011 period.[17] 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 4 where we compare active vs. passive enrollees.

3 Causal Impact of Auto-Enrollment Policy

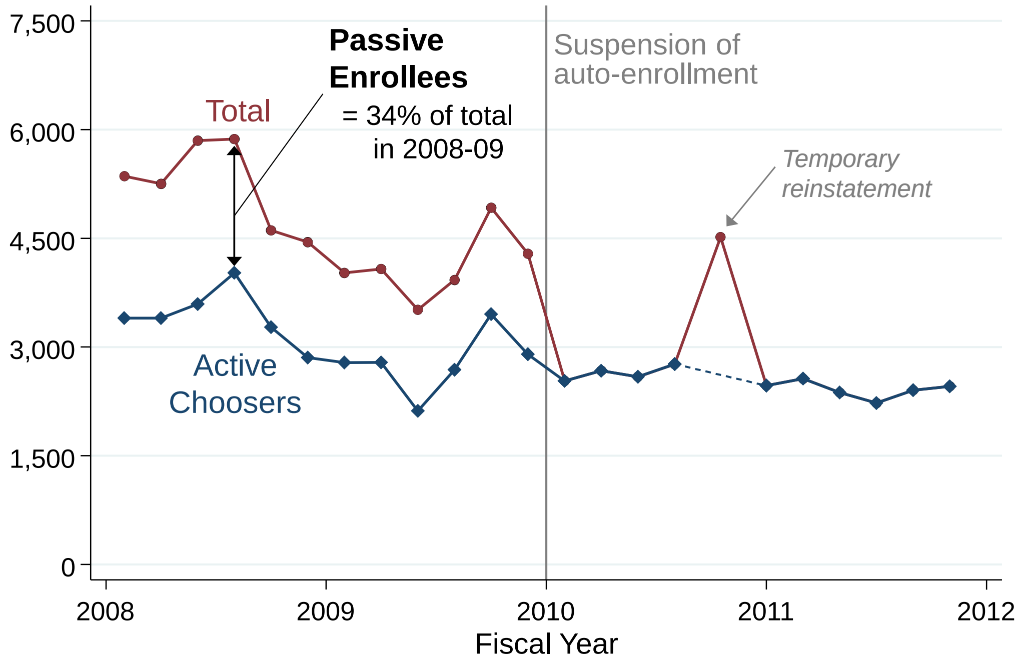
This section presents our estimates of the impact on take-up of suspending auto-enrollment in 2010. After presenting results in Section 3.1, we provide context on the magnitude in Section 3.2.

3.1 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:

[17] The points are bimonthly averages to smooth over noise; see Appendix Figure A.1 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.

Figure 2: Active vs. Passive New Enrollment into the CommCare Market



Note: The graph shows counts of new enrollees per month into the CommCare market 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.

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

where $NewEnr_{g,t}$ is (scaled) new enrollment for income group g (treatment or control) at time t. We run (1) on data from 2009-2011, excluding the period of temporary reinstatement of auto-enrollment at the end of 2010.[18] 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 3 plots the data for the regression in (1) 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%

[18] We start the analysis in 2009 (rather than 2007 or 2008) because of a subsidy change that affects the control group's premiums at the start of 2009. Subsidy rules are then steady from 2009-2011. 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.

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 3B 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.[19] 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 3B 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 following the policy change. As a further test, Appendix Figure A.2 shows that we see no evidence of compositional changes in the characteristics of active enrollees, which we would expect if some passive individuals 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.

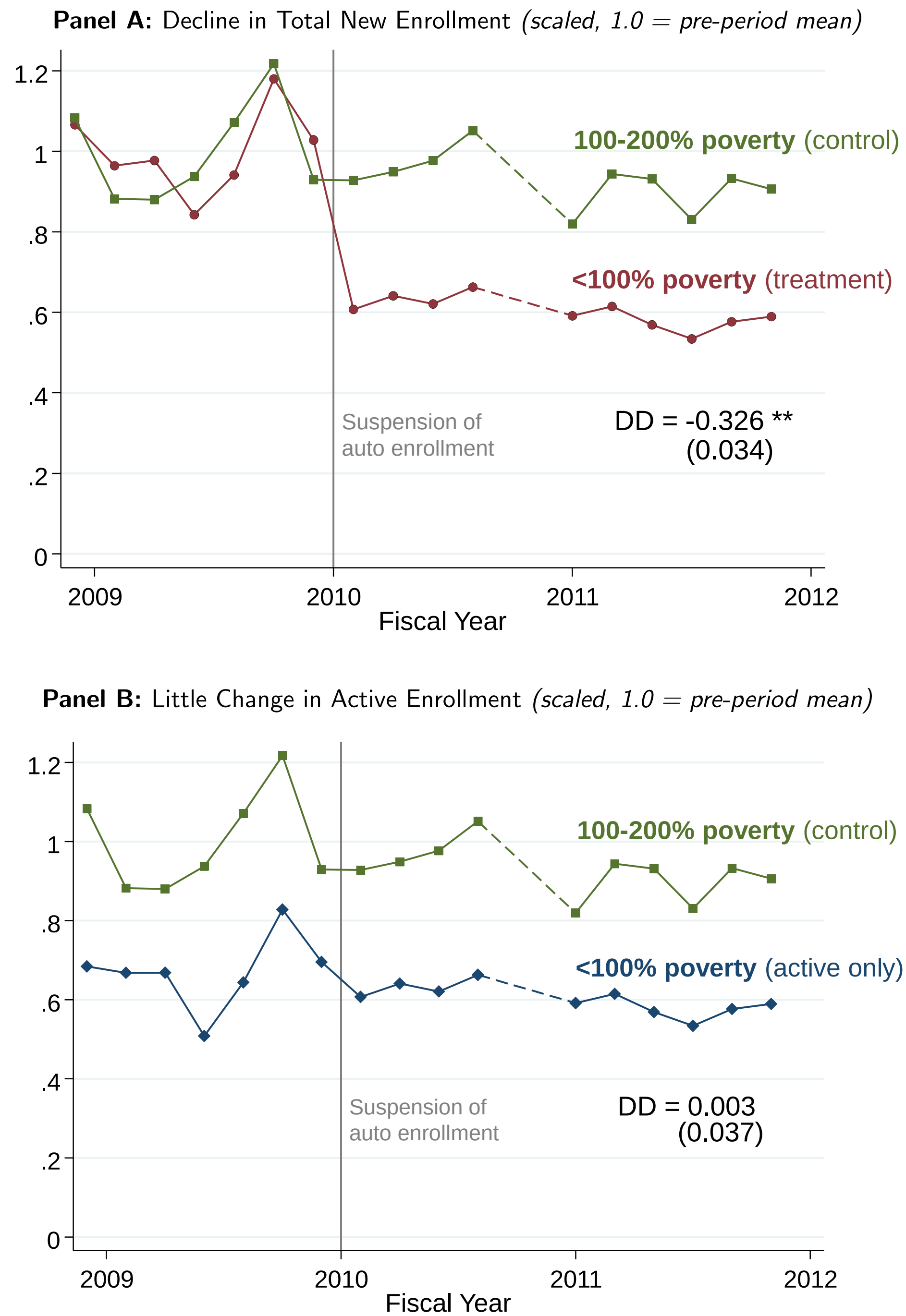
Effect on Steady-State Enrollment The results so far are on the *flow* of new enrollees, which falls immediately when auto-enrollment ends. The *stock* of total enrollment, however, changes more gradually, as existing enrollees exit while fewer new enrollees enter each month. To estimate the impact on 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.6).

Robustness: Alternate Specifications and Effects on Re-Enrollment The estimates shown in Figure 3 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, coded as a period when auto-enrollment is on.

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, despite some differences in the auto-enrollment policy for this group. We find that re-enrollment falls

[19] 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 3: Enrollment Impact of Auto-Enrollment's Suspension



Note: The figure shows scaled new enrollment per month into CommCare and estimates of the DD specification (1) 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.

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.

3.2 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. For instance, 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; Ericson et al., 2019; Myerson et al., 2021). 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).[20] Similarly, evidence from Aizawa and Kim (2020) suggests that a three-fold increase in government advertising of ACA marketplaces would increase market-level enrollment by 7.6% (or 1.3 percentage points). 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 premiums), 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; Jaffe and Shepard, 2020).[21] 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 qualified for CommCare and had incomes below poverty. Relative to this denominator, auto-enrollment's 14,900-person impact (see Appendix B.3) represents a 24% decline in the eligible uninsured population.

[20] 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.

[21] Evidence from the ACA – which involves a somewhat higher-income population than in CommCare – suggests smaller impacts of both subsidies and penalties. Frean, Gruber and Sommers (2017) find that each 10% point increase in non-group subsidies – about \$67 per month given the average premiums they report – increased Marketplace enrollment by about 10% relative to the pre-ACA non-group enrolled population (or by about 0.89% of the total population). The 32% effect of auto-enrollment would translate to a subsidy of >\$200 per month. Lurie, Sacks and Heim (2019) find a relatively small impact of increases in the ACA's uninsurance penalty, perhaps because it was not fully enforced.

4 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 4.1 provides descriptive evidence on targeting implications, comparing marginal (passive) vs. inframarginal (active) enrollees on characteristics related to the value and cost of insurance. Section 4.2 shows evidence that auto-enrollment is unlikely to be (invalidly) enrolling individuals with duplicate private health insurance. Section 4.3 assesses mechanisms, both rational and behavioral, for why a small hassle deters so many people from taking up free coverage.

4.1 Targeting Implications and Impact on Market Risk Pool

To study the targeting implications of auto-enrollment – i.e., estimating its marginal vs. inframarginal enrollees – we employ two methods. The first is motivated by our finding in Section 3.1 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 means that *observed passive* enrollees (prior to 2010) are also *marginal* enrollees who would not have enrolled without the policy in place.[22] Thus, we are in the fortunate position of 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, restricting attention to the main 2008-09 period[23] and controlling for entry timing using cohort fixed effects.[24]

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:

[22] 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 3.1 suggests that there are few if any conditional passives in our setting.

[23] As discussed in Section 2.2, the nature of auto-enrollment differed in early 2007 when the state auto-enrolled people from its pre-reform Uncompensated Care Pool. Appendix C.5 analyzes this period to see if auto-enrollment had different targeting implications. Interestingly, it finds very similar results as those for the main 2008-09 period shown in Table 1.

[24] 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).

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.

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.[25] Figure 4 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.[26]

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.[27] 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.

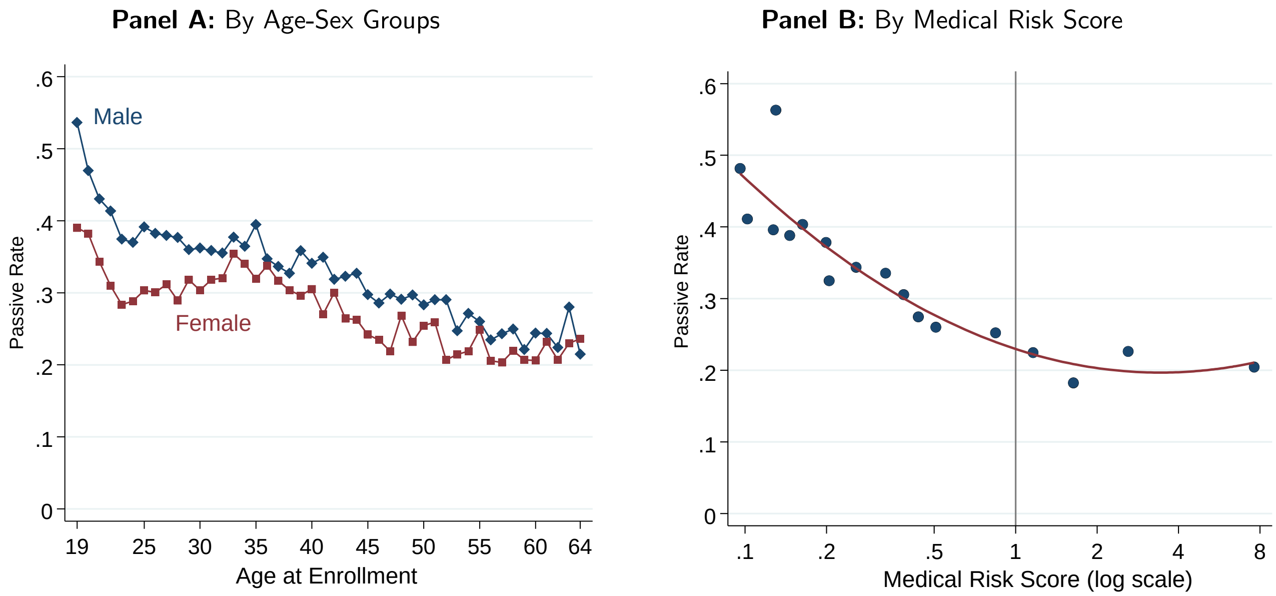
In line with their residence in lower-income neighborhoods, passive enrollees are also more likely to live nearby (within 2 miles) a safety net hospital or community health center. This proximity raises

[25] 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. A natural question is whether measured risk differences are driven by passive enrollees' shorter duration enrolled (see point #4 below). In practice, this does not appear to be a major source of bias; see Appendix C.1 for analysis showing that health differences are robust to the measurement period.

[26] 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 ($335 vs. $364 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.

[27] 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.

Figure 4: 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.

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 5. 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 How do these findings relate to the classic Nichols and Zeckhauser (1982) question of whether ordeals improve the targeting of public programs? In one sense, they are consistent with the "self-screening" hypothesis that ordeals screen out people who value or benefit from a program less. In welfare programs, low "benefit" is often equated with higher incomes – based on the idea that higher-income people need less support (or perhaps are distorting their labor supply downward to qualify). In health insurance, however, value is primarily a function of medical risk, as the healthy obtain less benefit from coverage of their lower expenses.[28] Thus, by this metric, the active enrollment ordeal does screen out low-benefit (i.e., healthy) individuals. Similarly, shorter-duration enrollees may have a shorter period of need for public coverage (e.g., between jobs); they therefore obtain less benefit over their full spell relative to a one-time hassle cost.

[28] By contrast, value is only weakly related to income; indeed, many estimates suggest low-income people have lower demand for health insurance (e.g., Tebaldi, 2020; Finkelstein et al., 2019b).

On the other hand, this self-screening may not be *socially* optimal. Young and healthy people cost less to insure, and short-duration enrollees use fewer months of public subsidies. Excluding passive enrollees from the market results in a higher-cost risk pool (see Figure 5 below); as a result, policymakers are often particularly interested in attracting and retaining these healthy individuals.

The key point, as we formalize in Section 5 below, is that value and cost are strongly correlated in insurance markets. Screening out low-value types also screens out low-cost types, and it is unclear whether they have low *value-cost ratios*. Additionally, screening out lower-income individuals reduces distributional equity and is consistent with concerns that ordeals are particularly costly to the poor (c.f. Bertrand et al., 2004; Deshpande and Li, 2019; Homonoff and Somerville, 2021). Finally, as we discuss in the next section, there are important externalities of formal insurance that may not be internalized in private value/demand. Thus, for these reasons, it is unclear that screening out low-value types is desirable in the insurance context.

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 (1) 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.

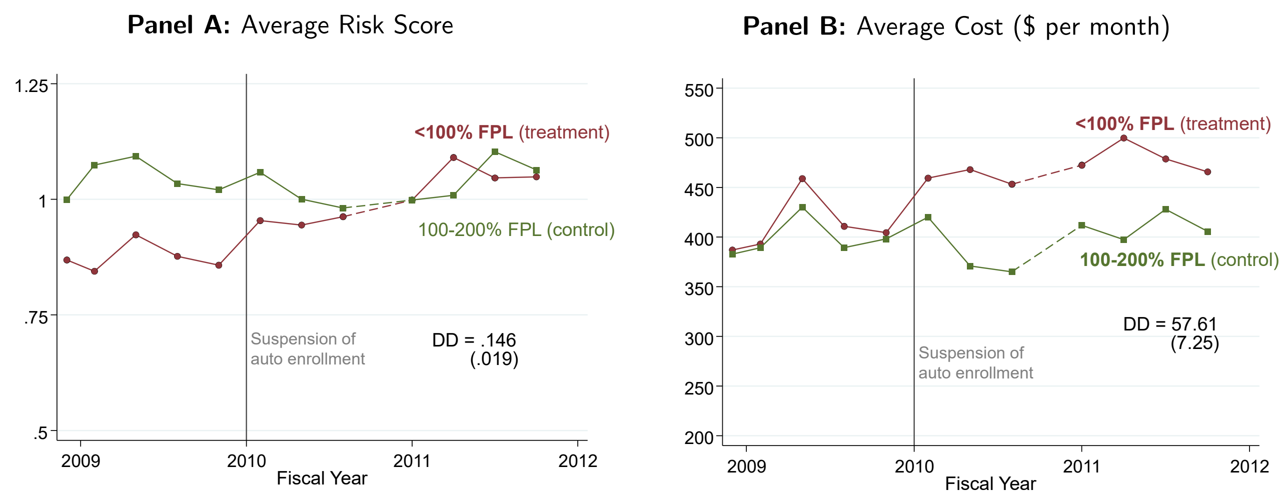
Figure 5 shows the raw data and DD estimates for two key risk pool variables: average risk score (panel A) and average cost (panel B) for new enrollees. There is a clear increase in both measures for the treatment group (red) relative to controls (green) after auto-enrollment is suspended.[29] The effects 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.[30] 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 5.[31]

[29] 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 Chandra, Gruber and McKnight (2014)).

[30] 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 3.

[31] 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.

Figure 5: 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 (1) 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.

4.2 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 "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.[32] We define the "duplication rate" as the share of CommCare enrollment months during which the member was simultaneously enrolled in other private insurance.[33] 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

[32] 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.

[33] 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.

of enrollment spells when auto-enrollment occurs (see Appendix Figure A.12). 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.

4.3 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.

4.3.1 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 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.10 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 4 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.[34]

[34] We observe that 25% of passive enrollees take a regular prescription medication every month they are enrolled, with

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, 2019a) and from losses due to insurance plan choice errors (Abaluck and Gruber, 2011; Bhargava, Loewenstein and Sydnor, 2017).

4.3.2 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.[35] Further, the passive enrollment rate is unrelated to the choice set size, which varies across areas due to selective insurer entry. Appendix Table A.6 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.[36] 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

an average cost of $45 per month. Over a typical 12-month enrollment spell, these prescription costs alone would add up to $540.

[35] 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).

[36] 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.

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.[37] 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 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.

5 Model and Analysis of Policy Tradeoffs

The paper has so far shown descriptive evidence of the large take-up and targeting impacts of ordeals reduction via auto-enrollment. In this section, we apply this evidence to a simple model of the government's optimal enrollment targeting problem. The goal of this exercise is twofold. First, it lets us formalize and test our paper's central targeting story: that hassles screen out low-value, but also low-cost enrollees. We show that screening out *low-value* types – the standard measure of "favorable" targeting used in past work – need not imply screening out people with low *value-cost ratios*, which we show is a more robustly valid measure of desirable targeting. Second, the exercise lets us quantify

[37] Immigrants were excluded from our main analysis sample, as discussed in Section 2.3. For this analysis, we augment the main sample to re-include them.

the policy tradeoffs involved with auto-enrollment, and to compare them to other take-up policies like subsidies.

Sections 5.1-5.2 develop a simple model of the take-up and targeting problem, building on the classic ideas of Nichols and Zeckhauser (1982) and the MVPF framework of Hendren (2016). Section 5.3 discusses how we empirically implement the model in our data. and Section 5.4 presents and discusses results.

5.1 Targeting Model Setup

Benefits and Costs of Insurance Consider a population of individuals (i) who, as in our empirical setting, qualify for a subsidized health insurance program and would otherwise be uninsured. The program generates both benefits and costs that vary across potential enrollees, creating a rationale for targeting. Insurance generates two types of benefits. First, individuals receive utility (in dollars) $V_i \geq 0$ from being insured (e.g., due to risk protection and health benefits). The program may charge an enrollee premium, $P \geq 0$, which is \$0 in our main analysis but which we allow for to model subsidies. After subtracting this fee, enrollees' net program utility is $V_i^{Net} \equiv V_i - P$. Second, insurance generates external social benefits, E_i, which includes both general spillovers (e.g., reduced communicable disease externalities in a pandemic) and savings on uncompensated care borne by private providers (Garthwaite, Gross and Notowidigdo, 2018).[38] These externalities are a key rationale for subsidizing insurance, rather than giving cash transfers (Finkelstein, Mahoney and Notowidigdo, 2018). The net benefit of insuring i is $V_i^{Net} + E_i$.

Along with benefits, the expected cost of insurance, C_i, also vary across enrollees due to heterogeneity in medical risk and spending. We assume these costs are fully borne by the government – either through direct financing or via contracting with insurers in a competitive (zero-profit) market.[39] Offsetting these direct costs are the premium (P) and any fiscal externalities of insurance, FE_i, which includes savings on *publicly* paid uncompensated care. The "net" public cost of covering each individual i is $C_i^{Net} \equiv C_i - FE_i - P$.[40]

Take-Up Policies: Auto-Enrollment and Subsidies Individuals enroll in insurance if their individual benefit, V_i, exceeds any premium owed (P) plus a "friction" η_i involved with active enrollment – i.e., they enroll if $V_i \geq P + \eta_i$. Importantly, we think of η_i as a behavioral friction (e.g., inattention or

[38] The key feature of E_i is that it captures spillover impacts on *private non-recipients* – i.e., people other than the insured person (whose benefits are in V_i) and other than the impact on government budgets (which are captured in fiscal externalities, discussed below).

[39] More generally, the incidence between government vs. insurers depends on the nature of risk-adjusted payments and insurer competition. Mechanically, when an enrollee i is added and joins plan j, the government pays a risk-adjusted price $\varphi_i P_j$ (where φ_i is the risk score and P_j is the insurer's base price). The insurer earns marginal profit of $\pi_{ij} = \varphi_i P_j - C_i$ – the sign/level of which depends on how well risk adjustment matches cost variation. Beyond this mechanical effect, auto-enrolling healthy individuals will lower insurer's average costs, which may lead insurers to *compete* down prices. The net incidence depends on the sum of the mechanical and competitive effects.

[40] In what follows, we assume $C_i^{Net} > 0$ so that there is a real fiscal tradeoff of additional enrollment. If $\overline{C}^{Net} \leq 0$ on average for any relevant groups (e.g., auto-enrollees), boosting take-up is a win-win policy with an infinite MVPF (Hendren and Sprung-Keyser, 2020), which would make a targeting analysis unnecessary.

misunderstanding) that affects take-up but does not impose a welfare-relevant cost on individuals who actively enroll.[41] Moreover, η_i is likely to be highly heterogeneous, with $\eta_i \approx 0$ for attentive and informed individuals, while $\eta_i > 0$ for inattentive individuals.

The government can use two policies to affect take-up. First, it can use *auto-enrollment* to remove the behavioral friction, η_i. As in our empirical setting, we focus on auto-enrollment for zero-premium enrollees ($P = 0$). In this case, everyone with $V_i \geq 0$ enrolls, which in our model implies full take-up. The set of marginal enrollees is $AE = \{i : V_i < \eta_i\}$. Note that consistent with our findings, the model suggests auto-enrollees will be people with relatively low value of insurance (V_i) and/or high frictions (η_i).

Alternatively, the government can use *subsidies* to reduce premiums for (actively enrolling) individuals. We consider an extra subsidy of size ΔS, which reduces enrollee premiums from $P_0 = (P + \Delta S)$ to $P_1 = P$. The set of marginal enrollees is $\mathcal{S} = \{i : V_i - \eta_i \in [P, P + \Delta S)\}$.

5.2 Optimal Targeting and the MVPF of Take-Up Policies

To understand targeting, it helps to start with a budget-constrained government's *optimal targeting* problem if it had full information and the ability to screen at the individual level. In this problem, the government chooses whether to enroll each individual i (denoted by $D_i \in \{0, 1\}$) to maximize $SocialBenefit = \sum_i \left(V_i^{Net} + E_i\right) \cdot D_i$ subject to budget constraint $\sum_i C_i^{Net} \cdot D_i \leq Budget$. The solution to this problem is to enroll any individual for whom:

$$R_i \equiv \underbrace{\frac{V_i^{Net} + E_i}{C_i^{Net}}}_{\text{Value-Cost Ratio}} \geq \lambda \tag{2}$$

where $\lambda > 0$ is the Lagrange multiplier on the government budget constraint, and R_i is the *"value-cost ratio."*[42] This value-cost ratio is a useful targeting index that captures how a budget-constrained government would optimally like to prioritize enrollees if it had full information. Importantly, targeting is a function of the *ratio* of enrollees' total social value of insurance, $V_i^{Net} + E_i$, to their net costs C_i^{Net}.

The targeting index R_i is closely connected to the MVPF of auto-enrollment, which following Hendren (2016) is the ratio of a policy's total social benefit to its net public cost. For auto-enrollment, this is:

$$MVPF_{AE} = \frac{\Delta SocialValue}{\Delta GovtCost} = \frac{\sum_{i \in AE} \left(V_i^{Net} + E_i\right)}{\sum_{i \in AE} C_i^{Net}} = \frac{\overline{V}_{AE}^{Net} + \overline{E}_{AE}}{\overline{C}_{AE}^{Net}} \tag{3}$$

where variables $\overline{X}_{AE}$ are averages for auto-enrollees ($i \in AE$). Notice that $MVPF_{AE}$ is simply a

[41] Eliminating the ordeal, therefore, has no welfare benefit (or harm) for active enrollees. In this sense, active enrollment is *a priori* more likely to be an optimal ordeal than those that impose a real hassle cost on inframarginal enrollees. We think of it as an example of a "micro-ordeal" that recent work finds to be a relatively promising way of allocating scarce program resources (Dupas et al., 2016).

[42] To see this, note that the Kuhn-Tucker conditions for the government's problem are that $D_i \cdot \left(V_i^{Net} + E_i - \lambda \cdot C_i^{Net}\right) \geq 0$, where λ is the Lagrange multiplier. For any i who is enrolled ($D_i = 1$), this implies (assuming that $C_i^{Net} > 0$) that $\frac{V_i^{Net} + E_i}{C_i^{Net}} \geq \lambda$. Note that for $\lambda = 1$, this collapses to a standard Kaldor-Hicks efficiency test whether $V_i^{Net} + E_i \geq C_i^{Net}$. However, equity concerns might merit a lower threshold $\lambda \in (0, 1)$, while severe budget constraints might require $\lambda > 1$.

(net-cost-weighted) average of R_i among auto-enrollment's marginal enrollees. A policymaker will wish to implement auto-enrollment if $MVPF_{AE}$ exceeds a threshold for alternate use of funds.

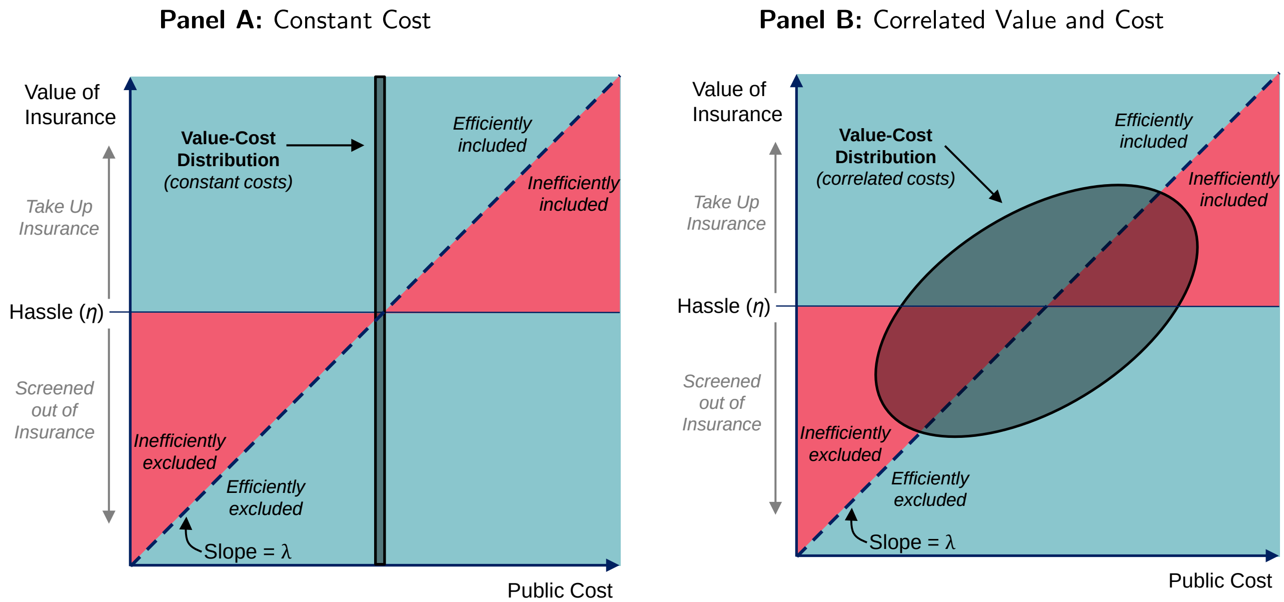
Value-Cost Correlation and Ordeals Targeting Errors Equation (3) shows the close link between the MVPF of ordeals reduction and its targeting properties. This is consistent with the basic message of Nichols and Zeckhauser (1982): the optimality of an ordeal depends on whether it screens out ("targets") more or less desirable enrollees. However, a key point of our paper is that "desirability" should be assessed based on an enrollee's social *value-cost ratio*, R_i. This differs from what we call the "standard ordeals logic," which thinks of targeting in terms of screening out people with low benefit or value, V_i^{Net}. Moreover, there is a natural reason to think that ordeals *will* screen out low-value types: under rational choice, people endure the ordeal if they perceive that the program's value (V_i^{Net}) exceeds the hassle cost (η_i). If the hassle cost is constant across individuals, then marginal people will be precisely those with low-values, i.e., $V_i^{Net} < \eta$.

Prior work critiquing this standard logic has questioned the rational take-up and constant hassle cost assumptions, arguing that ordeals may differentially impact the poor (Bertrand et al., 2004; Mullainathan and Shafir, 2013) or that people may mis-perceive program benefits (Finkelstein and Notowidigdo, 2019a). Our model, however, points to a complementary and deeper challenge: even when ordeals *do* screen out low-value types (as in rational take-up models), they may not improve targeting if *costs vary* and are *correlated with value*. Put simply, low-value types may not have low value-cost ratios (R_i) when the two are strongly correlated, e.g. because both are driven by risk. Correlated value and cost are a standard feature of adverse selection models going back to Akerlof (1970) and Rothschild and Stiglitz (1976); it is natural, therefore, that the same phenomenon should emerge with ordeals targeting.

Figure 6 visualizes this point about correlated value and costs in a stylized setting with rational take-up, no externalities ($E_i = 0$), and with a constant ordeal cost ($\eta_i = \eta$) – which as discussed above, implies a best-case scenario for the standard ordeals logic. Individuals vary along two axes: their public cost (C_i^{Net}) on the x-axis and their value of insurance (V_i) on the y-axis. Optimal targeting would involve enrolling people with $R_i = V_i/C_i^{Net} \geq \lambda$, corresponding to people above the diagonal dashed line. In practice, an ordeal of size η leads to enrolling people with $V_i \geq \eta$, indicated by people above the solid horizontal line. The blue areas indicate correct targeting, while the red areas indicate errors.

How well ordeals work depends on the distribution of enrollees across blue vs. red areas. Panel A shows this distribution for a program with roughly *constant costs* – think, for example, of slots in a free childcare program – indicated by the gray area being a vertical bar. If the hassle is chosen optimally, the ordeal can target nearly perfectly. On the other hand, Panel B shows a case where value and costs are positively correlated, indicated by the diagonal-tilted oval shape of the gray area. As a result, a significant part of the distribution lies in targeting error (red) regions. Thus, even in this best-case scenario, ordeals involve significant targeting errors. Indeed, depending on the shape of the distribution, ordeals may do more harm than good, and it may be optimal to either enroll everybody

Figure 6: Value-Cost Correlation and the Optimality of Ordeals Targeting



Note: The figure illustrates how correlated value and costs pose a challenge for the standard ordeals targeting logic, even in a stylized best-case scenario. Potential enrollees (i) are distributed along two dimensions: value V_i (y-axis) and net public cost C_i^{Net} (x-axis). Optimal targeting is for people with $V_i/C_i^{Net} \geq \lambda$ enrolling, corresponding to types above the diagonal dashed line. The ordeal involves a constant hassle cost, η, and everyone with $V_i \geq \eta$ enrolls. Panel A shows that when program costs are constant across enrollees – so the shaded value-cost distribution is a vertical line – an optimal ordeal generates perfect targeting. Panel B shows with (positively) correlated value and costs that any hassle η involves substantial targeting error.

or nobody.[43] Whether they do so in practice is an empirical question, which we assess with our model estimates below.

Comparison to Subsidies

How does the MVPF of auto-enrollment compare to those of a subsidy? Consider a subsidy of $\Delta S > 0$ that lowers premiums from $P_0 = (P + \Delta S)$ to $P_1 = P$. This provides a cash transfer of ΔS to the $D_0 = D(P_0)$ existing enrollees, and it also increases enrollment by ΔD_S. Defining $\overline{V}_S^{Net}$ as the average value of these marginal enrollees (and likewise for $\overline{E}_S$ and $\overline{C}_S^{Net}$), the MVPF of a subsidy is:

[43]The possibility that ordeals worsen targeting even with fully rational take-up and no externalities is closely related to the non-optimality of vertical choice with price-based screening in insurance markets, a point highlighted in recent work (Marone and Sabety, 2022). Sorting may be improved if ordeals (or prices) can be set higher for high-cost enrollees (Bundorf, Levin and Mahoney, 2012), but this is typically not done for enrollment hassles because doing so would be inequitable to the sick. In a different context, the fact that "prior authorization" hassles to obtain prescription drugs are differentially targeted at high-cost drugs may explain recent work finding that they yield savings well in excess of their costs (Brot-Goldberg et al., 2022).

$$MVPF_S = \frac{\Delta D_S\left(\overline{V}_S^{Net}+\overline{E}_S\right)+D_0\Delta S}{\Delta D_S\overline{C}_S^{Net}+D_0\Delta S} = \underbrace{\kappa_M \times \left(\frac{\overline{V}_S^{Net}+\overline{E}_S}{\overline{C}_S^{Net}}\right)}_{\text{MVPF of marginals}} + \underbrace{(1-\kappa_M)\times 1}_{\text{Transfer to inframarginals}} \tag{4}$$

where $\kappa_M \equiv \frac{\Delta D_S\overline{C}_S^{Net}}{\Delta D_S\overline{C}_S^{Net}+D_0\Delta S}$ 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).

Comparing (3) and (4), the MVPF of auto-enrollment vs. subsidies depends on their relative targeting properties (average R_i for their marginal enrollees) and whether the MVPF of additional insurance is less than or greater than 1 (the MVPF of a cash transfer). Intuitively, auto-enrollment works like a *highly targeted subsidy*, increasing take-up by removing frictions without any costs (or benefits) for inframarginals. By contrast, subsidies combine a targeted take-up policy with a cash transfer to inframarginals, which may be either more or less desirable than increasing insurance take-up.

5.3 Empirical Implementation

We now turn to empirically implementing the targeting analysis of auto-enrollment and subsidies, based on the framework above. To do so, we use our causal evidence on the impact of auto-enrollment from Sections 3-4. We also analyze the impact of subsidies in the same Massachusetts exchange, replicating and extending the method of Finkelstein, Hendren and Shepard (2019b). Using this evidence, we can directly measure the number of marginal enrollees and their average costs ($\overline{C}_{AE}$, $\overline{C}_S$), as well as analogs for inframarginals. To estimate the MVPF, however, we also need to estimate three additional quantities: (1) enrollee value of insurance, V_i, (2) social spillovers, E_i, and (3) fiscal externalities, FE_i, which let us calculate net costs of insurance. While not directly observable in our data, we can provide estimates by combining what we do observe with information from other studies and data sources. In what follows, we describe our strategy for estimating each term.

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.[44] 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, $p < 1$, of their medical bills, with uncompensated care covering the other $1 - p$. Thus, uncompensated care costs

[44] 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.

equal:

$$C_i^{UC} = \left(\frac{1-p}{1+MH}\right) C_i \tag{5}$$

Estimating C_i^{UC} requires values for p 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 $p = 0.21$, both of which we treat as constant across enrollees.[45] 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 $p = 0$ (full uncompensated care) and $p = 1$ (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, $\sigma_G \in [0, 1]$, of costs, which implies:

$$FE_i = \sigma_G \cdot C_i^{UC} \qquad \text{and} \qquad E_i = (1 - \sigma_G)\, C_i^{UC}. \tag{6}$$

Note that this assumes no other externalities of insurance besides uncompensated care, which is a conservative assumption.[46] To estimate σ_g, we draw on the evidence from Garthwaite, Gross and 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 $\sigma_G = 0.635$, 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. To make progress, we use price variation for higher-income CommCare enrollees (150-250% of poverty), 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 age-sex-risk group level). This exercise has clear limitations

[45] 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 $p = 569/2700 = 0.21$. We treat MH and p 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 MVPF.

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

– it requires assuming that demand reveals true WTP for higher-income enrollees (no behavioral biases) and that WTP can be extrapolated to our lower-income population. Nonetheless, this gives us a useful benchmark based on revealed preference in the same setting.

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 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. $V_i = E\left[D_{g(i)}(s)\right]$, where the average is over s.[47] 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.[48]

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., $V_i = \kappa \cdot C_i$ 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 (5). This implies $V_i = \left(\frac{p}{1+MH}\right) C_i = 0.16 C_i$ given the values of $p = 0.21$ and $MH = 0.333$.

[47] 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.

[48] Briefly, unobserved sorting relates to the range of s over which we average to calculate $V_i = E\left[D_{g(i)}(s)\right]$. 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.

5.4 Results: Targeting and Policy Tradeoffs

Targeting Impacts of Auto-Enrollment Table 2 shows our model's baseline estimates of the targeting impact of auto-enrollment, comparing value and cost estimates for active vs. passive enrollees in our main sample (the same as in Table 1 above). Consistent with the descriptive findings in Section 4, passive enrollees have lower private value of insurance at $93 per month – about 28% below that of active enrollees ($129). Because spillover benefits are also larger for active enrollees – which occurs because they are sicker, so use more uncompensated care when uninsured – the *total* social benefit is also substantially lower for passive enrollees. This finding that passive enrollees have lower value of insurance than actives holds across every sensitivity analysis we consider (see Appendix Table A.8). Our estimates, therefore, robustly suggest that the active enrollment ordeal screens out "low-benefit" types, which is the typical way the literature has thought about "favorable" targeting.

But although they have lower value of insurance, passive enrollees also have much lower costs. As a result, they have a *higher* value-cost ratio at 1.00 (versus 0.85 for active enrollees). Mechanically, this occurs because passive enrollees' proportional cost difference (-44%) exceeds the difference for enrollee and social value (-28% and -34%). Thus, in our baseline estimates, the active enrollment ordeal *worsens* targeting. This illustrates precisely the targeting ambiguity discussed in the theory when values and costs are correlated. Even when ordeals succeed in screening out low-benefit types, they may not improve targeting, correctly measured.

Appendix Table A.8 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. We also generally find that passive enrollees have similar or larger value-cost ratios, though this finding reverses if unobserved sorting on WTP is strong enough (as in the "perfect sorting" specification). Thus, the robust take-away is not that ordeals necessarily worsen (or improve) targeting. Instead, the take-away is that the correlation between value and costs injects greater ambiguity into ordeals targeting than is typically understood. Even when ordeals screen out low-benefit enrollees, they may not be optimal.

Comparing Auto-Enrollment and Subsidies Table 3 compares the implications of auto-enrollment (column 1) vs. the three subsidy changes in our data: reducing premiums from $39 per month to $0 (column 2), from $77 to $39 (column 3), and $116 to $77 (column 4).[49] We start by discussing the enrollment and cost variables, which do not rely on our demand model's assumptions. Interestingly, all four take-up policies involve similar enrollment impacts of +32-36%, implying that they are similar-scale interventions. They also all enroll a similar set of low-cost marginal enrollees, with medical costs of $196-281 per month – well below the CommCare market average ($370) and also the cost of inframarginals in each case.[50] Indeed, after subtracting premiums owed, the "gross subsidy" for

[49] Additional detailed statistics for this comparison are shown in Appendix Table A.9.

[50] Inframarginals' average costs are $408 for auto-enrollment, $380 for the subsidy at 150% of poverty, $386 for the subsidy at 200% of poverty, and $343 at 250% of poverty.

Table 2: Targeting Impact of Auto-Enrollment

Value or Cost Variable ($/month)	**Active Enrollees** (1)	**Passive Enrollees** (2)
Social Benefits		
Value to Enrollees (demand estimate, V_i)	$129	$93
Spillovers: Private Uncomp. Care Savings (E_i)	$88	$49
Total Benefits	**$217**	**$143**
Public Costs		
Medical Spending (gross costs)	$408	$228
Fiscal Externality: Public Uncomp. Care Savings (FE_i)	-$154	-$86
Net Public Cost (C_i^{Net})	**$255**	**$142**
Value-Cost Ratio (R_i)	***0.85***	***1.00***

Note: The table 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). The sample is our main 2008-09 new enrollee sample in the below-poverty group, just as in Table 1. See Section 5.3 for the model estimation method. Enrollee value comes from our demand estimates, using the specification with unobserved sorting equal to observed sorting on WTP.

marginal enrollees is remarkably similar across policies, ranging from $196 to $229. After subtracting fiscal externalities of uncompensated care savings, net costs are also similar (varying from $98 to $142).

However, the policies differ much more 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. This occurs because there are about three inframarginals for every marginal enrollee, which multiplies the $38-39 premium discount. As a result, auto-enrollment is a much more cost-effective policy to increase 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.

Of course, cost and cost-effectiveness are not the only lens to compare policies. Bringing in our demand model estimates, we can compare them based on social benefits and MVPF. Here, we see more variation across the policies, with the value-cost ratios for marginal enrollees ranging from 0.51 (the $39 to $0 subsidy) to 1.60 (the $116 to $77 subsidy), with auto-enrollment at 1.00 lying in the middle of this range. For the overall MVPF – which for subsidies, is a weighted average of marginals' value-cost ratio and the MVPF of a cash transfer (see equation (4)) – auto-enrollment (which has an MVPF = 1.0) again lies in the middle of the range for the subsidies (0.74-1.24).[51] Although these estimates naturally rely on the model assumptions, they suggest that subsidies and auto-enrollment are roughly comparable in terms of MVPF.

[51] Appendix Table A.8 gives sensitivity analysis on auto-enrollment's MVPF by reporting the V/C ratio for passive enrollees (column 8), which equals the MVPF of auto-enrollment. This MVPF ranges from 0.41 to 1.30 across specifications.

Table 3: Policy Comparison: Auto-Enrollment vs. Subsidies

	Auto Enrollment	**Subsidy Increase (↓ premiums)**		
		$39 to $0	$77 to $39	$116 to $77
	0-100% FPL	*150% FPL*	*200% FPL*	*250% FPL*
	(1)	(2)	(3)	(4)
Panel A: Marginal Enrollees				
Enrollment Impact	32%	34%	36%	32%
Social Benefit ($V_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 (5.3)) 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 5.3 to marginal enrollees' costs.

Summary and Discussion Overall, the model analysis suggests, consistent with our descriptive findings, that auto-enrollment expands insurance take-up while enrolling low-value but *not* low-MVPF marginal enrollees. The strong correlation between value and costs of health insurance meaningfully impacts targeting conclusions, making ordeals reduction much more favorable than it would appear in the standard view. Compared to subsidies, auto-enrollment is a much more cost-effective policy for expanding enrollment and similar in terms of MVPF. This suggests that a budget-constrained government wishing to maximize coverage may wish to prioritize auto-enrollment (and ordeals reduction in general) for people eligible for free coverage, even over reducing premiums for higher-income enrollees.

This analysis is highly relevant to understanding policy 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. Of course, our evidence speaks to a particular type of auto-enrollment for people *known* to be eligible for subsidized coverage – a similar group as those targeted in ACA take-up "nudging" experiments (Domurat et al., 2021; Ericson et al., 2019). It does not address its feasibility or impacts for (higher-income) people not eligible for $0 insurance, which would require a way to auto-collect premiums (e.g., via taxes). Nonetheless, our evidence suggests that if policy complexities can be overcome, auto-enrollment may have a large impact on take-up and targeting – especially among the young, healthy, and low-income populations most likely to be uninsured today.

6 Conclusion

This paper studies the impacts and public economic tradeoffs of reducing enrollment hassles for public health insurance through an automatic enrollment policy. Although auto-enrollment has been studied extensively in settings like pensions and savings programs, our paper provides the first direct causal evidence on its impact for health insurance. We highlight the conceptual link between auto-enrollment and the general class of "ordeals-reducing" policies for social programs. Understanding how reducing ordeals affects targeting efficiency, a question initiated by Nichols and Zeckhauser (1982), is an area of significant interest in economics but has not been studied in health insurance. More generally, we address whether reducing ordeals through auto-enrollment is a cost-effective and high-MVPF investment in greater coverage, or whether alternate policies like subsidies may do better.

Our paper has two main findings. First, using a natural experiment in Massachusetts, we find that minor hassles have a major impact on health insurance take-up. The simple hassle of being required to choose a plan reduces new enrollee take-up by 33%. In the reverse direction, removing this hassle via auto-enrollment expands new enrollment by 48% and steady-state enrollment by 32% – an enormous impact relative to lower-touch "nudges" like reminders and outreach (Domurat et al., 2021; Goldin et al., 2021; Ericson et al., 2019; Banerjee et al., 2021). The marginal enrollees are differentially young, healthy, and low-cost. They are also more economically disadvantaged, more likely to be immigrants, and more likely to be people transitioning out of Medicaid into exchange eligibility. Including auto-enrollees in the market not only expands take-up but meaningfully lowers the cost of the market risk pool.

We find little evidence that auto-enrollment crowds out active choice or that it incorrectly "over-enrolls" people who already have other insurance. Instead, a seemingly small hassle barrier makes the difference between whether a large group of people have coverage or are uninsured. This is consistent with growing evidence that modest financial premiums also have large impacts on take-up among the the poor (Dague, 2014; Finkelstein, Hendren and Shepard, 2019b; Tebaldi, 2020).

Our second main finding is a correction to the standard understanding of ordeals targeting introduced by Nichols and Zeckhauser (1982) and used in a large subsequent literature. "Favorable" targeting is typically equated with an ordeal that screens out low-benefit enrollees, who obtain less value from a social program. But when enrollee value and costs are highly *correlated* – a key feature of insurance – this targeting metric is incomplete. A program improves targeting only if it screens out people with low value *relative to* cost – or what we call the "value-cost ratio," which we connect to the MVPF metric of Hendren (2016). Using our reduced form evidence and a model of demand and cost, we find that the targeting implications of auto-enrollment are reversed if one uses the correct measure: it enrolls low-value types who nonetheless have similar or higher MVPF. We also find that auto-enrollment compares favorably to subsidies in both cost-effectiveness and MVPF.

This point about value-cost correlation and ordeals targeting is relevant for a wide variety of social insurance and transfer programs. Based on standard rational arguments, ordeals are well-designed to screen out low-value types – and there is evidence (including in this paper) that this often occurs (Alatas et al., 2016; Dupas et al., 2016; Finkelstein and Notowidigdo, 2019a), though not always (Deshpande and Li, 2019; Homonoff and Somerville, 2021). But ordeals are not well-designed to screen out people with *low value-cost ratios.* Unless policymakers are willing to target ordeals at higher-cost enrollees – which for health insurance, would mean larger hassles for the sick – this suggests a fundamental limitation of ordeals targeting that goes beyond previous critiques based on behavioral biases.

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Online Appendix: "Reducing Ordeals through Automatic Enrollment"

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),[52]
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.[53] 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.

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).

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

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

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.1 shows the monthly count of new enrollees in the 0-100% of poverty group (analogous to the similar Figure 2 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 2.3 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 3. 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 $\leq$12 months were immediately enrolled in their previous plan without having to take any action (though they could switch ex-post). Further, among $\leq$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 $\leq$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 $\leq$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 3 combining the number of new enrollees plus the 13+ month re-enrollees into both treatment and control groups.[54] 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.3. 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 3. 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.4 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.

[54] 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 2 shows that constant enrollment inflow (separately for actives and passives) is a reasonable approximation from 2009 on, and Appendix Figure A.5 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.5). 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.6 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).[55] 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.

[55]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 4 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.[56]

Figure A.7 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.[57] 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

[56] 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.

[57] 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.

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.8 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.9 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 5. 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 4.1. 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 (1)):

$$Y_{i,g,t} = \alpha_g + \beta_t + \gamma \cdot 1\{g = Treat, t \geq 2010\} + \varepsilon_{i,g,t} \tag{7}$$

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 (7) 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:

$$Y_{i,g,t} = \alpha_g + \beta_t + \gamma \cdot 1\{g = Treat, t \geq 2010\} + \delta_g \times t + \varepsilon_{i,g,t} \tag{8}$$

Appendix Table A.4 reports estimates of γ from these regressions, with the simple DD estimates (equation (7)) shown in columns (3)-(4) and the DD model with linear time trends (equation (8)) shown in columns (6)-(7).[58] 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 $\overline{Y}_P$

[58]Note that the DD estimates for risk score and costs reported in body text Figure 5 are based on the simple DD model in equation (7).

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:

$$\Delta\overline{Y} = \overline{Y}_{Post2010} - \overline{Y}_{Pre2010} = s_P \cdot \left(\overline{Y}_A - \overline{Y}_P\right) \tag{9}$$

Columns (1)-(2) of Table A.4 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.[59] 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 5. 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.

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

As discussed in Section 2.2, 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.1). 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.5 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 char-

[59] 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.

acteristics/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.10 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 4.3, 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.6 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.6 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:

$$PassiveRate_{a,t} = \alpha_a + \beta_t + \gamma \cdot \Delta NPlans_a * 1\{t \geq 2009\} + \epsilon_{a,t} \tag{10}$$

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.7 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 4.3. 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.[60]

The top panel of Table A.7 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).

[60]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.

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).

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).[61] 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.11 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.[62]

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-

[61] 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.

[62] Note that Figure A.11 includes all new and re-enrollees in all income groups, which is why the counts differ from what is shown in Figure 2 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.12 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.12B 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.13 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 5), 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.[63] 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.[64] 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.8. 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

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

[64] 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 5 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.[65]

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.14 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.[66]

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 fun the following regression across income bins (b) collapsed at the 2% of FPL level:

$$Enr_b = \alpha_{s(b)} + \beta_{s(b)} Inc_b + \epsilon_b \tag{11}$$

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.[67]

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

[66] 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).

[67] 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.[68]

Panel B of Appendix Figure A.14 shows results of this RD analysis for CommCare enrollment during 2009-2011, the relevant period for our study.[69] The graph shows average monthly enrollment by 5% of FPL income bins, along with linear RD best-fit lines and RD estimates based on (11). 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 (11). 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:

$$dEnr_g^k \equiv (\hat{Enr}_g^{above(k)} / \hat{Enr}_g^{below(k)}) - 1 \tag{12}$$

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.

[68] 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 argue 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.

[69] 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:

$$\frac{s_g(\$39)}{s_g(\$0)} = 1 + dEnr_g^{150\%} \tag{13}$$

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., $s_g(\$0) \equiv 1.0$.[70] We then iteratively construct inverse demand at higher prices (P_L = \$39, \$77, \$116) using the RD estimates:

$$\begin{aligned} s_g(\$39) &= \overbrace{s_g(\$0)}^{\equiv 1.0} \times \left(1 + dEnr_g^{150\%}\right) \\ s_g(\$77) &= s_g(\$39) \times \left(1 + dEnr_g^{200\%}\right) \\ s_g(\$116) &= s_g(\$77) \times \left(1 + dEnr_g^{250\%}\right) \end{aligned} \tag{14}$$

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 due to small samples. In these cases, we enforce non-upward sloping demand by using $dEnr_g^k = 0$ in (14) 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.15 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

[70] 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.8, 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.,

$$V_i = E_s\left[D_{g(i)}(s)\right] \tag{15}$$

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 (15). 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 $s_A \sim U\left[0, 1 - 0.33\omega\right]$, while passives are uniformly distributed over $s_P \sim U\left[0.67\omega, 1.0\right]$. 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 $s_A \sim U\left[0, 1 - 0.33\omega\right]$ and $s_P \sim U\left[0.67\omega, 1.0\right]$. To do so, we derive:

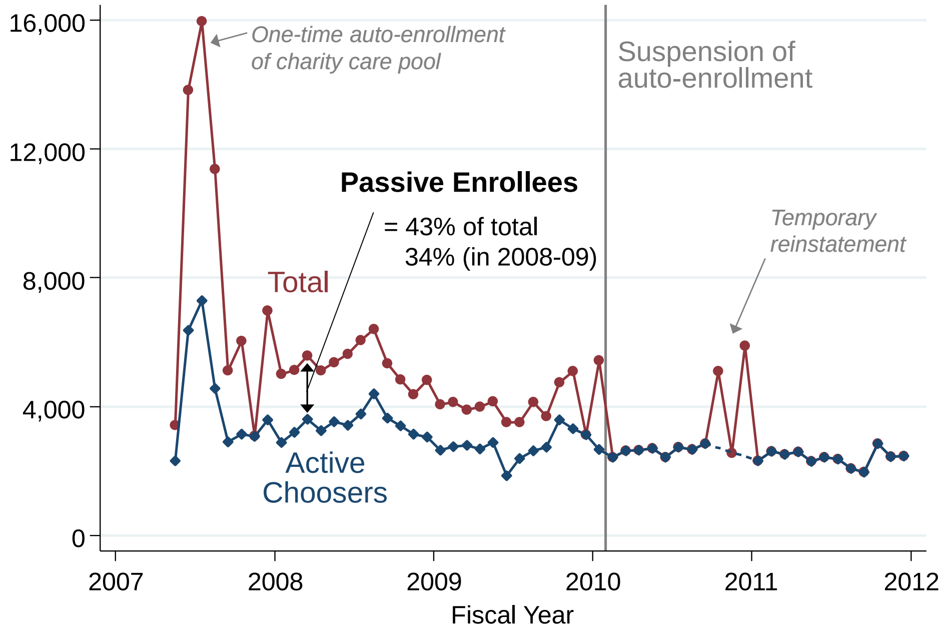
$$
\begin{aligned}
Pr\left(s_A < s_P\right) &= 1 - Pr\left(s_P < s_A\right) \\
&= 1 - E_{s_A}\left[Pr\left(s_P < s_A \,|\, s_A\right)\right] \\
&= 1 - E_{s_A}\left[\max\left\{0, \tfrac{s_A - 0.67\omega}{1 - 0.67\omega}\right\}\right] \\
&= 1 - E_{s_A}\left[\tfrac{s_A - 0.67\omega}{1 - 0.67\omega} \,|\,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

Solving (numerically) for the value of ω that makes this expression equal 0.56 yields $\omega = 0.12$. This in turn implies $s_A \sim U\left[0, 0.96\right]$ and $s_P \sim U\left[0.08, 1.0\right]$, just as noted above.[71]

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

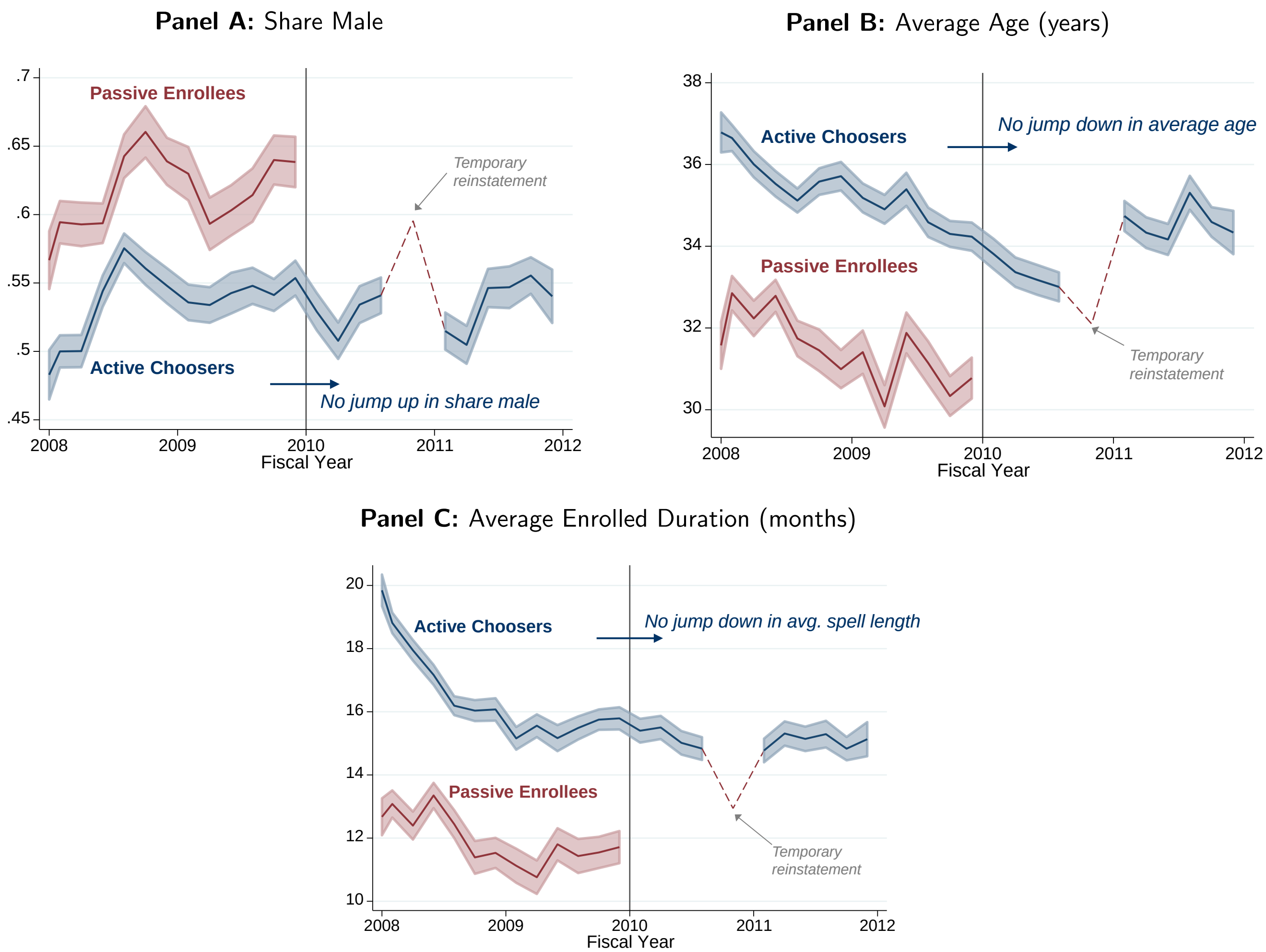
G Appendix Figures and Tables

Figure A.1: 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 2 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 2.1). 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.

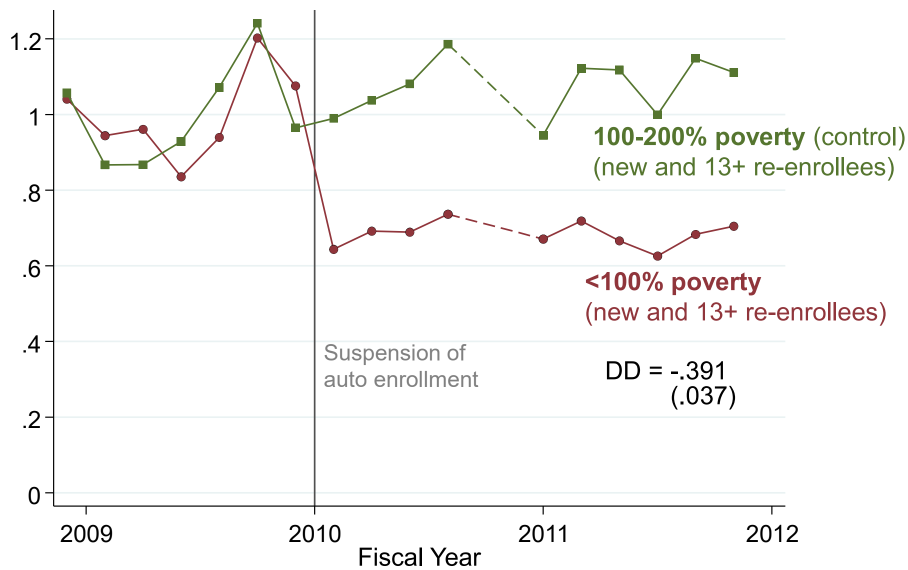
Figure A.2: Evidence against Purposely Passive: No Jumps in Active Enrollee Characteristics



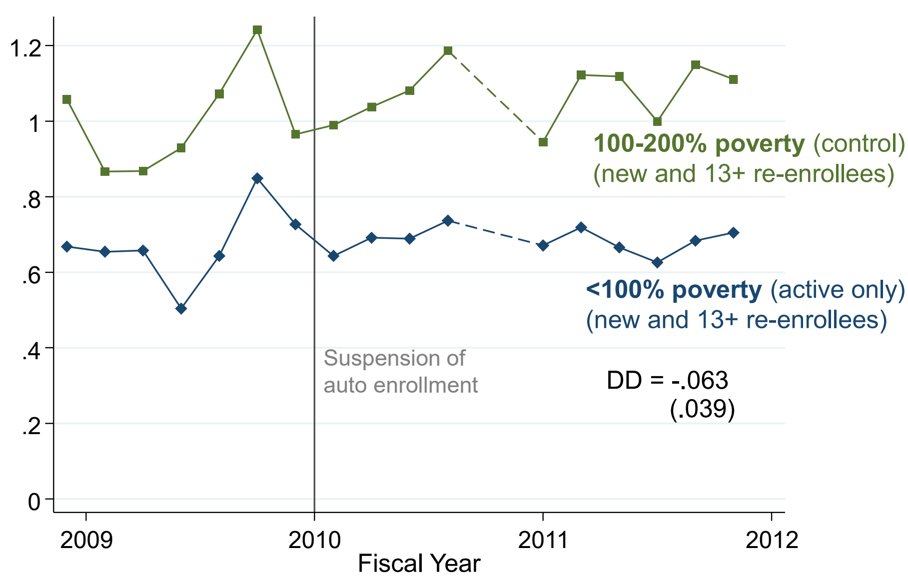
Note: The figure examines whether there are changes in the average characteristics of active new enrollees at the suspension of auto-enrollment (start of 2010), which could indicate the presence of "purposely passive" types (see Section 3.1 for a definition). If there were purposely passive types, we 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 3B), 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.3: 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)*

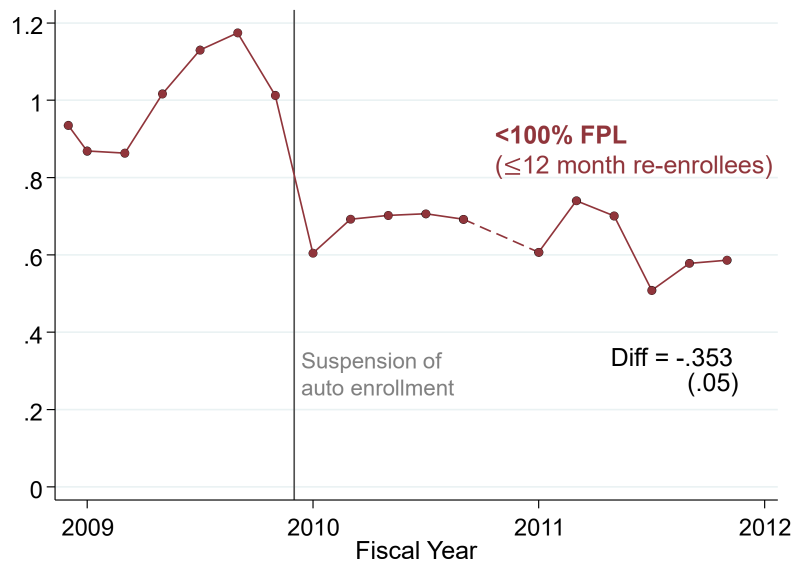


Panel B: Active New + 13+ Mon. Re-enrollees per Month *(scaled, 1.0 = pre-period mean)*



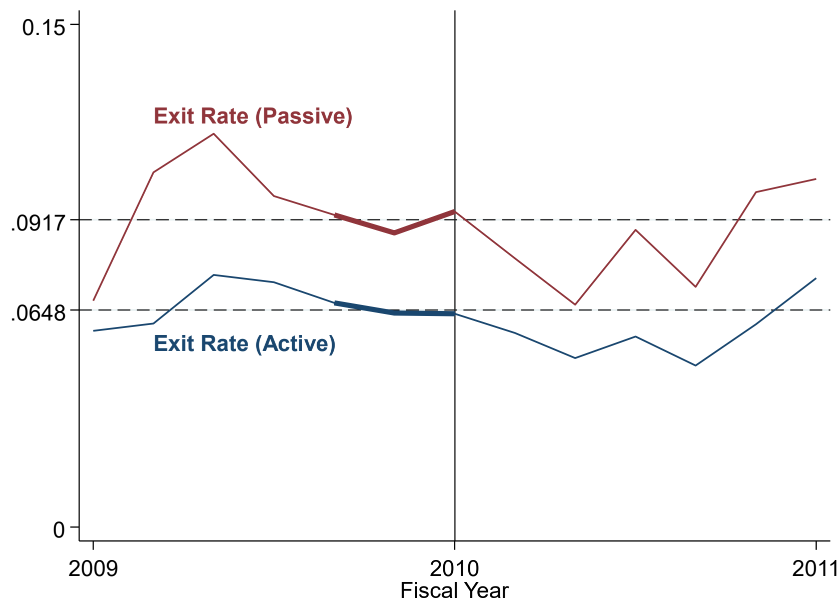
Note: The figure shows data on the scaled sum of new enrollment and 13+ month re-enrollment per month into the CommCare market and estimates of the difference-in-difference specification (1) for estimating the causal effect of the suspension of auto-enrollment at the start of 2010. Each panel compares 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 3.1 for a definition). These results are qualitatively similar to the results reported in the main paper excluding all re-enrollees.

Figure A.4: Change in Enrollment of Short-Gap Re-enrollees (with $\leq$ 12 month gap)



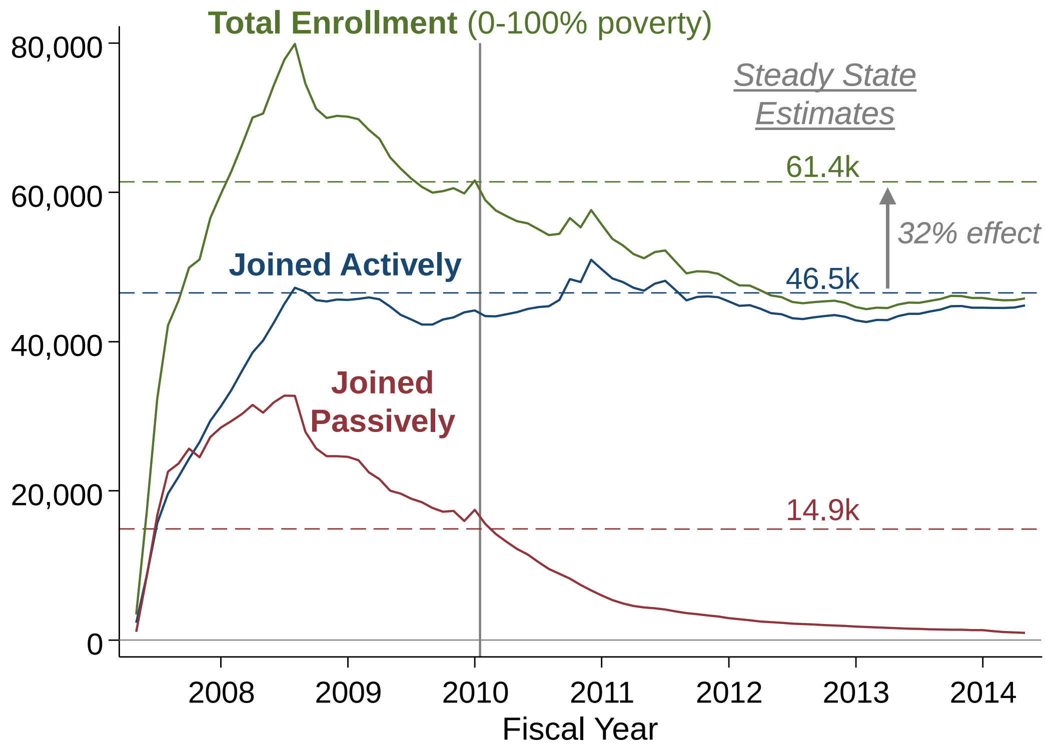
Note: The figure shows data on the scaled number of below-poverty $\leq$ 12 month re-enrollees per month, with the pre-period mean scaled to be 1.0. It shows estimates of the pre/post difference after the suspension of auto enrollment at the end of 2009. As noted in the text, we cannot implement a difference-in-difference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

Figure A.5: Exit Rate per Month for Active vs. Passive Enrollees



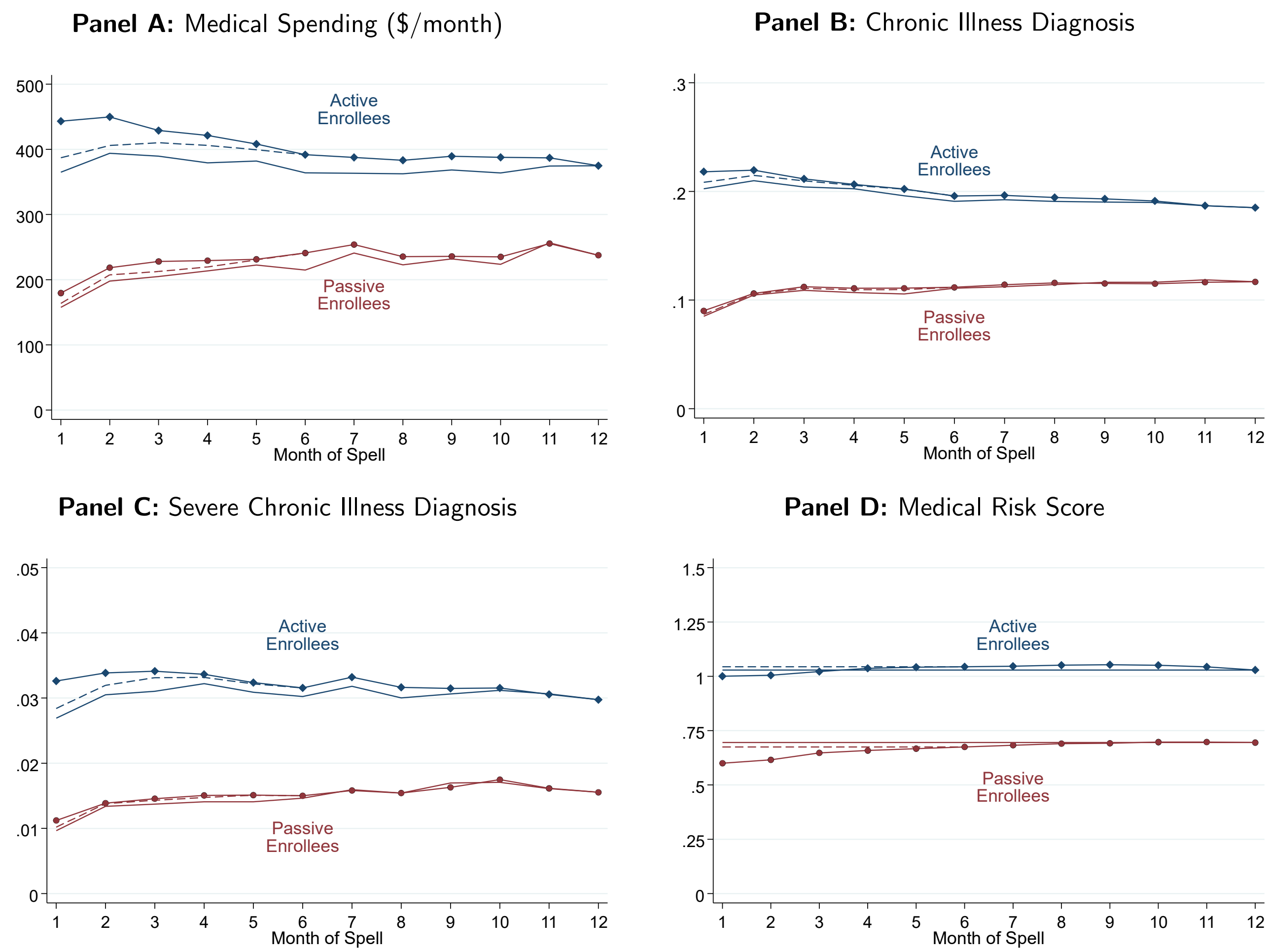
Note: The figure plots the exit rates in bi-monthly bins for active (blue) vs. passive (red) enrollees as an input to our steady state market size categories. The segments of each curve shown in bold are the samples used to estimate the average exit rates for each category, corresponding to the final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

Figure A.6: Total CommCare Enrollment (0-100% poverty), by Whether Joined Actively/Passively



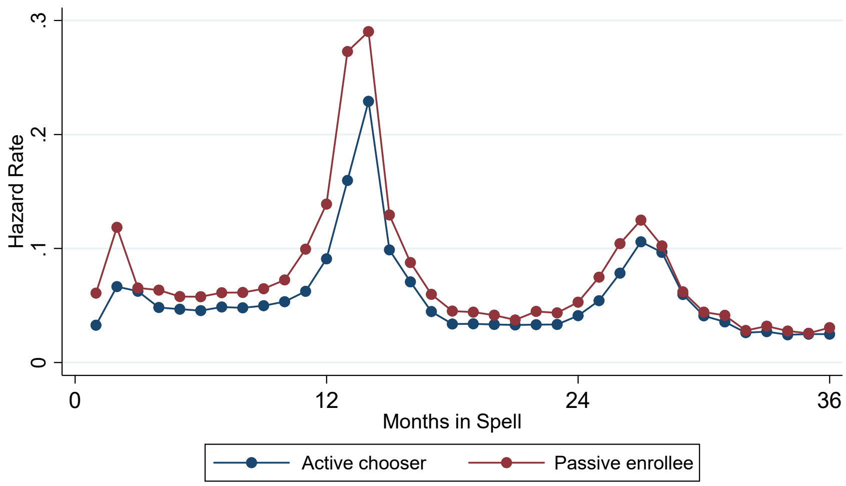
Note: The figure plots the stock of CommCare enrollment over time in the 0-100% of poverty group subject to auto-enrollment, both overall (green) and separately by whether each enrollee initially joined the market by actively choosing (blue) or passively (red). The enrollment counts are restricted to 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.7: Active vs. Passive Health Differences Observed in Claims by Month of Enrollment Spell



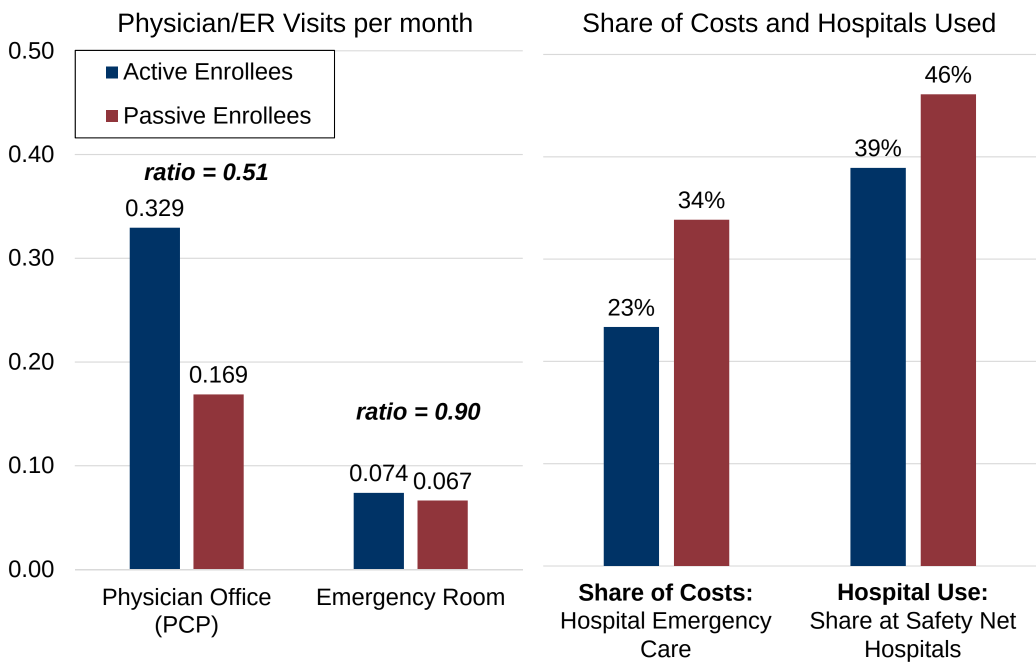
Note: The figures show the monthly rate of health measures separately for active vs. passive enrollees over the first 12 months of the enrollment spell. The solid line with markers plots the unconditional mean for all enrollees still enrolled as of that month of their enrollment (which is an unbalanced 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 $\geq$12 months; the dashed line without markers does the same for the balanced panel enrolled for ≥ 6 months. The four panels show: (A) medical spending ($ 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.8: Exit Hazard Rate from CommCare: Active vs. Passive Enrollees



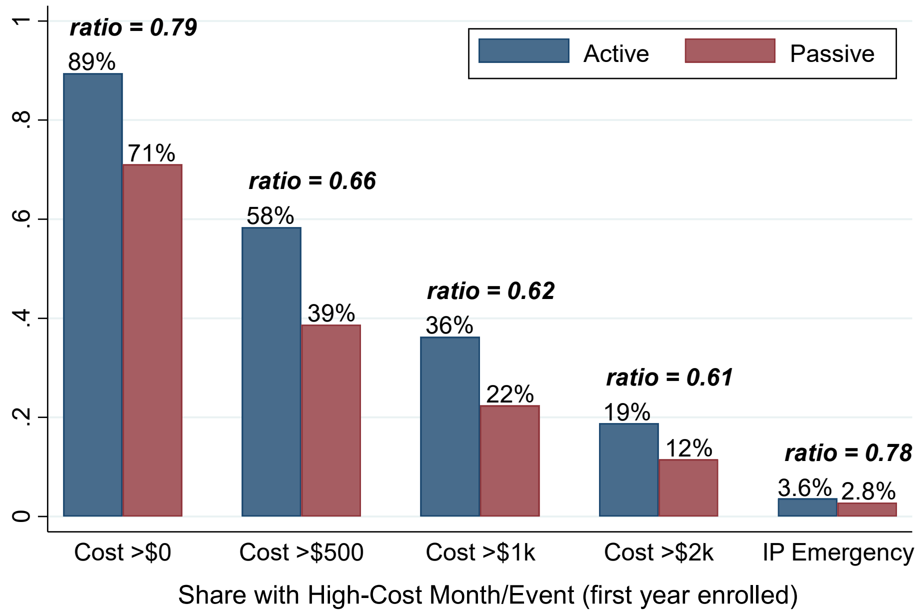
Note: To understand the reasons for passive enrollees' shorter enrollment durations, the graph shows the hazard rate of spell endings for active choosers (blue) vs. passive enrollees (red) by month since the start of their CommCare spell. The hazard rate is the share of enrollees whose spells end just 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.9: Utilization of Common Sources of Charity Care



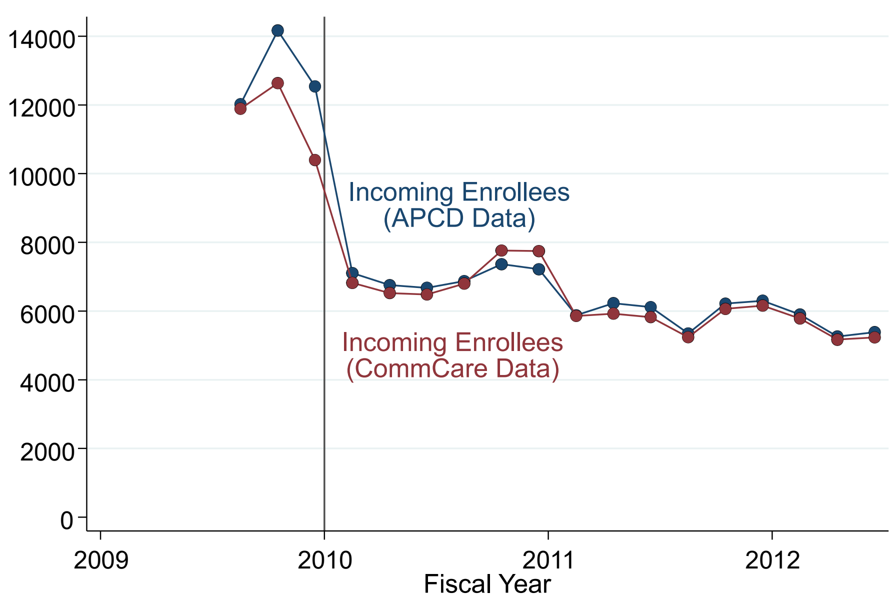
Note: The graph compares active and passive enrollees on several measures of use of common sources of charity care. The first two sets of bars show monthly rates of physician office visits (less likely to be obtained via charity care) versus emergency room visits (the classic source of charity care). 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.10: Rates of Medical Shocks for Active vs. Passive Enrollees



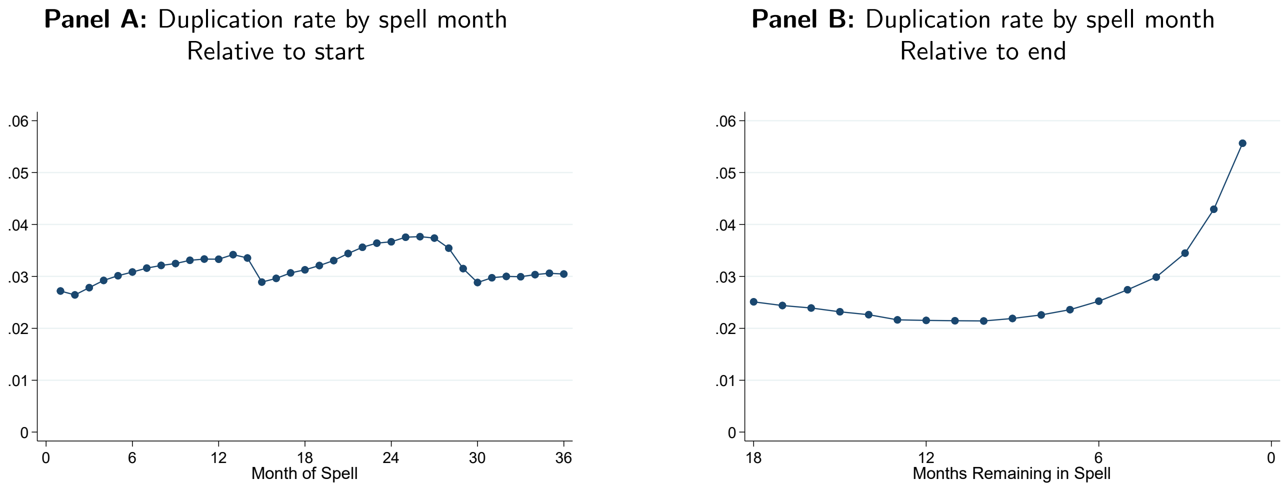
Note: The graph shows active and passive enrollees' rates of various expensive medical shocks during their first year enrolled, along with the risk ratio for passives / actives (shown above each set of vars). The first four bars are the likelihood of experiencing a single month with spending exceeding \$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.11: Validation of APCD Data on CommCare: Incoming Enrollees per Month



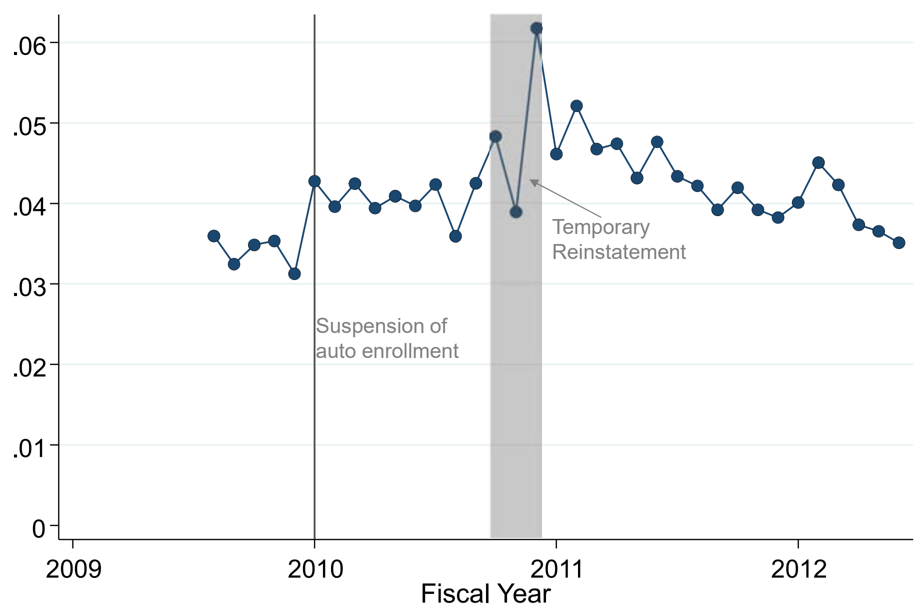
Note: The figure plots the number of incoming CommCare members in bi-monthly bins in both the administrative CommCare data (red) and in the APCD (blue). Incoming enrollees includes new enrollees and re-enrollees.

Figure A.12: 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.

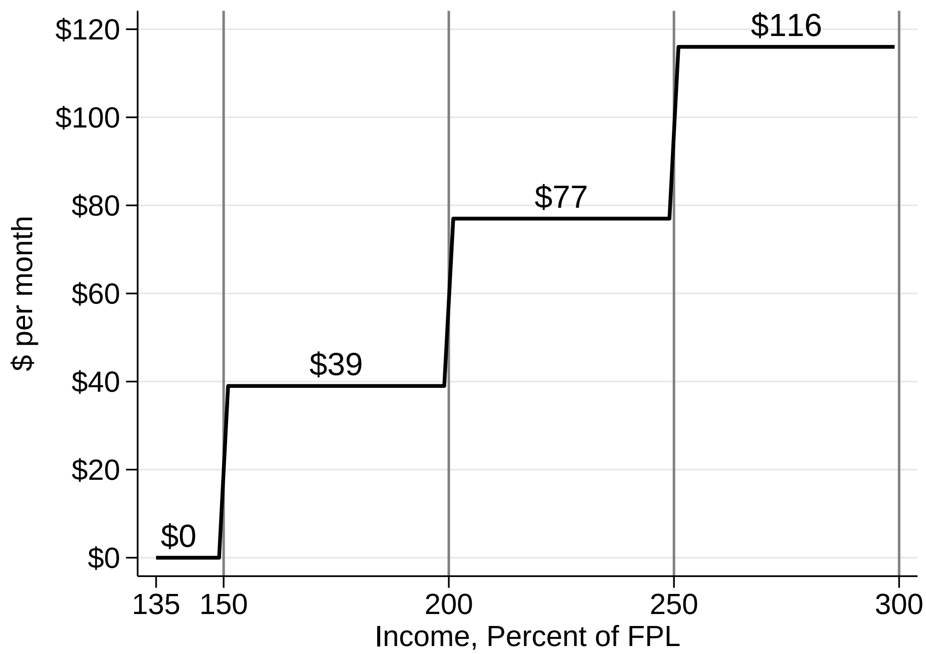
Figure A.13: CommCare Duplication Rate by Date of Entry into Market



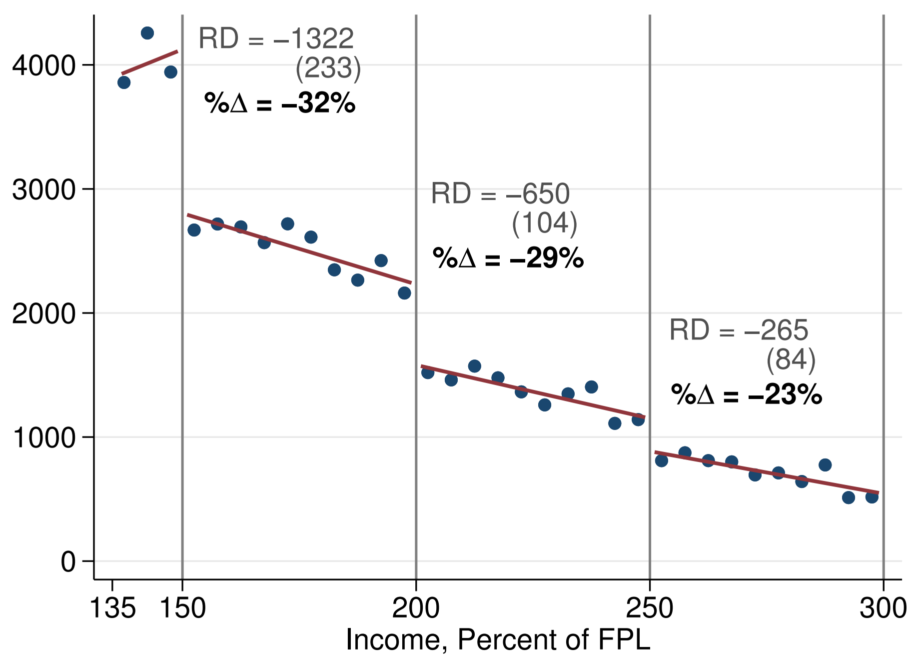
Note: The figure uses the APCD data to plot the duplication rates (over the first 12 months of the spell) for each monthly cohort of individuals entering CommCare (as new or re-enrollees, which we cannot distinguish). The first data point is February 2009 (or FY 2009m8), which is the earliest we can 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.14: Demand Model: Premium Variation and Enrollment RDs

Panel A: Enrollee Premium ($ per month) for Cheapest Plan, by Income

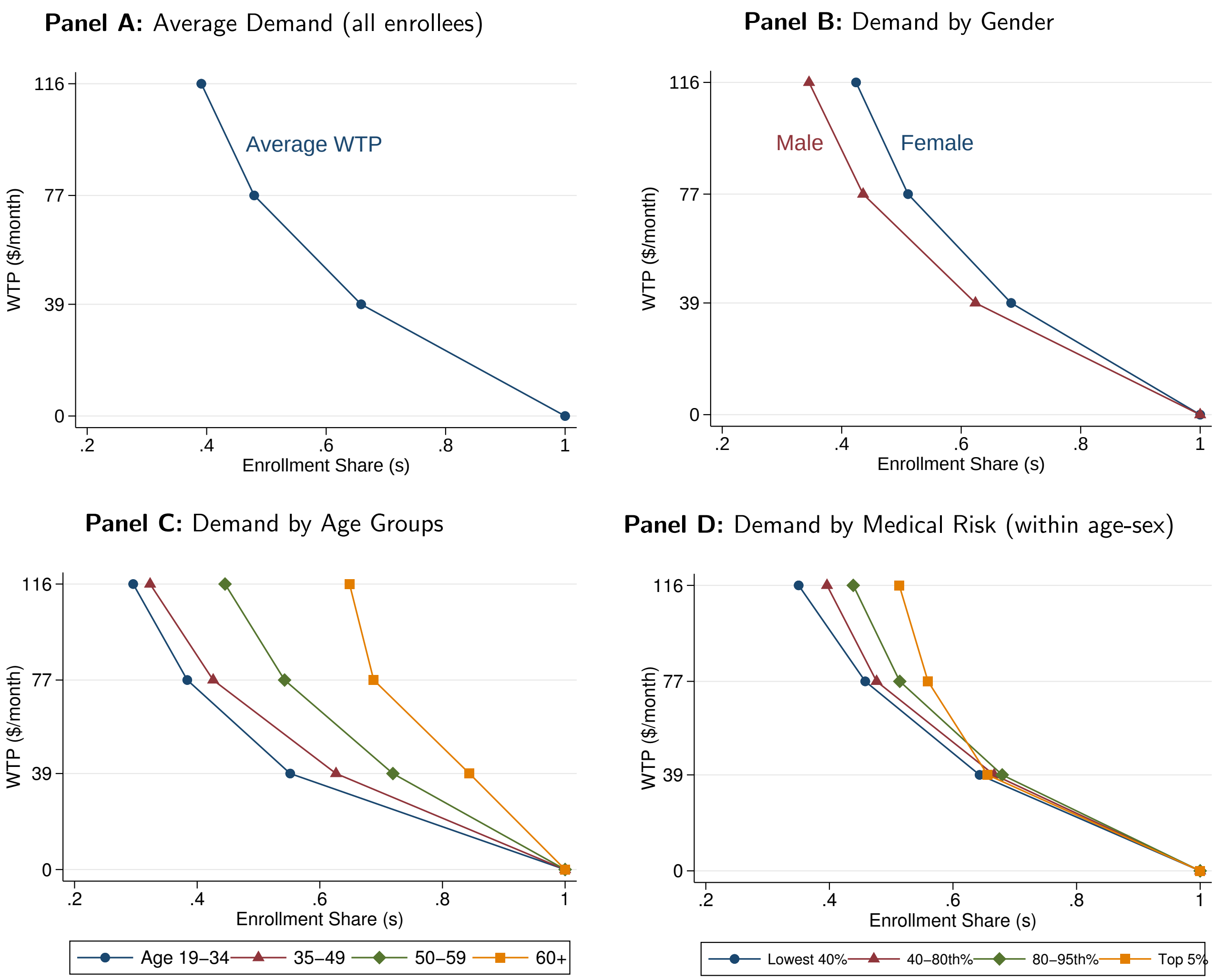


Panel B: Average Enrollment by Income and RD Estimates



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.15: 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 (0.034)	-0.324 (0.034)	-0.329 (0.045)	-0.343 (0.032)	-0.399 (0.036)	-0.339 (0.035)
Effect on Active New Enrollment	0.003 (0.037)	0.005 (0.037)	0.000 (0.046)	-0.014 (0.034)	-0.070 (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 3 are reported in column (1). These are based on the DD regression in equation (1), 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 (1) 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: Robustness: DD Estimates of Change in Enrollment Composition

Variable	Implied ΔAverage (from Table 1 ests.)		Simple DD Estimate			DD with Linear Trends		
	Est.	Std. Err.	Est.	Std. Err.	*Overlap CIs?*	Est.	Std. Err.	*Overlap CIs?*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(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 (4.1). 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 (7) 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 (8). 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.5: 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.6: Tests of Choice Overload: Passive Rate vs. Choice Set Size

Panel A: Cross-Area Relationship			Panel B: Diff-in-Diff	
# of Plans Available	Passive Rate	Sample Size (new enrollees)	Outcome: Passive Rate	
1	33.9%	6,696	ΔPlans*Post	-0.014
2	34.5%	5,009		(0.011)
3	35.2%	50,886	Num Obs.	
4	32.9%	46,103	(area-months)	874
Avg	*34.1%*	*108,694*		

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.7: 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.8: 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)*	**$129**	**$217**	**$255**	***0.85***	**$93**	**$143**	**$142**	***1.00***
Alternate Estimates: Enrollee Value								
(1) Demand: No unobserved sorting	$124	$212	$255	*0.83*	$110	$159	$142	*1.12*
(2) Demand: Perfect unobs. sorting	$170	$258	$255	*1.01*	$18	$67	$142	*0.47*
(3) Demand: Median WTP	$81	$169	$255	*0.66*	$71	$120	$142	*0.84*
75th percentile WTP	$31	$120	$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*
Alternate Estimates: Uncompensated Care								
(6) Mass. HSN Data Estimate	$129	$192	$299	*0.64*	$93	$146	$137	*1.07*
(7) Zero Uncomp. Care *(LB)*	$129	$129	$408	*0.32*	$93	$93	$228	*0.41*
(8) Full Uncomp. Care *(UB)*	$129	$241	$214	*1.13*	$93	$156	$120	*1.30*

Note: The table shows robustness checks on the targeting analysis reported in Table 2 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 2. Rows (1)-(5) 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, and (4-5) simple value value estimates based on results in Finkelstein, Hendren and Luttmer (2019a) ("FHL"). Rows (6)-(8) show alternate assumptions for the uncompensated care estimates. Row (6) uses estimates based on the Massachusetts Health Safety Net (HSN) data (see Appendix E), and rows (7)-(8) report a lower and upper bound of zero and full uncompensated care. See Sections 5.3-5.4 for further description of the model and these sensitivity analyses.

Table A.9: Robustness: Comparison of Auto-Enrollment and Subsidies

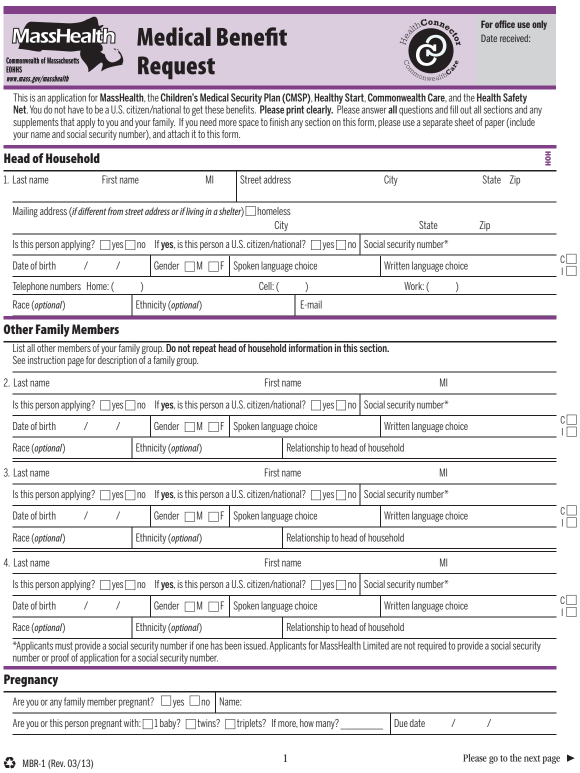
	Auto Enrollment	Subsidy Increase (↓ premiums)		
		$39 to $0	$77 to $39	$116 to $77
	0-100% FPL	*150% FPL*	*200% FPL*	*250% FPL*
	(1)	(2)	(3)	(4)
Panel A: Marginal Enrollees				
Enrollment Increase	32%	34%	36%	32%
Social Benefits of Insurance ($/month)				
Value to Enrollees (V_i)	$93	$20	$58	$97
Spillover Benefits (E_i)	$49	$42	$58	$61
Total Benefit ($V_i + E_i$)	**$143**	**$62**	**$116**	**$157**
Govt Costs of Insurance ($/month)				
Medical Costs	$228	$196	$268	$281
- Premiums Paid	$0	$0	$39	$77
= Public Subsidy (gross)	$228	$196	$229	$204
- Govt-Paid Uncompensated Care	$86	$74	$101	$106
= Net Public Cost (C_i^{Net})	**$142**	**$122**	**$128**	**$98**
Benefit-Cost Ratio (Marginals)	***1.00***	***0.51***	***0.90***	***1.60***
Panel B: Transfers to Inframarginals				
Premium Discount ($/month)	--	$39	$38	$39
x Inframarginals per marginal	3.12	2.92	2.80	3.14
= Transfer Spending per marginal	**$0**	**$114**	**$106**	**$123**
Benefit-Cost Ratio (Inframarginals)	**--**	***1.00***	***1.00***	***1.00***
Panel C: Cost-Effectiveness and MVPF				
Cost-Effectiveness				
Gross Govt Cost per Newly Insured	$228	$310	$336	$326
Net Govt Cost per Newly Insured	$142	$236	$235	$221
ΔInsured per $1 million Net Cost	**7,024**	**4,238**	**4,261**	**4,530**
Overall MVPF of Policy	***1.00***	***0.74***	***0.95***	***1.27***

Note: The table shows additional statistics for the comparison of auto-enrollment and subsidies reported in Table 3 in the body text. The setup of the table is identical to Table 3; see that table's note for a description. The current table reports additional statistics, including: (a) enrollee value (V_i) and spillover benefits (E_i) separately from total benefit ($V_i + E_i$) (in Panel A), (b) additional statistics underlying the calculation of net public costs (also in Panel A), and (c) "gross" government cost per newly insured (which excludes public uncompensated care savings) (in Panel C).

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

Health Connector Commonwealth Care

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

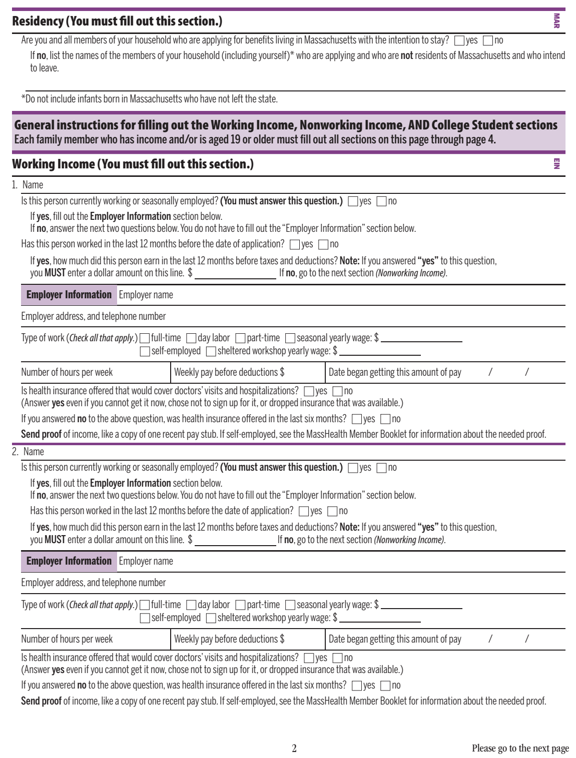
*Applicants must provide a social security number if one has been issued. Applicants for MassHealth Limited are not required to provide a social security number or proof of application for a social security number.

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 UN

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 UN

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.