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# ESTIMATING THE HETEROGENEOUS WELFARE EFFECTS OF CHOICE ARCHITECTURE: AN APPLICATION TO THE MEDICARE PRESCRIPTION DRUG INSURANCE MARKET

Jonathan D. Ketcham
Nicolai V. Kuminoff
Christopher A. Powers

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**ABSTRACT**

We develop a structural model for bounding welfare effects of policies that alter the design of differentiated product markets when some consumers may be misinformed about product characteristics and inertia in consumer behavior reflects a mixture of latent preferences, information costs, switching costs and psychological biases. We use the model to analyze three proposals to redesign markets for Medicare prescription drug insurance: (1) reducing the number of plans, (2) providing personalized information, and (3) defaulting consumers to cheap plans. First we combine administrative and survey data to determine which consumers make informed enrollment decisions. Then we analyze the welfare effects of each proposal, using revealed preferences of informed consumers to proxy for concealed preferences of misinformed consumers. Results suggest that each policy produces large gains and losses for some consumers, but the menu reduction would unambiguously harm most consumers whereas personalized information would unambiguously benefit most consumers.

Jonathan D. Ketcham
Earl G. and Gladys C. Davis Distinguished Research Professor in Business
Department of Marketing, Box 4106
W.P. Carey School of Business
Arizona State University
300 E. Lemon Street
Tempe, AZ 85287-4106
ketcham@asu.edu

Nicolai V. Kuminoff
Department of Economics
Arizona State University
P.O. Box 879801
Tempe, AZ 85287
and NBER
kuminoff@asu.edu

Christopher A. Powers
U.S. Department of Health and Human Services
Centers for Medicare and Medicaid Services
7500 Security Boulevard
Mailstop B2-29-04
Baltimore, MD 21244
Christopher.Powers@cms.hhs.gov

One of the frontiers in empirical microeconomics is to assess the equity and efficiency of polices that alter a market's design and "nudge" consumers toward making certain decisions. Thaler and Sunstein (2008) denoted this approach to policy as "choice architecture". Examples of choice architecture include restricting the number of differentiated products in a market, providing consumers with personalized information about their options, and making default choices for consumers but letting them opt out. Numerous government organizations including the United States and the World Bank have begun using choice architecture to nudge the beneficiaries of public programs.

A stated goal of choice architecture is to benefit consumers who do not make fully informed decisions. Such paternalistic policies may also harm some consumers by eliminating their preferred products, by making it harder to buy those products, and by causing prices to increase. Despite the potential for important and heterogeneous effects, little work has predicted the distribution of gains and losses of prospective choice architecture policies. To do so within a revealed-preference framework requires addressing two challenges. First, analysts must identify which decisions are misinformed and hence potentially misleading about consumer preferences. Second, analysts must infer the preferences of both informed and misinformed consumers. In this article, we develop an empirical framework to address both challenges and use it to evaluate policies that have been proposed to nudge consumer decision making in health insurance markets.

We operationalize Bernheim and Rangel's (2009) conceptual logic for policy analysis in the presence of latent constraints that undermine revealed preference logic for some consumers. We envision consumers facing differentiated costs of acquiring information about their choice sets so that some consumers may choose to purchase products without becoming fully informed. Analysts often observe the characteristics of consumers, their choices and their choice sets but typically do not observe how consumers form beliefs or make decisions. In our context, we observe signals about whether each decision was made by an informed consumer. First, we have access to the results of survey-based tests of consumers' knowledge about the products they are choosing. Second, we observe each person's full menu of choices, their actual choice outcomes, and the counterfactual outcomes under each option available to them. With this information, we develop signals of whether the choices reveal or conceal preferences, such as whether their survey responses indicate comprehension of key market institutions and whether their choices are consistent with axioms of consumer theory. We examine how these signals correspond to proxies for being informed, such as the presence of Alzheimer's disease, educational levels, and self-reported efforts to gather information. We use these signals to identify the subset of choices that we suspect may fail to reveal preferences. We show that welfare analysis is possible in this setting if the mapping between preferences and consumer demographics is stable across the groups of consumers making "suspect" and "non-suspect" choices.[1] Under this stability assumption, we estimate a repeated choice multinomial logit model that incorporates heterogeneity on observed consumer attributes and we derive welfare measures that characterize how heterogeneous consumers are affected by choice architecture policies. Our measures are consistent with the idea that consumer inertia may arise from a latent mixture of preferences, information costs, switching costs, and psychological biases. In the special case where all consumers are fully informed, freely mobile and immune to biases, our welfare measures reduce to those derived by Small and Rosen (1981).

We use our model to study financial decisions among elderly Medicare beneficiaries in the US. The elderly population is particularly important because they control a large share of wealth and frequently experience declines in cognitive function (Querfurth and LaFerla, 2010, Fang, Silverman and Keane 2008, Agarwal et al. 2009, Keane and Thorp 2016). We analyze their choices in markets for Medicare standalone prescription drug insurance plans. In 2015, these government-designed, taxpayer-subsidized markets annually enrolled 25 million older adults with federal outlays of $75 billion (US Department of Health and Human Services 2017). Beneficiaries' enrollment decisions are multifaceted and financially important. Between 2006 and 2010, the average new enrollee chose among 50 plans that differed in cost, risk protection and quality. Returning enrollees were automatically reassigned to their previously chosen plans unless they opted to switch plans during the annual

[1] The "suspect / non-suspect" terminology is borrowed from Bernheim and Rangel (2009). This language emphasizes that it is virtually impossible for analysts to determine beyond a doubt whether a consumer is fully informed at the time of her decision. Our framework requires only that non-suspect choices be fully informed. Suspect choices may or may not be fully informed.

open enrollment window. The median enrollee spent approximately 6% of her annual household income on premiums and out-of-pocket costs.

Due to concerns about expenditure levels, market complexity and consumer inertia, researchers and federal agencies have proposed several reforms to prescription drug insurance markets (McFadden 2006, Thaler and Sunstein 2008, Federal Register 2014). These include reducing the number of plans, providing consumers with personalized information about their options, and auto-assigning people to default plans that are expected to minimize cost. We assess the welfare effects of these proposals by combining administrative records and survey data on a national panel of enrollees from 2006-2010. Specifically we link the longitudinal Medicare Current Beneficiary Survey (MCBS) to administrative records of the respondents' annual enrollment decisions, drug claims, and medical conditions. This novel linkage allows us to combine information on enrollees' efforts to learn about the market, their knowledge of how products differ, whether they self-enrolled in plans or had help from advisors, their demographics, their choices and their choice outcomes, and their health, including their prescription drug utilization.

We capitalize on the depth and breadth of these linked data to develop several signals of consumers' knowledge given that any one signal is potentially controversial. Our primary approach is to assume a decision is informed if two conditions are satisfied: (i) the decision maker's performance on the MCBS knowledge test demonstrates that she understands that her out-of-pocket prescription drug costs vary across plans and (ii) the plan choice can be rationalized by a preference ordering that is complete, transitive, monotonic and weakly risk averse. These two requirements are jointly satisfied for 58% of enrollment choices. Enrollees in this non-suspect group tend to be better educated and have exerted more effect to learn about the market. They also tend to be younger, to have fewer drug claims, and are less likely to be diagnosed with Alzheimer's disease or other forms of dementia. Our secondary approaches to partitioning decisions include using the knowledge test alone, using other choice outcomes based on either ex ante or ex post drug consumption, and using other combinations of the two. This variety of measures also allows us to provide new insights about what specific knowledge consumers may be lacking. While the measures based on choice outcomes incorporate consumers' knowledge about plans as well as their individual-specific drug needs, the measure based on the MCBS question isolates consumers' knowledge about plan design specifically.

After dividing choices into suspect and non-suspect groups, we estimate and validate multinomial logit models for each group, incorporating heterogeneity within each group in terms of income, education, race, age, sex, prescription drug use, and information-seeking efforts. We model annual plan choices as a static repeated-choice process with a cost of switching plans.[2] The results show that enrollees in the non-suspect group are sensitive to price and risk averse at levels consistent with evidence from other insurance markets (Cohen and Einav 2007, Handel 2013, Handel and Kolstad 2015). In contrast, enrollees in the suspect group make choices that seemingly imply they are risk loving, less price sensitive, and highly averse to switching plans.

We use our estimates to simulate three prospective choice architecture policies. The first policy is the government's proposal to limit each insurer to sell no more than two plans per market (Federal Register 2014). Second, we calibrate our model to replicate a field experiment by Kling et al. (2012) in which enrollees were told which plan would be cheapest for them and how much money they could expect to save by switching. In the third experiment, we simulate the government's proposal to reassign people to their cost-minimizing plans (Health and Human Services 2014). Our framework formalizes ways in which each policy may create winners and losers.[3] We simulate each policy under a range of assumptions about consumer foresight, about the causes of inertia, and about how the policies will affect consumers' decisions. Specifically, we report the share of consumers who benefit from each policy and measures of consumer surplus as bounds on ranges that we obtain by repeating our analyses under the extreme assumptions about the efficacy of choice architecture. In our "most effective" scenario we assume that each policy causes consumers in the

[2] A static model is appropriate here because it is difficult for consumers to forecast their own future prescription drug needs, let alone the drug needs and enrollment decisions of other consumers together with the implications for plan prices and offerings. Our static approach is similar to other health insurance applications such as Handel (2013) and Handel and Kolstad (2015).

[3] For example, the menu restrictions may benefit misinformed consumers by reducing their ability to choose low utility plans. The information treatment and default assignment policies could create losers due to asymmetric information because the government would only use prior drug claims, and by creating incentives for consumers to choose plans that are cheaper but potentially lower utility due to lower quality or risk protection.

suspect group to behave like their analogs in the non-suspect group. This scenario also assumes that inertia is caused entirely by misinformation. At the opposite extreme, our "least effective" scenario assumes the policies would not change consumer behavior and that inertia in the non-suspect group reflects the hassle cost of switching plans and/or their utility from latent features of their preferred plans.

Our results show that each policy would yield different outcomes. Reducing the number of plans makes at least two thirds of consumers worse off because people are heterogeneous and no plans are universally poor matches for consumers. This policy also embeds a strong incentive for regulatory capture as insurers can increase their rents by influencing which plans are retained. In contrast, we find that at least three quarters of consumers benefit from personalized information or are unaffected by it, with average welfare gains of 2 to 11 percent of consumers' out-of-pocket spending. Similarly, defaults benefit over 80 percent of consumers if they can costlessly opt out. However, average opt out costs of \$65 to \$198 entirely eliminate these gains.

To determine whether these results are sensitive to whether and how we divide choices into suspect and non-suspect groups, we repeat our analysis first without distinguishing between choice types and second using alternative combinations of signals based on knowledge tests, preference axiom tests, and the level of potential savings. We demonstrate that distinguishing between informed and uninformed decisions improves the model's performance and changes its implications for the welfare effects of prospective policies. Specifically, when we estimate a standard conditional logit model that does not incorporate signals about consumers' knowledge, similar to Lucarelli, Prince and Simon (2012), we understate welfare gains for the consumers that we assign to the suspect group and overstate gains for those we assign to the non-suspect group. In contrast, while our choice about which signals to use to identify suspect choices affects the fraction of choices we assign to the suspect group (from 17% to 48%), it has relatively little effect on our welfare estimates for the average consumer. The robustness of our policy conclusions reflects the distribution of choices in our data. Intuitively, as we assign more choices to the suspect group the benefits of choice architecture policies to the marginally assigned consumers diminish.

Our simulation results are conditional on three maintained assumptions. First, we assume that the demand for prescription drugs is perfectly inelastic. Holding drug consumption constant across all options is a common way of simplifying the empirical specification of consumers' indirect utility function, but it may alter the levels and distributions of each policy's effects. Second, we omit supply-side responses and hold plan attributes constant, including premiums. Full general equilibrium outcomes under the prospective policies may diverge from those we simulate, although regulators have some ability to constrain supply-side changes. Third, apart from our bounding estimates for the inertia parameters, our analysis assumes that the conditional differences in behavior between suspect and non-suspect types are due to differences in information rather than preferences. We discuss the implications of each assumption in detail after presenting our simulation results.

Our work adds to the empirical literature that aims to understand heterogeneity in consumers' decision processes and its implications in markets where frictions may undermine revealed preference assumptions for some consumers (Harris and Keane 1999, Miravete 2003, Handel 2013, Keane and Wasi 2013, Ambuehl, Bernheim and Lusardi 2014, Miravete and Palacios-Huerta 2014, Allcott and Taubinsky 2015, Bernheim, Fradkin and Popov 2015, Chetty et al. 2015, Handel and Kolstad 2015, Wiswall and Zafar 2015, DeCicca, Kenkel, Liu and Wang 2016, Houde 2016, Keane and Thorp 2016, Kenkel, Peng, Pesko and Wang 2017, Arcidiacono et al. 2017). We contribute to this literature in several ways. From a methodological perspective our study is the first to operationalize Bernheim and Rangel's (2009) logic within the workhorse multinomial logit model. This makes our econometric framework easy to apply to other markets and straightforward to extend to dynamic decision making. Further, we derive bounds on welfare that are robust to a wide range of mechanisms that have been suggested as potential explanations for consumer inertia.

From an empirical perspective, our study is the first to analyze distributional welfare effects of federal choice architecture policies targeting a financially important decision. By contrast, prior applications have focused on employees at a small number of firms (Handel and Kolstad 2015, Bernheim, Fradkin and Popov 2015) or participants in laboratory and field experiments (Arcidiacono et al. 2014, Allcott and Taubinsky 2015, Wiswall and Zafar

2015) or they have abstracted from welfare measurement (Chetty et al. 2015). We also provide two new insights regarding which choices violate revealed preference assumptions. First, we find that knowledge tests and preference axiom tests provide complementary information about such violations. Some decision makers understand how the market works but still choose plans that violate the preference axioms; others misunderstand market institutions but choose plans that do not violate the axioms. Either way their choices can fail to reveal preferences. This is relevant because prior studies have sought to identify violations of revealed preference assumptions using knowledge tests alone (Handel and Kolstad 2015) or preference axiom tests alone (Bernheim, Fradkin and Popov 2015, Ketcham, Kuminoff and Powers 2016) but never both together. Second, we find that advisors who make decisions for consumers tend to behave similarly to consumers who make their own decisions. This finding addresses a limitation with survey-based or experimental tests of consumer knowledge—they may be uninformative about the decision process when decisions are influenced by advisors (Giustinelli 2016). We would expect spouses, children and other advisors to play a significant role in older adults' retirement planning and housing decisions, in addition to health insurance. In fact, 38% of enrollees in our data had help choosing a plan or someone chose a plan for them, in which case we use a test of the advisors' knowledge. In our context, controlling for advisors' input has little effect on our conclusions.

## I. Medicare Prescription Drug Insurance Markets

US citizens typically become eligible for Medicare benefits when they turn 65. In 2006, Medicare Part D extended these benefits to include prescription drug insurance. A novel and controversial feature of Part D is that it created quasi-private markets for delivering insurance.[4] Part D created 34 state or multistate markets within which the average enrollee chose among 50 standalone prescription drug insurance plans (PDPs) sold by 20 private insurers.[5] The default for new beneficiaries is to be uninsured.[6] After an enrollee chooses

[4] Prior to the Patient Protection and Affordable Care Act, Part D was the largest expansion of public insurance programs since the start of Medicare.
[5] Subject to CMS approval, insurers can sell multiple PDPs in each market and make annual changes to existing plans.
[6] Enrollees who qualify for low-income subsidies are autoenrolled to certain plans, but we exclude them from our analysis.

a plan, she is automatically reassigned to the same plan the following year unless she switches to a different one during open enrollment. Enrollees pay monthly premiums as well as out of pocket (OOP) costs for the drugs they purchase and taxpayers subsidize the total costs of non-poor enrollees by an average of 75.5%.

PDPs differ in terms of premiums, OOP costs of specific drugs, and quality measures such as customer service, access to pharmacy networks, the ability to obtain drugs by mail order, and the prevalence and stringency of prior authorization requirements.[7] The novelty of the market together with the complexity of the product led many analysts to speculate that consumers would struggle to navigate the market. Liebman and Zeckhauser (2008) summarize this view when they write, "Health insurance is too complicated a product for most consumers to purchase intelligently and it is unlikely that most individuals will make sensible decisions when confronted with these choices." Some analysts flagged Part D as a candidate for libertarian paternalism (McFadden 2006, Thaler and Sunstein 2008). Moreover, the government has expressed a desire to simplify health insurance markets and nudge enrollees toward cheaper plans. In 2014, CMS proposed limiting insurers to selling no more than two plans per region, which would reduce the average consumer's choice set by about 20% (Federal Register 2014). The US Department of Health and Human Services also announced that it is considering redesigning federal health insurance exchanges to automatically reassign people to low-cost plans unless they opt out (Health and Human Services 2014). The welfare effects of these types of policies depends on consumers' preferences for PDP attributes, the cost of switching plans, and how the policies affect consumers' decision processes.

Several prior studies have investigated the role of information and consumer behavior in Medicare Part D. Over the first five years of the program, the average enrollee could have reduced annual expenditures (premium + out of pocket) by 25% (or $341) by switching to their cheapest available plan (Ketcham, Lucarelli and Powers 2015) and more than 75% of consumers chose plans that did not minimize their costs on an ex ante basis (Heiss et al. 2013). Yet, the implications for consumer welfare remain ambiguous. When enrollees

[7] Many insurers require consumers to have prior authorization from a doctor in order to obtain certain drugs, but the stringency of these requirements differs from insurer to insurer.

are surveyed about their experiences in Part D, most report being satisfied with the plans they chose (Heiss, McFadden and Winter 2010, Kling et al. 2012). Furthermore, Ketcham, Kuminoff and Powers (2016) demonstrate that most of the people who could have saved money by switching chose plans that were either superior in some measure of quality or provided greater protection from negative health shocks. These consumers could be making informed decisions to pay for quality and risk protection. On the other hand, when Kling et al. (2012) asked 406 Wisconsin enrollees how much they thought they could save by switching plans, most respondents underestimated the true figure. Kling et al. also found that sending enrollees a letter with personalized information about their potential savings increased the rate at which enrollees switched plans by 11.5 percentage points. Overall, the existing evidence suggests that some consumers are misinformed, but others may be choosing to pay more for plans with higher quality and/or greater risk protection.

A few prior studies have developed multinomial logit models of Part D enrollment decisions, but none have explored the role of heterogeneity in consumers' beliefs about the market or its implications for the distributional effects of choice architecture policies. Lucarelli, Prince and Simon (2012) use the fully revealed preference benchmark approach to assess welfare effects of reducing the number of insurance plans. Under their approach, they assume that all consumers are fully informed, implying that nobody can be made better off by restrictions on choice. Ho, Hogan and Scott-Morton (2017), Polyakova (2016) and Heiss et al. (2016) document the empirical prevalence of inertia among consumers and explore its implications for adverse selection and insurance company profits. Finally, Abaluck and Gruber (2011) and its sequels conclude that the "representative" consumer places too much weight on premiums relative to out-of-pocket costs and is mistakenly indifferent to expenditure risk. Then they assess the welfare gains from a hypothetical policy that reassigns consumers to their utility maximizing plans. Nobody can be made worse off from the hypothetical reassignment because the social planner is assumed to be benevolent and omniscient. Ketcham, Kuminoff and Powers (2016) show that Abaluck and Gruber's calculation requires them to know the precise parametric form of every consumer's utility function, up to and including their individual-specific levels of iid Type I extreme value distributed preference parameters.

We diverge from all of the prior Medicare Part D studies by developing a model to investigate heterogeneity in consumers' beliefs about the market. Importantly, we leverage novel aspects of our data and utilize revealed preference logic in a way that recognizes for the first time that policies restricting choice may create both winners and losers. Our parametric approximation to utility is similar to the studies cited above, but our econometric approach is novel in three respects. First and foremost we allow decision makers to have heterogeneous beliefs about observable plan attributes, conditional on their preferences for those attributes. Second, we recognize that the decision maker may be someone other than the consumer—a feature that is especially relevant when studying an aging population prone to cognitive decline. Third, we allow consumers' preferences for plan attributes to vary with a rich set of demographics. While demographic data are commonly used in the broader discrete choice literature, our paper is the first study of Medicare Part D to obtain access to data on a rich set of demographics including enrollees' incomes, educations, marital status, family structure, and internet use.

## II. A Parametric Model of Decision Making with Heterogeneity in Beliefs

We assume that consumer $i$'s utility from drug plan $j$ in year $t$ depends on the mean and variance of her potential expenditures in that plan under all possible health states. Expenditures equal the plan premium, $p_{jt}$, plus out of pocket costs, $oop_{jt}(x_{it})$, of an exogenously given vector of drug quantities, $x_{it}$. Utility also depends on a vector of measures of plan quality, $q_{ijt}$, that reflect the time and effort required for an individual to obtain her eligible benefits under the plan. To keep notation simple we treat the beneficiary and the person who makes her enrollment decision as being indivisible, using the $i$ subscript for both. The distinction between the beneficiary and the decision maker is neutral to the structure of our model, though it potentially influences our estimates by affecting which decisions we treat as informed—an issue we investigate in sections IV and VII.

## A. *Initial Enrollment Decision*

When a beneficiary first enters the market in year 0, she must actively choose a plan to obtain insurance. She will choose the plan that maximizes her utility, conditional on her beliefs about plan attributes.

(1) $U_{ij0} = \alpha_{it}\acute{c}_{ij0} + \beta_{it}\acute{\sigma}^2_{ij0} + \gamma_{it}\acute{q}_{ij0} + \epsilon_{ij0}.$

$\acute{c}_{ij0}$ denotes the amount that person $i$ expects to spend under plan *j* in terms of the premium plus out of pocket costs for prescription drugs, $\acute{\sigma}^2_{ij0}$ is the variance of out of pocket costs, $\acute{q}_{ij0}$ is a vector of quality attributes, and $\epsilon_{ij0}$ is a person-plan specific preference shock. The accents indicate that the variables reflect decision maker *i*'s beliefs about plan attributes. Heterogeneity in beliefs is discussed below. Beneficiaries may also have heterogeneous marginal rates of substitution between expected cost, variance, and quality. We model this heterogeneity as a function of the beneficiary's demographics, some of which may evolve over time: $\alpha_{it} = \alpha_0 + \alpha_1 d_{it}$, and similarly for $\beta_{it}$ and $\gamma_{it}$. Finally, people may lose utility from the time and effort required to learn about a plan and enroll in it. We assume that this cost is constant across plans so that it cancels out of between-plan comparisons and can therefore be suppressed in (1).

## B. *Subsequent Enrollment Decisions*

After an enrollee chooses a plan in year 0 she is automatically reassigned to that plan in year 1 unless she actively switches to a different plan during the annual open enrollment window.[8] As before, making an active decision may be costly. In contrast, no effort is required to reenroll in the default plan:

(2) $U_{ij1} = \alpha_{it}\acute{c}_{ij1} + \beta_{it}\acute{\sigma}^2_{ij1} + \gamma_{it}\acute{q}_{ij1} + \eta_{it}\Delta\acute{B}_{ij1} + \delta_{it}\Delta\acute{P}_{ij1} + \epsilon_{ij1}.$

Two terms capture the utility loss from actively switching plans: $\Delta\acute{P}_{ijt}$ is an indicator for

[8] Plans are occasionally discontinued, which can force people to make an active choice. In such case, we can revert to equation (1) to model the new enrollment decision.

whether plan $j$ is a non-default plan sold by the same insurer as the default plan, and $\Delta\acute{B}_{ijt}$ is an indicator for whether plan $j$ is a non-default plan sold by a different insurer. The disutility of switching plans is captured by the parameters $\eta_{it} = \eta_0 + \eta_1 d_{it}$ and $\delta_{it} = \delta_0 + \delta_1 d_{it}$, which summarize how inertia varies with demographics. We consider how to interpret inertia when we discuss welfare measurement in Section III. After a consumer chooses a plan in year *1*, the decision process is the same in years *2*,…,*T*.

### *C. Heterogeneity in Information*

We say that a decision maker's enrollment decision is "informed" if her beliefs about plan attributes coincide with the empirical measures that we observe as analysts. The assumption that decision makers are informed is ubiquitous (and often implicit) in revealed preference models because it is typically needed to infer consumers' preferences from their observed choices. Revealed preference logic typically fails if decision makers have beliefs about the objects of choice that diverge from information used by analysts.

Decision makers' full beliefs are unknown but we see signals about them in the data. Some decision makers send signals that cause us to suspect that they are not informed. Borrowing from Bernheim and Rangel (2009), we label their enrollment decisions as "suspect" because we suspect that they may fail to reveal the beneficiary's preferences.[9] Other decision makers send signals that lead us to believe they are informed; we label their decisions as "non-suspect". As in Bernheim and Rangel's conceptual model, we assume that preferences for plan attributes are stable across individuals in the non-suspect ($n$) and suspect ($s$) groups conditional on demographics and drug consumption:

(3) $U^n_{ijt} = \alpha_{it} c_{ijt} + \beta_{it}\sigma^2_{ijt} + \gamma_{it} q_{jt} + \eta_{it}\Delta B_{ijt} + \delta_{it}\Delta P_{ijt} + \epsilon_{ijt}.$

(4) $U^s_{ijt} = \alpha_{it} \acute{c}_{ijt} + \beta_{it}\acute{\sigma}^2_{ijt} + \gamma_{it} \acute{q}_{ijt} + \eta_{it}\Delta\acute{B}_{ijt} + \delta_{it}\Delta\acute{P}_{ijt} + \epsilon_{ijt}.$

We dropped the accents in (3) to indicate that we are using our empirical measures of plan attributes for the non-suspect group.

[9] Latent heterogeneity in beliefs is one case of what Bernheim and Rangel refer to as "ancillary conditions" on decision making.

Because we do not observe the beliefs of people making suspect choices, we do not necessarily identify their preferences from their observed behavior. To see this notice that if we replace the subjective beliefs about plan attributes in (4) with empirical measures of plan attributes then, in general, we must also allow the values of the preference parameters and the error term to change in order to maintain their utility ranking of plans:

(5) $U_{ijt}^{s} = \acute{\alpha}_{it} c_{ijt} + \acute{\beta}_{it} \sigma_{ijt}^{2} + \acute{\gamma}_{it} q_{ijt} + \acute{\eta}_{it} \Delta B_{ijt} + \acute{\delta}_{it} \Delta P_{ijt} + \acute{\epsilon}_{ijt}$.

For example, if people make suspect choices because they have downward biased expectations about their drug needs at the time they choose a plan (i.e. $c_{ijt} > \acute{c}_{ijt}$) then we would expect $\alpha_{it} < \acute{\alpha}_{it}$. Likewise, if they have downward biased expectations about their potential savings from switching plans, then we would expect $\eta_{it} < \acute{\eta}_{it}$ and $\delta_{it} < \acute{\delta}_{it}$.

To facilitate estimation we assume that the person-plan specific taste shocks in (3) and (5) are *iid* draws from type I extreme value distributions. The variances may differ between the suspect and non-suspect groups because the idiosyncratic shocks in (5) will absorb any residual utility differences needed to maintain the preference ordering over plans when we move from (4) to (5). Therefore, when we normalize the model variances to $\pi^2/6$, the coefficients estimated for the suspect group will be scaled by the ratio of the group-specific variances. After making this normalization, we can rewrite the estimating equation for the suspect group ($s$) as

(6) $U_{ijt}^{s} = \alpha_{it}^{s} c_{ijt} + \beta_{it}^{s} \sigma_{ijt}^{2} + \gamma_{it}^{s} q_{ijt} + \eta_{it}^{s} \Delta B_{ijt} + \delta_{it}^{s} \Delta P_{ijt} + \epsilon_{ijt}$,

where $\alpha_{it}^{s} = \acute{\alpha}_{it}\sqrt{var(\epsilon_{ijt})/var(\acute{\epsilon}_{ijt})}$ and similarly for $\beta_{it}^{s}$, $\gamma_{it}^{s}$, $\eta_{it}^{s}$, and $\delta_{it}^{s}$. Our econometric model identifies the parameters of (3) and (6).

### *D. Identification*

Once we divide enrollment decisions into suspect and non-suspect groups the identification of model parameters for each group is straightforward and analogous to prior studies that assume consumers have identical beliefs (Lucarelli, Prince and Simon 2012, Polyakova 2016). Intuitively, our ability to observe each individual's plan choice when they first enter the market allows us to overcome the initial conditions problem. Consider the non-suspect group. Given the parametric form for utility and the distributional assumption about $\epsilon_{ijt}$, we can use a multinomial logit model of initial plan choices to identify the parameters that describe how marginal rates of substitution between cost, variance, and quality vary with demographics, $\alpha_0, \alpha_1, \beta_0, \beta_1, \gamma_0, \gamma_1$. Then we can use a model of their subsequent plan choices to identify the inertia parameters, $\eta_0, \eta_1, \delta_0, \delta_1$, via the rates at which individuals actively switched out of their initial plans. In practice, we pool data from all plan choices and estimate the parameters simultaneously using (3). The same arguments can be made to identify the parameters of (6) for the suspect group. From a policy evaluation perspective, the novelty of our approach is to estimate separate parameters for suspect and non-suspect groups. Differentiating their decision processes and allowing those processes to vary with beneficiaries' demographics is critical to accurately measuring the heterogeneous welfare effects of prospective policies.

## III. Welfare Effects of Choice Architecture Policies

When some decisions are misinformed, reforms that reduce information costs and/or simplify the choice process can, in principle, increase some consumers' welfare. Consider a policy implemented between periods 0 and 1 that changes the set of available plans from $J$ to $K$. Consumer welfare may be affected through multiple channels. The policy may change the menu of options by adding choices, removing choices, and regulating their costs or quality. The policy may also change how consumers make decisions, e.g. by lowering the cost of switching plans or by changing default assignment rules.[10]

[10] In general equilibrium, if the policy induces consumers and firms to adjust their behavior then those adjustments may feed back into the levels of endogenous attributes such as premiums.

### A. Non-Suspect Group

The expected change in welfare for people in the non-suspect group ($n$) is derived by integrating over $\epsilon_{ijt}$ in the standard expression for consumer surplus to generate the log sum ratio from Small and Rosen (1981).

$$(7)\ \Delta E[CV_i^n] = \frac{1}{\alpha_{it}^n}\left\{ln\frac{\sum_{k\in K}\left[exp\left(V_{ik}^{n1}\right)\right]}{\sum_{j\in J}\left[exp\left(V_{ij}^{n0}\right)\right]}\right\},$$

where $V_{ij}^{n0}$ and $V_{ik}^{n1}$ denote the observed part of utility in (3) evaluated for PDPs $j$ and $k$ before and after the policy. The temporal subscript is suppressed for brevity such that $V_{ij}^{n0} = V_{ijt}^{n0}(\theta^n, d_{it}) = U_{ijt}^{n0} - \epsilon_{ijt}$, where $\theta^n = [\alpha^n, \beta^n, \gamma^n, \eta^n, \delta^n]$ and each letter is a vector of parameters describing how preferences vary with demographics.

### B. Suspect Group

Welfare calculation is more involved for the suspect group. The observed part of (6) determines how PDP attributes affect their enrollment decisions, but their ex post realized utility from those decisions is determined by (3). This follows from our assumption that, conditional on prescription drug use and demographics, the suspect and non-suspect groups share the same underlying preference parameters. Therefore, a single plan's contribution to expected utility is defined by integrating over the product of (3) and the probability of choosing that plan based on (6). Aggregating over the PDP menu prior to the policy yields the following expression

$$(8)\ E[U_i^{s0}] = \sum_{j\in J}\int_{-\infty}^{\infty}\left(V_{ij}^{n0} + \epsilon_{ij}\right)F_j\left(V_{ij}^{s0} - V_{i1}^{s0} + \epsilon_{ij}, \ldots, V_{ij}^{s0} - V_{iK}^{s0} + \epsilon_{ij}\right)d\epsilon_{ij},$$

where $F_j(\cdot)$ is the derivative of the joint CDF of the preference shocks with respect to $\epsilon_{ij}$. Subtracting this expression from the post-policy measure of expected utility, dividing by the marginal utility of income, and integrating over the preference shocks yields the following expression for welfare:

(9) $\Delta E[CV_i^s] = \frac{1}{\alpha_{it}^n}\left\{ln\frac{\sum_{k\in K}[exp(V_{ik}^{s1})]}{\sum_{j\in J}\left[exp\left(V_{ij}^{s0}\right)\right]} + \sum_{k\in K}\left[\psi_{ik}^{s1}\left(V_{ik}^{n1} - V_{ik}^{s1}\right)\right] - \sum_{j\in J}\left[\psi_{ij}^{s0}\left(V_{ij}^{n0} - V_{ij}^{s0}\right)\right]\right\}$,

where $V_{ij}^{s0} = V_{ijt}^{s0}(\theta^s) = U_{ijt}^{s0} - \epsilon_{ijt}$, $\theta^s = [\alpha^s, \beta^s, \gamma^s, \eta^s, \delta^s]$, and $\psi_{ij}$ is the logit probability of choosing plan $j$ so that $\psi_{ij}^{s0} = exp\left(V_{ij}^{s0}\right)/\sum_{m\in J}[exp(V_{im}^{s0})]$. The first term inside braces in (9) is the standard log sum ratio evaluated at $\theta^s$. The second and third terms adjust the log sum ratio to account for the welfare implications of the difference between $\theta^s$ and $\theta$ for each choice, weighted by the predicted probability of making that choice before and after the policy.[11] In the special case where $\theta^s = \theta$, equation (9) reduces to the standard welfare measure in (7).

### *C. Bounding the Welfare Implications of Inertia*

Equations (7) and (9) treat the non-suspect group's inertia parameters as being directly relevant for welfare. This is consistent with interpreting inertia as a mixture of latent preferences and hassle costs of switching plans. Kling et al. (2002) argue that inertia is more likely to reflect downward biased expectations for the savings from switching plans along with other psychological factors such as status quo bias, procrastination, and limited attention or inattention. These mechanisms have no direct effect on consumer welfare; they affect welfare indirectly by lowering the rate at which consumers switch plans. Our data do not allow us to distinguish the importance of psychological bias relative to latent preferences and switching costs. One can separate them, in principle, by adding assumptions on the form of statistical distributions for unobserved preference heterogeneity and switching costs (e.g. Heckman 1981, Dube et al. 2010, Polyakova 2016, Heiss et al. 2016). We avoid such assumptions by taking a partial identification approach similar to Handel (2013) and Bernheim, Fradkin, and Popov (2015). We calculate welfare for two extreme cases that provide bounds on the share of inertia that is welfare relevant. In the first case, inertia is assumed to be entirely welfare relevant (as in (7) and (9)) and in the second case it is assumed to be entirely irrelevant, e.g. due to psychological bias.

[11] Leggett (2002) derived a similar expression as a way to describe decision making under misinformation in a static model of recreation demand without inertia.

To calculate the change in expected welfare when inertia reflects psychological biases we replace equations (7) and (9) with (7') and (9').

$$(7')\ \Delta E[CV_i^n] = \frac{1}{\alpha_{it}^n}\left\{ln\frac{\sum_{k\in K}[exp(V_{ik}^{n1})]}{\sum_{j\in J}\left[exp\left(V_{ij}^{n0}\right)\right]} + \sum_{k\in K}\left[\psi_{ik}^{n1}\left(V_{ik}^{n^*1} - V_{ik}^{n1}\right)\right] - \sum_{j\in J}\left[\psi_{ij}^{n0}\left(V_{ij}^{n^*0} - V_{ij}^{n0}\right)\right]\right\}.$$

$$(9')\ \Delta E[CV_i^s] = \frac{1}{\alpha_{it}^n}\left\{ln\frac{\sum_{k\in K}[exp(V_{ik}^{s1})]}{\sum_{j\in J}\left[exp\left(V_{ij}^{s0}\right)\right]} + \sum_{k\in K}\left[\psi_{ik}^{s1}\left(V_{ik}^{n^*1} - V_{ik}^{s1}\right)\right] - \sum_{j\in J}\left[\psi_{ij}^{s0}\left(V_{ij}^{n^*0} - V_{ij}^{s0}\right)\right]\right\}.$$

These equations differ from (7) and (9) in that $V_{ik}^{n^*1} = V_{ik}^{n1} - \eta_{it}^n \Delta B_{ijt} - \delta_{it}^n \Delta P_{ijt}$ and $V_{ij}^{n^*0} = V_{ik}^{n0} - \eta_{it}^n \Delta B_{ijt} - \delta_{it}^n \Delta P_{ijt}$. Hence, in this case inertia has no direct effect on consumer welfare; it only affects welfare indirectly via consumers' enrollment decisions.

*D. Bounding the Policy's Effect on Consumer Behavior*

We may also need to take a stance on whether a counterfactual choice architecture policy would induce consumers to behave differently. In principle, a policy designed to simplify the choice process could induce decision makers in the suspect group to update their beliefs about the market and behave like decision makers in the non-suspect group. Or it could have no effect at all. In the absence of empirical evidence, we can again take a partial identification approach and consider two extreme scenarios. One scenario assumes that the policy has no effect on behavior; the other assumes that the policy induces consumers in the suspect group to behave like those in the non-suspect group, conditional on demographics and prescription drug utilization. The second case involves replacing $V_{ik}^{s1}$ with $V_{ik}^{n1}$ and $\psi_{ik}^{s1}$ with $\psi_{ik}^{n1}$ in equations (9) and (9').

*E. Discussion*

Our welfare framework is consistent with divergent theories of consumer decision making. When it is costly for consumers to acquire information, to make a decision, or to negotiate a transaction they may choose not to become fully informed (Stigler and Becker 1977). Misinformation may also stem from psychological biases (Kahneman, Wakker, and

Sarin 1997).[12] Our framework requires observing which decisions are affected by some combination of these mechanisms, but it avoids the need to model them or take a stance on their relative importance. The disadvantage of being unable to disentangle these mechanisms is that we only recover bounds on welfare. Whether the bounds are informative is an empirical question.

The bounds that we derive extend Small and Rosen (1981) to recognize that consumers differ in the information they use to make decisions. Our adjustment for misinformation implements Bernheim and Rangel's (2009) proposal for how to measure welfare when the analyst suspects that some choices will not reveal preferences. This allows us to recognize that choice architecture may create winners and losers. For example, consider a policy that automatically assigns each consumer to a plan, but allows them to opt out and choose a different plan if they prefer. Nobody can be made better off from such a policy within a standard discrete choice model that assumes all consumers are fully informed and freely mobile, as in Lucarelli, Prince, and Simon (2012). At the opposite extreme, nobody can be made worse off within a model that assumes the policy is implemented by a benevolent and omniscient regulator, as in Abaluck and Gruber (2011). Our framework nests both extremes as special cases. If we assign all consumers to the non-suspect group, then our framework reduces to the standard discrete choice model. If we assign all consumers to the suspect group and let the analyst decide which plans would maximize consumers' utilities, then our model reduces to the one in Abaluck and Gruber (2011). Our framework generalizes these approaches to allow for a middle ground in which we use ancillary information to determine which consumers are misinformed and use informed consumers' choices to reveal preferences. Equation (7) and its analogs recognize that informed consumers can be made worse off from restrictions on choice. Equation (9) and its analogs recognize that that misinformed consumers may gain or lose from restrictions on choice. Aggregating the gains and losses can yield criteria for policy evaluation consistent with the concept of asymmetric paternalism (Camerer et al. 2003).

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[12] To use the terminology from Kahneman, Wakker, and Sarin (1997), one can think of $V_{ij}^{n}(\theta)$ as approximating the "hedonic utility" derived by consuming a good and $V_{ij}^{s}(\theta^{s})$ as approximating the "decision utility" function maximized by people who are misinformed.

Applying our model to a prospective policy involves three steps. First we must use signals about which decision makers are informed to divide their choices into suspect and non-suspect groups. Then we must estimate parameters describing how suspect and non-suspect choice probabilities vary with plan attributes, $\theta^n$ and $\theta^s$, to calibrate $\psi_{ij}^{s0}$, $V_{ij}^{n0}$, $V_{ij}^{s0}$, and $V_{ik}^{n^*0}$. Finally we must map the policy onto plan attributes and utility to calibrate $\psi_{ij}^{s1}$, $V_{ij}^{n1}$, $V_{ij}^{s1}$, and $V_{ik}^{n^*1}$ and calculate bounds on welfare. In the remainder of this paper we implement each step and evaluate prospective changes to Medicare Part D choice architecture using data drawn from beneficiaries' administrative records together with surveys of the decision makers who made their enrollment choices.

## IV. Linking Administrative Records on Health Insurance to Enrollee Surveys

We rely on the Medicare Current Beneficiary Survey (MCBS) linked to the respondents' administrative records at the US Centers for Medicare and Medicaid Services (CMS). The MCBS is a national rotating panel questionnaire that began in 1991 and is administered to approximately 16,000 people annually.[13] It collects information about Medicare beneficiaries and their use of health care services. Each participant is interviewed up to three times per year for four consecutive years, regardless of whether they stay at the same address or move into and out of long-term care facilities. Importantly for our purposes, participants are tested on their knowledge of the PDP market. The MCBS also asks participants if and how they searched for information about Medicare services and it provides rich demographic data. Also of particular value for our study, the MCBS indicates whether a proxy responded to the survey, and whether the beneficiary makes health insurance decisions on her own, with help from someone else, or whether the proxy makes decisions

[13] A potential limitation of working with the MCBS sample is that it is not designed to be nationally representative without weighting, and selecting the appropriate weights is complicated by panel rotation and by our exclusive focus on respondents who participated in the standalone PDP market without a low-income subsidy. Respondents who do not purchase a standalone PDP can instead obtain prescription drug insurance through an employer sponsored plan or a Medicare Advantage plan. Further, the MCBS does not sample individuals from 3 PDP regions: 1(Maine and New Hampshire), 20 (Mississippi), and 31 (Idaho and Utah). To assess whether using unweighted MCBS data might compromise the external validity of our results, we compared the unweighted demographics of the average enrollee in our linked sample with a random 20% sample of all Part D enrollees from CMS's administrative files. Table A2 shows that the average enrollee in our linked sample is 1 to 2 years older. Otherwise, the two samples are virtually identical in terms of race, gender, rates of dementia and depression, number of PDP brands and plans available, expenditures on plan premiums and OOP costs, and the maximum amount of money that the average enrollee could have been saved by enrolling in their cheapest available plan. Given the strong similarity between the two samples, we expect that our findings from the linked MCBS-administrative sample can be generalized to the broader population of non-poor Part D enrollees.

for her.

For each MCBS respondent who purchased a standalone PDP between 2006 and 2010 we obtained administrative records on the universe of their prescription drug claims, the set of PDPs available to them, and their annual enrollment decisions. Then we calculated what each enrollee would have spent had they purchased the same bundle of drugs under each alternative PDP in their choice set. This was done by combining their actual claims with the cost calculator developed in Ketcham, Lucarelli and Powers (2015). Briefly, this calculator incorporates information on each plan's prices paid and OOP prices for every drug as determined by their coverage decisions (i.e., formulary design). This allowed us to determine what each person would have paid for each drug and their total OOP costs for the year under every plan, as well as what each plan would have spent on each person had they enrolled in that plan and consumed the same drugs as under the plan they actually chose. The calculator factors in the non-linearities in benefits design (i.e., changes in OOP costs depending on the cumulative spending in the coverage year) as people move through the deductible, "donut hole" coverage gap, and catastrophic coverage phases.[14] Like prior studies of PDP choice, we limit our analysis to enrollees who did not receive a low-income subsidy.[15] Finally, we used administrative data from CMS's Chronic Condition Data Warehouse to determine if and when each individual had depression or dementia, which are associated with diminished cognitive performance (Agarwal et al. 2009).

Our linked sample includes 3,547 individuals who made 10,867 annual enrollment decisions between 2006 and 2010.[16] Table A1 reports annual means of the key variables. The typical enrollee is a retired high school graduate with living children. Approximately 22% are college graduates, 55% are married, and 55% have annual pre-tax household incomes over $25,000. Only 35% report that they ever personally use the internet to get information

[14] The calculator code is available at https://www.aeaweb.org/articles?id=10.1257/aer.20120651. There is a correlation of .94-.98 each year between the out of pocket costs predicted for the actual plan and the realized cost observed in the administrative data. Differences between the calculator's predictions and realized costs are due to changes in plan design or drug pricing that occur after open enrollment and are not observable to consumers at the time they make enrollment decisions.

[15] We exclude those receiving low-income subsidies because they are autoenrolled into plans, they receive larger premium subsidies, and their copayments are much more uniform across plans. Hence, they are less relevant for our evaluation of prospective policies designed to alter choice architecture. Despite excluding them, our sample has similar income levels to the national average of people age 65 and above. In our sample 54% of households have annual income over $25,000 (weighted 2006-2010 dollars), compared with 63% (constant 2010 dollars) based on all householders 65 and older in the 2010 Census American Community Survey.

[16] This excludes observations on beneficiaries who reenrolled in plans they had originally chosen prior to joining the MCBS. We drop these observations because we cannot observe the beneficiaries' knowledge at the time they first selected their current plans.

of any kind. However, among those who do use the internet most have used it to search for information on Medicare programs (27%). Another 17% report having called 1-800-Medicare for information. The average beneficiary's total expenditures on premiums and out of pocket costs increased from $1,203 in 2007 to $1,400 in 2010.[17] This is a significant share of income given that 45% of beneficiaries have household incomes below $25,000. The data also reveal that by the end of our study period significant fractions of enrollees had been diagnosed with dementia (12%) and depression (11%).

Given the relatively large amount of money at stake, the age range of the eligible population and the prevalence of cognitive illnesses it is unsurprising to find that 38% of enrollees did not make health insurance decisions on their own: 27% had help and 11% relied on a proxy to make the decision for them. Table 1 shows that beneficiaries who get help are likely to be older, sicker, lower income, less educated, and use the internet less than beneficiaries who made decisions on their own. Those getting help are also more likely to have been diagnosed with depression or dementia. All of these differences are amplified when we compare beneficiaries who make their own health insurance decisions to those who rely on proxies to make decisions for them.

Only 8% of the enrollment decisions in our data minimize ex post expenditures. In 2006, the average enrollee could have saved $460 by choosing their cheapest available plan.[18] This is equivalent to reducing total expenditures by 45%. Potential savings declined to $349 in 2007 (or 29% of expenditures) and remained similar thereafter. Why are people leaving money on the table? We hypothesize that the answers differ from person to person. Some may be making informed decisions to pay more for plans that provide better risk protection and higher quality. Others may misunderstand how the market works or underestimate their potential savings. We must distinguish between these groups to evaluate the welfare effects of prospective choice architecture policies.

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[17] The figure for 2006 is $1,013. It is smaller because during the inaugural year of the program open enrollment extended through May. Less than half the enrollees in our sample were enrolled for all of 2006. If we limit the sample to full-year enrollees, the 2006 mean annual consumer expenditure is $1,366.

[18] This figure sums over premiums and out of pocket costs. See Table A1 for details. This average falls below the $520 figure reported by Ketcham, Lucarelli and Powers (2015) based on CMS's 20% sample of 2006 full year enrollees because our average also includes people who only enrolled for part of the year. The primary reason for part-year enrollment in 2006 was the fact that the initial open enrollment period was extended through May (Heiss, McFadden, and Winter 2010).

TABLE 1—CHARACTERISTICS OF PEOPLE WHO MAKE THEIR OWN DECISIONS OR GET HELP

| | Who makes health insurance decisions? | | |
|---|---|---|---|
| | Beneficiary | Beneficiary gets help | Proxy |
| number of enrollment decisions | 6,790 | 2,906 | 1,171 |
| high school graduate (%) | 83 | 75 | 61 |
| college graduate (%) | 25 | 19 | 14 |
| income>$25k (%) | 57 | 53 | 48 |
| uses the internet (%) | 39 | 33 | 18 |
| mean age | 77 | 78 | 80 |
| dementia including Alzheimer's (%) | 5 | 11 | 31 |
| depression (%) | 9 | 11 | 14 |
| mean number of drug claims | 32 | 36 | 40 |
| mean premium ($) | 416 | 411 | 426 |
| mean out-of-pocket costs ($) | 885 | 1,030 | 1,285 |
| mean potential savings ($) | 325 | 325 | 357 |

Note: The table reports means for key variables for the sample of Medicare Part D enrollees found in both the MCBS and cost calculator samples from 2006-2010. See the text for details.

## V. Identifying Suspect and Non-Suspect Choices

A central aspect of our model is the need to identify the subset of suspect choices that will not necessarily reveal the beneficiary's preferences for plan attributes. This process is potentially controversial because we do not fully observe decision makers' beliefs in the data. We address this by implementing a variety of approaches, finding that the magnitudes of key results vary across approaches but our qualitative findings do not. Our primary approach is to classify an enrollment decision as *suspect* if the decision maker reveals that she misunderstands the primary source of variation in drug spending across plans, if her enrollment decision violates basic axioms of consumer theory, or both. After we explain the nuances of our primary approach, we discuss several alternatives and provide reduced form evidence on who makes suspect choices.

### *A. Our Primary Approach*

We focus on the signals that decision makers send about their beliefs at the time they actively enroll in plans. Similar to Chetty et al. (2015) and Ho, Hogan and Scott-Morton

(2015) we define an enrollment choice as *active* if either of the following statements is true: (i) the person is new to the market and must select a plan to become insured or (ii) the person switched to a new plan during open enrollment. If neither statement is true, then the decision maker took no action during open enrollment and was automatically reenrolled in the plan she chose last year—her default—in which case we define her choice as *passive*. After the inaugural enrollment cycle in 2006 between 77% and 80% of enrollees made passive choices each year. When coding a passive reenrollment decision as suspect or non-suspect we focus on the signals sent by the decision maker in the period when she actively enrolled in that plan.[19]

The first signal comes from a module of the MCBS survey that was implemented in 2006 to 2010 to test respondents' knowledge of PDP markets. Most of the test questions asked about institutional features of the markets that were neutral to the choice among plans. As described in detail in Table B1, only one of these questions is suitable for identifying informed choices.[20] This single question tested an area of knowledge that is critical to the choice among plans. It asked decision makers to state whether the following sentence is true or false.

*"Your OOP costs are the same in all Medicare prescription drug plans*."

For the small subset of beneficiaries with no drug claims, the statement is true. For every beneficiary with any claims the statement is false due to variation in formularies, deductibles and coinsurance. Understanding that costs vary across plans is key to understanding how the PDP markets work. Moreover, this variation is financially important: the average beneficiary's OOP costs for her purchased drugs vary by over $1,100 across her available plans. Decision makers who do not understand this may choose plans with higher out of

[19] While we could focus on signals sent at the time of the reenrollment decision, doing so would require taking a stronger stance on the welfare interpretation of inertia. Deference to active decisions is common in the literature (e.g. Handel 2013, Chetty et al. 2015, Ho, Hogan and Scott-Morton 2015, Polyakova 2016).

[20] Unlike Handel and Kolstad (2015) who used an index from a range of questions, we had no input into the design of the questions asked. All other questions in the MCBS were either redundant relative to the question we use, infrequently asked (e.g. for only a single year, making it non-viable for our study), focused on institutional features of PDP markets that were irrelevant to the choice among plans, or were about general efforts to collect information about Medicare (e.g. calling the Medicare 1-800 number) rather than knowledge about Part D specifically as our current measure. Therefore we believe the index approach is inferior to our use of the single most relevant question because it would be a less informative signal about the areas of knowledge that matter for evaluating choices in our context.

pocket costs and less protection from unexpected health shocks. Moreover, failure to give a correct answer sends a strong signal that the decision makers' beliefs deviate from our empirical measures of plan attributes. Thus, we assign an enrollment choice to the suspect group if the decision maker did not answer this question correctly.

We use each beneficiary's drug claims to determine her correct answer to the MCBS question. Because respondents may be unsure about which enrollment year the question is referring to, we code a respondent's answer for year *t* as correct if it is correct for either year *t* or year *t-1.* The first row of Table 2 shows that 44% of respondents answered incorrectly in the first year of the program when everyone had to actively enroll in a plan. The table shows gradual improvement over the next four years, consistent with prior evidence on learning in PDP markets (Ketcham, Lucarelli, and Powers 2015, Ketcham et al. 2012).[21] On average, respondents who answered incorrectly could have saved 16% more by switching to a different plan than those who answered correctly.[22]

TABLE 2—INDICATORS OF SUSPECT CHOICES

| Suspect choice indicator | Percent of choices | | | | | Percent with E[CS]>0 | |
|---|---|---|---|---|---|---|---|
| | 2006 | 2007 | 2008 | 2009 | 2010 | menu restriction | default assignment |
| fails knowledge test | 44 | 37 | 34 | 29 | 28 | 19 | 79 |
| plan dominated ex post | 19 | 18 | 18 | 16 | 15 | 20 | 77 |
| fails knowledge test \| plan dominated ex post | 54 | 48 | 45 | 40 | 38 | 23 | 81 |
| fails knowledge test \| plan dominated ex ante | 54 | 49 | 46 | 40 | 38 | 24 | 82 |
| fails knowledge test \| plan dominated ex post \| save > 50% | 61 | 55 | 51 | 45 | 43 | 30 | 82 |

Note: The table reports the share of choices triggering each indicator, by year, and previews the sensitivity of some of our results to the choice among indicators. The MCBS knowledge test (row 1) determines whether decision makers understand that out of pocket costs vary across plans. The dominated plan test (row 2) determines whether their chosen plans are dominated on expected costs, variance and quality based on ex post drug claims during the enrollment year. Row 3 reports our primary indicator—the union of rows 1 and 2. Row 4 is the same as Row 3 but implements the dominated plan test using ex ante drug claims from the prior year. Row 5 is the union of Row 3 and an indicator for whether ex post costs could have been lowered by more than 50% by choosing another plan. See the text for additional details. The last two columns preview the results from two of our policy experiments described below. E[CS] denotes expected consumer surplus.

[21] Table A3 reports separate results for active enrollment decisions and passive reenrollment decisions.

[22] Table A4 shows that when we focus on active enrollment decisions, failing to answer the knowledge question correctly is associated with a 1.3 percentage point increase in the probability of choosing a dominated plan and a $68 increase in the amount of money that could be saved by switching to the cheapest available plan, even when conditioning on education, income, employment status, presence of living children, internet use, effort to search for information about CMS programs online or by calling 1-800-Medicare, getting help making enrollment decisions, the number of available plans, gender, race, age, dementia, depression, number of drug claims, and dummies for year and CMS region. For 11% of our sample the person who responds to the survey and makes the enrollment decision is a proxy for the beneficiary, such as a spouse or child (Table A1).

Answering the knowledge question correctly is necessary, but not sufficient, for us to code a choice as *non-suspect*. A decision maker may understand how the market works in general but choose not to exert effort to learn about the attributes of her available options. Therefore, we also test whether decision makers' active enrollment decisions can be rationalized as maximizing a well behaved utility function under full information, using the test from Ketcham, Kuminoff and Powers (2016). Assuming that beneficiaries are weakly risk averse and have preference orderings that are complete, transitive, and strongly monotonic over expected cost savings, risk protection, and quality, an informed decision maker will not actively enroll in a plan, $j$, that is dominated by another, *k,* in the sense that the following four conditions hold simultaneously:

(10) $E(c_{ikt}) \leq E(c_{ijt})$

(11) $var(c_{ikt}) \leq var(c_{ijt})$

(12) $q_{ijt} \leq q_{ikt}$

(13) $At\ least\ one\ of\ the\ inequalities\ is\ strict.$

We refer to choices that satisfy (10)-(13) as being *dominated*.[23] In theory, an individual may choose a dominated plan if she is risk loving, if she dislikes quality, if she has a negative marginal utility of income, or, more likely, if she is misinformed about her options. Hence, if we observe someone actively choosing a dominated plan we assign her choice to the suspect group.[24]

To test whether enrollees chose dominated plans we define cost, variance, and quality analogously to prior studies of PDP choice (Abaluck and Gruber 2011, Ketcham, Kuminoff, and Powers 2016). First we assume that informed consumers have unbiased expectations of their drug needs for the upcoming year: $E(c_{ijt}) = c_{ijt}$. Next, we use a cohort approach to calculate variance. We calculate $var(c_{ijt})$ from the distribution of expenditures

[23] By focusing on the first two moments of the cost distribution, our definition of dominance differs from measures of state-by-state dominance used by studies such as Handel and Kolstad (2015).

[24] Consumers who violate at least one condition are choosing plans on what Lancaster (1966) called the "efficiency frontier" in attribute space. Every plan on the frontier can be rationalized as maximizing some utility function that satisfies the preference axioms and weak risk aversion under full information. For example, an informed risk averse consumer may optimally choose a more expensive and lower quality plan that better insures her against negative health shocks.

under plan *j* for the drugs used in year *t* by people in consumer $i$'s cohort in terms of year *t-1* drug claims. Specifically, we use CMS's random 20% sample of all PDP enrollees to assign each individual in the MCBS sample to 1 of 1000 cells defined by the deciles to which she belonged in the national distributions of the prior year's total drug spending, days' supply of branded drugs, and days' supply of generic drugs.[25] Then we calculate $var(c_{ijt})$ for the distribution of drugs used by everyone in consumer *i*'s cell. Finally, we allow utility to depend on two measures of plan quality. First is an index measure developed by CMS and publicized as "star ratings". This index is based on factors such as customer complaints, customer satisfaction, difficulty with appeals and availability of benefits and pricing information. Second are indicators for insurance companies. These indicators reflect all aspects of PDP quality that vary across insurers even beyond those omitted from the star ratings, such as customer service, pharmacy networks, mail order options, and prior authorization requirements.[26] In identifying consumer-specific quality preferences for insurance brands, we maintain the revealed preference assumption and infer that they view their chosen brand as superior to all others. In most cases, the brand-based quality indicators subsume the star ratings because there is no within-brand variation in the star ratings. However, in some cases CMS generated separate indexes for different plans within a given brand. In these cases, both quality metrics enter our definition of suspect choices.

Because we allow utility to depend on insurer dummies, a chosen plan will be dominated if and only if the enrollee could have chosen a different plan offered by the same insurer that would have lowered the mean and variance of her drug expenditures, or lowered one holding the other constant. The second row of Table 2 shows that 19% of beneficiaries actively enrolled in dominated plans in 2006, declining to 15% in 2010. The decline is partly due to people switching out of dominated plans and partly due to new enrollees being less likely to select a dominated plan.

We base our primary approach to identifying suspect choices on the union of dominated

[25] In cases where CMS did not have the person's drug claims from the prior year, such as 2006, we predicted their deciles based on current and future drug claims and past, current and future health.

[26] For example, stringent prior authorization requirements for certain drugs may be unattractive to consumers who believe they have a high likelihood of purchasing those drugs and irrelevant to consumers who do not. Likewise, consumers differ in their proximity to in-network pharmacies. These factors vary across insurance brands and consumers but not across plans within a brand.

plan choices and knowledge test failures. Between 38% and 54% of choices are assigned to this group each year, as shown by the middle row of Table 2. This definition recognizes that decision makers may understand how the market works in general without becoming informed about their individual choice sets. It also recognizes that decision makers may enroll in undominated plans even if they do not understand how the market works. In both cases, we suspect that choices will fail to reveal preferences. Conversely, if a decision maker passes the knowledge test and her enrollment choice can be explained by a well-behaved utility function then theory and data provide no basis for rejecting revealed preference assumptions, and we assign her to the non-suspect group.

### *B. Alternative Approaches to Defining Suspect Choices*

Table 2 previews the sensitivity of our findings to replacing our primary approach with alternatives that are either more inclusive or more exclusive in how they define suspect choices. The last two columns report the shares of consumers with expected welfare gains from counterfactual policies (analyzed in detail in section VII) that would limit insurers to selling no more than two plans per market (menu restriction) or reassign enrollees to cost minimizing plans but let them opt out (default assignment). Under our primary approach, 23% of consumers have expected welfare gains from the menu restriction whereas 81% have expected welfare gains from the assignment rule.

Rows 1 and 2 define suspect choices using the knowledge test alone and the dominated plan test alone. Row 4 uses the union of both tests—like our primary approach—but implements the dominated plan test assuming that consumers are myopic in the sense that they expect their drug needs in the upcoming year to match their actual drug use in the prior year: $E(c_{ijt}) = c_{jt}(x_{it-1})$. Because drug use is strongly persistent over time, this yields similar results to Row 3. Comparing across rows, a majority of choices defined as suspect in our primary definition are due to the consumers' lack of knowledge about plan design specifically rather than difficulty in forecasting their individual-specific drug utilization. Row 5 extends our primary approach to include as suspect any enrollment decision that results in the beneficiary being able to reduce costs by more than 50% by switching plans. This approach is more consistent with Kling et al. (2012), Heiss et al. (2013) and others who interpret money left on the table as a signal of poor decision making.

As we expand the suspect group the shares of consumers who benefit from choice architecture polices increase. However, the differentials are small. Moving from row 2 to row 5 more than doubles the number of people in the suspect group but increases the number who benefit from the policies by less than half. The intuitive reason for this stability is that when we add more people to the suspect group, the differences in observed behavior between the average individual in the two groups diminish. Mechanically, $\left|V_{ij}^{n^{*}0} - V_{ij}^{s0}\right|$ tends to decrease for the marginal individual in the suspect group, lowering the probability that she experiences a welfare gain from choice architecture policies. This feature of the data makes our qualitative policy conclusions robust to how we define suspect choices across a wide range of alternatives.

### *C. Who is More Likely to Make Suspect Choices?*

To develop intuition for potential mechanisms driving suspect choices, we estimate linear probability models in which the dependent variable, $S_{irt}$, is an indicator for whether person *i* in CMS region *r* made a suspect choice in the year *t* enrollment cycle,

(14) $S_{irt} = \kappa + \lambda d_{irt} + \phi_r + \rho_t + e_{it}$.

On the right of the equality $d_{irt}$ is a vector of demographics, some of which change over time, and $\rho_t$ and $\phi_r$ are indicators for enrollment year and region.[27]

The first column of Table 3 reports results for enrollment decisions from 2006-2010 using our primary approach to defining suspect choices. The omitted indicators define the reference person as a 65 to 69 year old unmarried and retired white male with no high school diploma who has not searched for information on CMS programs and makes his own enrollment decisions. The coefficients imply that obtaining a college degree is associated with a 5.8 percentage point reduction in the probability of making a suspect choice.

[27] These indicators capture variation in the complexity of choice sets across space and time. For example, in the first year of the program the number of available plans per region ranged from 27 to 52. The number of plans also changed over time, increasing noticeably between 2006 and 2007. This variation allows us to test the choice overload hypothesis that consumers are less likely to make informed decisions as the number of options grows. Ketcham, Lucarelli and Powers (2015) test choice overload in Part D more extensively, capitalizing on individual-specific variation in the number of plans available by the person's relative cost of those plans.

The probability is higher for nonwhites (+11.8) which might proxy for unobserved differences in wealth or education. The probability is lower for enrollees who searched for information about CMS programs using the internet (-9.0) or calling 1-800-Medicare (-5.8), but it is not any lower for beneficiaries who had help making enrollment decisions.[28]

TABLE 3—ASSOCIATION BETWEEN SUSPECT CHOICES AND DEMOGRAPHICS

| | Wrong answer and/or dominated plan | | | | Wrong answer | | Dominated plan | |
|---|---|---|---|---|---|---|---|---|
| | 2006-2010 | | 2007-2010 | | 2007-2010 | | 2007-2010 | |
| college graduate | -0.058 | [0.021]*** | -0.058 | [0.021]*** | -0.082 | [0.020]*** | 0.006 | [0.016] |
| income>$25k | -0.012 | [0.018] | -0.012 | [0.019] | -0.029 | [0.018] | 0.028 | [0.014]** |
| currently working | 0.011 | [0.025] | 0.009 | [0.026] | 0.004 | [0.024] | -0.005 | [0.019] |
| married | 0.012 | [0.020] | 0.011 | [0.020] | 0.003 | [0.020] | 0.007 | [0.015] |
| has living children | -0.057 | [0.033]* | -0.064 | [0.034]* | -0.024 | [0.033] | -0.053 | [0.028]* |
| uses the internet | -0.020 | [0.021] | -0.015 | [0.022] | -0.006 | [0.020] | -0.004 | [0.016] |
| searched for CMS info: internet | -0.090 | [0.021]*** | -0.083 | [0.021]*** | -0.086 | [0.020]*** | -0.020 | [0.015] |
| searched for CMS info: 1-800-Medicare | -0.058 | [0.019]*** | -0.066 | [0.020]*** | -0.055 | [0.018]*** | -0.003 | [0.016] |
| has help making insurance decisions | 0.025 | [0.017] | 0.016 | [0.018] | 0.018 | [0.017] | -0.002 | [0.013] |
| number of available plans (standardized) | -0.005 | [0.014] | -0.003 | [0.016] | -0.001 | [0.014] | -0.010 | [0.013] |
| female | 0.024 | [0.019] | 0.028 | [0.019] | 0.032 | [0.019]* | 0.015 | [0.014] |
| nonwhite | 0.118 | [0.035]*** | 0.114 | [0.036]*** | 0.115 | [0.036]*** | 0.018 | [0.026] |
| age: 70-74 | 0.050 | [0.021]** | 0.047 | [0.023]** | 0.079 | [0.019]*** | -0.010 | [0.019] |
| age: 75-79 | 0.066 | [0.025]*** | 0.065 | [0.027]** | 0.101 | [0.024]*** | -0.009 | [0.021] |
| age: 80-84 | 0.072 | [0.027]*** | 0.071 | [0.028]** | 0.119 | [0.026]*** | -0.009 | [0.022] |
| age: over 84 | 0.120 | [0.029]*** | 0.118 | [0.030]*** | 0.166 | [0.028]*** | 0.005 | [0.024] |
| dementia including Alzheimer's | 0.048 | [0.026]* | 0.040 | [0.027] | 0.049 | [0.027]* | 0.001 | [0.020] |
| depression | 0.012 | [0.022] | 0.011 | [0.023] | 0.014 | [0.023] | -0.006 | [0.018] |
| number of drug claims (standardized) | 0.027 | [0.008]*** | 0.033 | [0.008]*** | 0.028 | [0.008]*** | 0.017 | [0.006]*** |
| number of plan choices | 10,867 | | 9,119 | | 9,119 | | 9,119 | |
| number of enrollees | 3,547 | | 3,444 | | 3,444 | | 3,444 | |
| mean of the dependent variable | 0.44 | | 0.42 | | 0.32 | | 0.17 | |
| R-squared | 0.064 | | 0.059 | | 0.077 | | 0.020 | |

Note: The table reports coefficients and standard errors from linear probability models of individual's plan choices. The dependent variable equals one if we suspect the choice was misinformed. See the text for a formal definition. All explanatory variables are binary except the number of available plans and the number of drug claims, both of which are standardized. The omitted indicators define the baseline enrollee as a 65 to 69 year old white male who did not finish high school, has income below $25k, does not get help making insurance decisions, has not searched for CMS information using the internet or 1-800-Medicare, has the mean number of drug claims, and has not been diagnosed with dementia or depression. All regressions include indicators for enrollment year and region. Robust standard errors are clustered by enrollee. *,**, and *** indicate the p-value is less than 0.1, 0.05, and 0.01 respectively.

Looking at the administrative variables, the probability of making a suspect choice is

[28] The lower probability for those calling 1-800-Medicare is consistent with Kling et al.'s (2012) audit of the Medicare help line in which actors calling the number for information found that customer service representatives consistently identified low-cost plans based on the actors' fictional drug needs. The positive (but insignificant) coefficient for those getting help could be driven by principal agent problems, the helpers' opportunity costs of time, and/or added complexity in the decision process because those getting help tend to use more drugs and are more likely to be diagnosed with dementia and depression (Table 1).

increasing in age, consistent with prior evidence on the decline in cognitive performance for individuals over 65 (Agarwal et al. 2009, Tymula et al. 2013). The predicted probability is approximately 7 percentage points higher for enrollees in their late 70's and 12 percentage points higher for enrollees in their late 80's. This is after controlling separately for diagnosed cognitive illnesses normally associated with aging, namely dementia (+4.8), and conditioning on the increased complexity of decision making associated with greater drug needs via a measure of total drug claims (+2.7 for a one standard deviation increase in claims). Having living children, even conditional on receiving help choosing, is associated with a nearly 6 percent reduction in the probability of making a suspect choice. In comparison, we find that income, gender, and marital status have small and statistically insignificant effects. We also obtain a precisely estimated zero on the number of available plans, providing evidence against the hypothesis that choice overload causes suspect choices (Ketcham, Lucarelli and Powers 2015).

The second column of Table 3 shows that the results are largely unchanged if we drop 2006. We exclude 2006 enrollment decisions from our main analysis because of the improvement in knowledge question responses in 2007. Because consumers appear to have learned during the inaugural year of the program, their choices in that first year may be less informative for analyzing prospective policies. That said, we show that our main findings are invariant to whether we include or exclude 2006 choices. Finally, the last two columns show that most of the demographic associations are driven by the MCBS knowledge test.

## VI. Structural Model Estimates and Validation Tests

### *A. Main Multinomial Logit Results*

Table 4 presents the estimates that we use as the basis for policy experiments.[29] The first column reports results for a conventional model that ignores heterogeneity in consumers' decision-making processes by pooling data on suspect and non-suspect choices. The

[29] We also estimated more flexible models that interacted PDP attributes with more comprehensive sets of demographic variables. However the additional interactions tend to have small and statistically insignificant effects (Table A5), which led us to use the more parsimonious specification in Table 4. A notable result from the more comprehensive model is that enrollees who do and do not get help making health insurance decisions make choices that imply virtually identical marginal rates of substitution between cost, variance, and quality. The main difference between the two groups is that those who get help exhibit less inertia, as shown in Table 4.

main effects have the expected signs and are precisely estimated, with the exception of variance. Its insignificant coefficient mirrors the finding from Abaluck and Gruber (2011) and Ketcham, Kuminoff and Powers (2016) that if we ignore heterogeneity in decision making, then the representative enrollee appears to ignore risk protection.

TABLE 4—LOGIT MODELS OF PRESCRIPTION DRUG PLAN CHOICE

| | All Choices | | Non-Suspect choices | | Suspect choices | |
|---|---|---|---|---|---|---|
| expected cost | -0.283 | [0.017]*** | -0.377 | [0.029]*** | -0.197 | [0.021]*** |
| variance | 0.076 | [0.085] | -0.433 | [0.118]*** | 0.621 | [0.126]*** |
| quality (CMS index) | 0.035 | [0.078] | 0.056 | [0.104] | -0.012 | [0.124] |
| within-brand switch | -3.307 | [0.109]*** | -3.239 | [0.152]*** | -3.396 | [0.155]*** |
| between-brand switch | -5.181 | [0.095]*** | -4.923 | [0.128]*** | -5.591 | [0.141]*** |
| cost x 1{ bottom tercile of claims } | -0.172 | [0.034]*** | -0.194 | [0.039]*** | -0.089 | [0.053]* |
| cost x 1{ top tercile of claims } | 0.082 | [0.021]*** | 0.128 | [0.035]*** | 0.027 | [0.024] |
| cost x 1{ sought CMS info } | -0.043 | [0.022]* | -0.074 | [0.032]** | 0.037 | [0.030] |
| quality x 1{ income > $25k } | 0.170 | [0.091]* | 0.202 | [0.118]* | 0.095 | [0.147] |
| quality x 1{ sought CMS info } | 0.283 | [0.096]*** | 0.241 | [0.122]** | 0.326 | [0.165]** |
| switch within brand x standardized age | -0.162 | [0.069]** | -0.138 | [0.093] | -0.179 | [0.103]* |
| switch within brand x 1{ income > $25k } | -0.383 | [0.126]*** | -0.364 | [0.169]** | -0.373 | [0.183]** |
| switch within brand x 1{ help } | 0.335 | [0.122]*** | 0.271 | [0.170] | 0.474 | [0.181]*** |
| switch within brand x 1{ sought CMS info } | 0.126 | [0.131] | 0.262 | [0.167] | -0.200 | [0.208] |
| switch within brand x 1{ nonwhite } | -0.812 | [0.297]*** | -1.211 | [0.450]*** | -0.587 | [0.396] |
| switch brand x standardized age | -0.122 | [0.055]** | -0.167 | [0.073]** | 0.025 | [0.081] |
| switch brand x 1{ income > $25k } | -0.390 | [0.106]*** | -0.411 | [0.139]*** | -0.429 | [0.163]*** |
| switch brand x 1{ help } | 0.263 | [0.105]** | 0.233 | [0.141]* | 0.383 | [0.160]** |
| switch brand x 1{ sought CMS info } | 0.285 | [0.102]*** | 0.178 | [0.133] | 0.263 | [0.165] |
| switch brand x 1{ nonwhite } | -0.794 | [0.239]*** | -1.371 | [0.348]*** | -0.107 | [0.341] |
| pseudo $R^2$ | 0.66 | | 0.64 | | 0.71 | |
| number of enrollment decisions | 9,119 | | 5,248 | | 3,871 | |
| number of enrollees | 3,442 | | 2,175 | | 1,560 | |

Note: The table summarizes logit models estimated from data on all choices; non-suspect choices only; and suspect choices only. All models include indicators for insurers. Excluded demographic interactions define the reference person as white and 78 years old with no college degree and annual income below $25,000. This person is in the middle tercile of the distribution of total drug claims, did not get help making an enrollment decision, and did not use the internet or 1-800-Medicare to search for information. Robust standard errors are clustered by enrollee. *,**, and *** indicate that the p-value is less than 0.1, 0.05, and 0.01 respectively.

The last two columns repeat the estimation for non-suspect and suspect choices separately. Comparing main effects across the three columns reveals that the insignificant coefficient on variance in the pooled model is driven by aggregating over heterogeneous decision making processes for the suspect and non-suspect groups. Taken literally, the coefficient on variance for the suspect group implies they are risk loving. In contrast, the non-suspect group is risk averse at levels consistent with findings from prior studies (Cohen and Einav 2007, Handel 2013, Handel and Kolstad 2015). For example, our results imply that enrollees in the non-suspect group would be indifferent between a 50-50 bet of winning \$1,000 and losing between \$854.7 and \$937.3.[30] Further, the non-suspect group is more sensitive to price with the implication that the monetary value of inertia—defined by dividing the switching indicators by the expected cost coefficient—is nearly three times larger for the suspect group.

Focusing on non-suspect choices in column 2, the interaction coefficients are consistent with intuition. Interactions between cost and indicators for whether the beneficiary is in the top or bottom terciles of the claims distribution imply that the marginal utility of income declines as people become sicker. People who have previously taken the time to search for information about Medicare programs on the internet or by calling 1-800-Medicare tend to be more sensitive to price and to have stronger preferences for CMS's "star rating" index of overall plan quality which is based, in part, on customer satisfaction. Preferences for plan quality are also higher among higher income enrollees. One explanation is that the opportunity cost of time is increasing in income and that choosing a higher quality plan reduces the time and effort required to interact with the insurer.

Inertia tends to be lower for people who get help choosing a plan and who searched for information about CMS programs, whereas it tends to be higher for people who are older, nonwhite and who have higher incomes, though some of these effects are imprecisely estimated. The income effect could again be due to heterogeneity in the opportunity cost of time. The directions of these effects are mostly consistent across the suspect and non-suspect groups, but the monetary implications are larger for the suspect group. The average non-suspect enrollee would have to be paid \$846 to hold their utility constant if they were

[30] These calculations are based on the fact that our specification for utility provides a 1st order approximation to a CARA model. Our calculations are additional discussion are provided in Table A6 and associated discussion in the supplemental appendix.

randomly reassigned to a different plan offered by the same insurer or $1,292 if they were reassigned to a plan offered by a different insurer. Comparable figures for the suspect group are $1,888 and $2,958. The fact that we see greater inertia for between-insurer switches compared to within-insurer switches is consistent with the inertia parameters reflecting latent preferences and hassle costs. Between-insurer switches are likely to require more time and effort than within-insurer switches as different plans offered by the same insurer tend to have the same formularies, pharmacy networks, customer service, and so on. In contrast, insurers typically differ along these dimensions, so that switching insurers may require new prior authorization requests, transferring prescriptions to new pharmacies, and becoming familiar with new formulary and customer service systems. Psychological biases might also be greater for between-brand switches.

### *B. Validation Tests*

A potential concern with our approach to modeling heterogeneity in consumer decision making is that it could be overfitting the data and consequently yielding less accurate predictions for how consumers will respond to prospective policies. We assess the model's predictive power by using validation tests similar to Keane and Wolpin (2007) and Galiani, Murphy, and Pantano (2015). The idea is to compare the out of sample predictions from our model with the standard pooled model that assumes a homogeneous decision process. Our validation test is powered by the largest year-to-year change in the PDP choice set that occurred during our study period. Between 2008 and 2009, the number of plans fell by 10%. We use data from 2008 to estimate the standard and refined models and then use each set of estimates to predict how consumers would adapt to their new choice sets in 2009.[31] Table A7 shows that among suspect choosers the refined model more accurately predicts the share that chose dominated plans; the share that chose the least expensive plans offered by their insurers; mean expenditures; the average amount that consumers who chose dominated plans could save by switching; and the share who chose to switch plans and the share who chose plans with gap coverage. The refined model likewise outperforms the pooled

[31] We exclude indicators for insurance brand because some new insurers joined the market in 2009 so we are unable to estimate indicators for them in 2008.

model in making out-of-sample predictions for the choices of non-suspect choosers for all but two of these measures. Overall, this exercise suggests that distinguishing between suspect and non-suspect choice processes improves the model's predictive power out of sample.

As an indirect test of our maintained assumption that people in the suspect and non-suspect groups share the same underlying utility parameters, conditional on demographics and prescription drug use, we leverage the panel structure of our data to repeat the estimation for four mutually exclusive sets of enrollment decisions: (1) choices made by enrollees who always make suspect choices (n=3,311); (2) suspect choices made by enrollees who sometimes make non-suspect choices (n=560); (3) non-suspect choices made by enrollees who sometimes make suspect choices (n=634); and (4) choices made by enrollees who always make non-suspect choices (n=4,616). The results, shown in Tables A8-A9, reveal that the estimated marginal rates of substitution between cost, variance, and quality are similar between groups 1 and 2, and between groups 3 and 4, despite some reduction in statistical precision. In other words, when people who switch between the suspect and non-suspect groups make non-suspect choices they behave in similar ways to the people who always make non-suspect choices. This supports the assumptions underlying our approach of using non-suspect preference parameters to predict welfare effects for people in the suspect group.

## VII. Evaluating Prospective Choice Architecture Policies

### *A. Bounding the Estimated Outcomes using Signals of Consumers' Information*

Section III explained our approach to bounding the welfare effect of inertia and the policy's effect on consumer behavior. We use these bounds to report results for two extreme cases. At one extreme is the case where the policy is "most effective" as a nudge in the sense that it causes the suspect group to start behaving like the non-suspect group <u>*and*</u> the inertia parameters estimated for the non-suspect group reflect psychological bias and hence have no direct effect on welfare, i.e. using equation 7' and 9' with $V_{ik}^{n1}$ and $\psi_{ik}^{n1}$. At the other extreme is the case where the policy is "least effective" as a nudge in that it does not change the suspect group's behavior *and* the inertia parameters for the non-suspect group reflect the hassle cost of switching plans and/or preferences for latent plan attributes and hence are welfare relevant (i.e. using equations 7 and 9).[32] To provide statistical bounds on our estimates, we report the 2.5$^{th}$ percentile from a 100 replication bootstrap for the least effective scenario and the 97.5$^{th}$ percentile for the most effective scenario.

### *B. The Distributional Effects of a Particular Menu Restriction*

In early 2014, CMS proposed a series of changes to Medicare Part D that included a provision to limit each parent organization to offering no more than one basic and one enhanced plan per region (Department of Health and Human Services 2014).[33,34] This would have forced some current enrollees to switch plans. While the proposal was controversial and has yet to be implemented, it provides an opportunity to investigate the effects of a realistic menu restriction.

CMS must approve each PDP that an insurer offers, but the proposed regulation did not specify how, exactly, CMS would determine which plans to retain. Therefore, we start by assuming that CMS would require each sponsor to continue to offer their most popular plans; i.e. the single basic plan and the single enhanced plan with the highest enrollments.[35] Then we consider alternative rules as robustness checks. The menu restriction reduces the number of plans on the average enrollee's menu from 47 to 31.

The menu restriction affects consumer welfare in several ways. First, people will be made worse off if their utility maximizing plans are eliminated. Second, individuals who switch plans may incur utility costs of switching. Third, individuals in the suspect group may be made better off if the policy forces them to switch out of a dominated plan or if contracting their choice sets reduces their inertia and nudges them to switch to plans that

[32] Alternatively, one could solve jointly for a continuous fraction of inertia that is welfare relevant and a continuous fraction of suspect group consumers who start behaving like their analogs in the non-suspect group in order to minimize and maximize particular moments of the distribution of welfare effects.

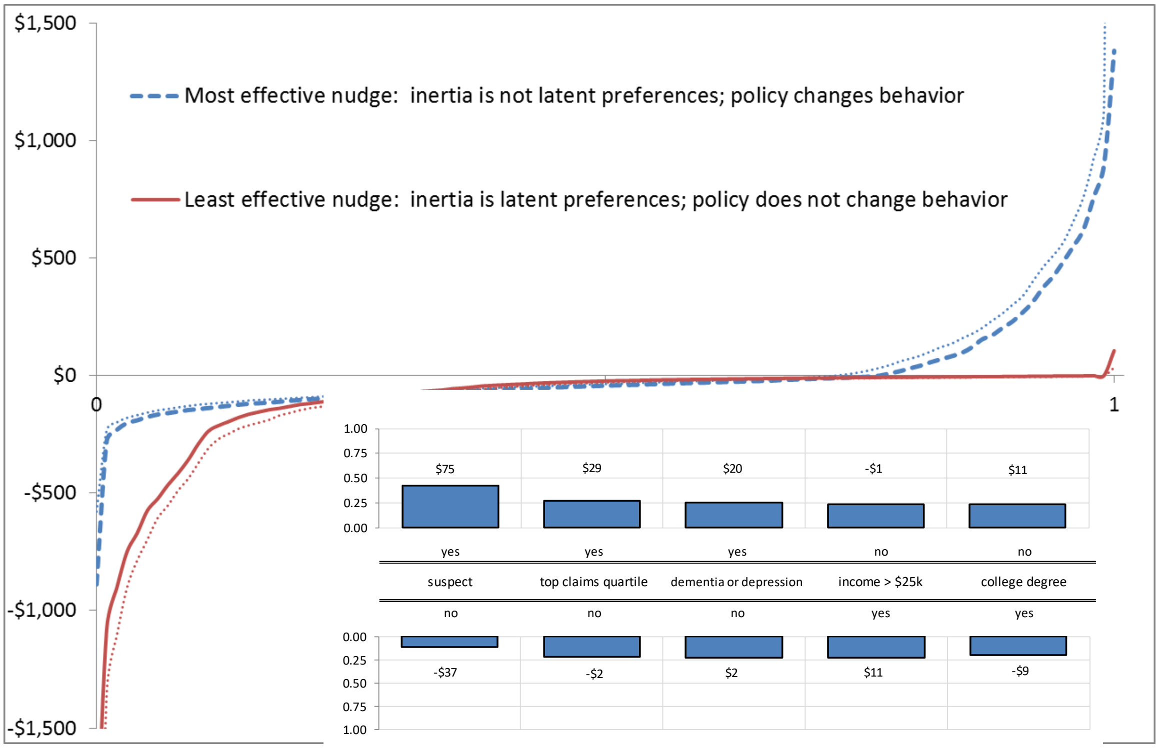
[33] "Parent organizations" or "sponsors" are entities that contract with CMS to sell PDPs. They may include multiple brand names. Basic plans may differ in design but must be deemed actuarially equivalent to the standard benefits package for some representative enrollee(s). Enhanced plans offer supplemental benefits.

[34] The proposal included the rationale to "…ensure that beneficiaries can choose from a less confusing number of plans that represent the best value each sponsor can offer" (Federal Register 2014).

[35] This is consistent with our interpretation of CMS' impact analysis (Federal Register 2014).

are cheaper, higher quality, and provide better insurance against health shocks. The magnitude of each of these gains or losses depends on which plans are eliminated and the relative benefits of switching.

FIGURE 1: DISTRIBUTION OF THE WELFARE EFFECTS FROM A MENU RESTRICTION




Note: The figure shows CDFs of the expected change in welfare from limiting each insurer to selling one basic plan and one enhanced plan, assuming that CMS requires insurers to keep the plans with the highest current enrollment. The small dotted lines represent the nonparametric 95% upper bound on the most effective nudge and the 95% lower bound on the least effective nudge based on a 100 replication bootstrap. The bar graphs show the fractions of consumers with welfare gains by demographic group and the numbers above or below each bar report average consumer surplus within the groups.

To summarize results we start by focusing on the case in which CMS requires each insurer to retain their basic and enhanced plans with the highest numbers of enrollees. Figure 1 summarizes the distributional effects on the beneficiary population. It shows CDFs of the expected consumer surplus under the "most effective" and "least effective" scenarios for the efficacy of the policy in nudging consumers (henceforth ME and LE). The bar charts in the lower half of the figure summarize the average changes in expected consumer surplus and the shares of consumers with expected welfare gains under the ME scenario for several types of people who might be of interest to policymakers: (i) those making suspect choices, or not, (ii) those in the top quartile of the distribution of total drug claims, or not, (iii) those with dementia or depression, or not, (iv) those with income over $25,000, or not, and (v) those with a college degree, or not.[36] In both the ME and LE scenarios fewer than 25% of consumers are made better off by the menu restriction. Further, the median consumer in every one of the 10 demographic groups is made worse off. While those in the suspect group have larger average gains and a higher probability of gains than those in the non-suspect group (the bootstrap confidence intervals show these are significantly different at 1%) even the median consumer in the suspect group is expected to lose from menu restrictions.

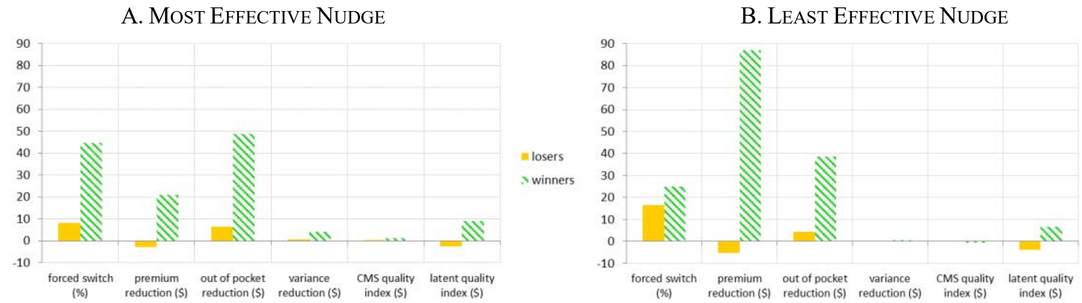
Figure 2 summarizes the mechanisms that drive welfare effects in the ME and LE scenarios. It reports the shares of winners and losers who are forced to switch because the policy eliminates their default plans, followed by the expected reductions in their premiums and OOP expenditures, the expected reductions in their expenditure variances, and the expected increases in plan quality (both the CMS quality index and the index of latent quality defined by the insurer dummy variables). Changes in variance and quality are converted to dollar equivalents using the non-suspect group's marginal utility of income.

In the ME scenario just under 25% of consumers are made better off.[37] Nearly half of those winners are forced to switch plans. Many of the people who are forced to switch, particularly those in the suspect group, are better off from switching because their new plans provide more generous coverage and there is no utility cost of switching in the ME scenario. Furthermore, by assumption, people in the suspect group now place more emphasis on cost and risk protection when selecting plans. As a result, the average winner pays $21 less in expected premiums and $41 less in expected out of pocket costs after the policy. Their expected risk exposure declines by an amount equivalent to a certain payment of $4 and they have an expected improvement in plan quality worth just over $10 (summing the effects of the CMS index and insurer dummies). Nevertheless, most people experience welfare losses because they become enrolled in plans with higher costs and they have more

[36] This comparison is less interesting in the LE scenario because in that scenario virtually all consumers have welfare losses.
[37] We code anyone with changes in welfare of |$0.01| or less as having no change. In tables 5, 6, and 7 the percentages of consumers with no change in welfare equal 100 minus the reported percentages with welfare gains and losses.

desirable plans eliminated. A small number of consumers, particularly those in the non-suspect group, experience relatively large losses because the policy eliminates their chosen plans, resulting in substantially higher expected premiums and lower expected quality.

FIGURE 2: MECHANISMS UNDERLYING THE WELFARE EFFECTS OF A MENU RESTRICTION




Note: The first column reports the shares of consumers with expected welfare gains (winners) and expected welfare losses (losers) who are forced to switch because their chosen plans are eliminated. The next two columns report expected reductions in premiums and out of pocket expenditures. The last three columns use the marginal utility of income for the non-suspect group to report the expected reduction in variance and expected increases in plan quality in monetary equivalents.

TABLE 5: EXPECTED OUTCOMES FROM ALTERNATIVE MENU RESTRICTION RULES

| | Max enrollment | | Max frontier | | Min expenditures | | Max profit | |
|---|---|---|---|---|---|---|---|---|
| | most effective | least effective | most effective | least effective | most effective | least effective | most effective | least effective |
| % enrollees with default plan eliminated | 16.7<br>(0.0) | 16.7<br>(0.0) | 24.7<br>(0.0) | 24.7<br>(0.0) | 19.4<br>(0.0) | 19.4<br>(0.0) | 37.0<br>(0.0) | 37.0<br>(0.0) |
| Δ expected welfare / enrollee ($) | 6.0<br>(9.0) | -106.8<br>(8.3) | 26.0<br>(9.2) | -162.5<br>(10.3) | 32.1<br>(9.0) | -125.4<br>(9.0) | 22.2<br>(8.9) | -218.8<br>(11.3) |
| % enrollees with expected welfare gain | 23.1<br>(1.6) | 1.2<br>(0.6) | 29.0<br>(1.6) | 1.2<br>(0.5) | 27.8<br>(1.6) | 1.4<br>(0.6) | 31.9<br>(1.2) | 0.6<br>(0.4) |
| % enrollees with expected welfare loss | 76.9<br>(1.6) | 98.8<br>(0.6) | 71.0<br>(1.6) | 98.8<br>(0.5) | 72.2<br>(1.6) | 98.6<br>(0.6) | 68.1<br>(1.2) | 99.3<br>(0.4) |

Note: The table shows the sensitivity of outcomes to the menu restriction rule. Max enrollment is the baseline that corresponds to figures 1 and 2. Max frontier retains the basic and enhanced plans with the highest shares of enrollees on the efficiency frontier. Min expenditure retains plans with the lowest average expenditures. Max profit allows insurers to retain the plans with the highest average profit per enrollee. Standard errors from a 100 replication bootstrap are in parentheses.

In the LE scenario, only 2% of consumers are made better off. For most people, the utility loss from being forced to switch plans more than offsets the cost savings, risk reduction, and improvements in plan quality experienced by switchers. The small fraction of winners is comprised entirely of individuals in the suspect group who have large reductions in expected premiums and expected OOP costs. Hence, if we think that inertia primarily reflects hassle costs and consumer preferences, then the menu restriction significantly harms the vast majority of consumers in exchange for small benefits for a small share of people in the suspect group who become less able to choose inferior plans.

The first two columns of Table 5 summarize the shares of people who have their default plans eliminated by the policy, the average changes in expected welfare per enrollee, the shares of winners and losers and the changes in insurer revenue per enrollee. The ME scenario predicts a net effect on consumer welfare that is statistically indistinguishable from zero, as large gains for a small fraction of consumers offset smaller losses for the majority. The LE scenario predicts a statistically significant mean welfare reduction of -$107, as 99 percent of consumers are made worse off. The last six columns show comparable results for three other hypothetical rules for how CMS could determine which plans to keep on the menu: the plans that are on the efficiency frontier for the greatest number of people; the plans with the minimum average cost to the enrollee; and the plans with the highest net revenue per enrollee.[38] Our results on consumer welfare are qualitatively robust across these scenarios. The most striking differences are the reductions in consumer welfare that occurs when insurers are allowed to retain their highest profit plans. Under the LE scenario, welfare is expected to fall by $219, amounting to 15.6% of enrollees' average spending.

### *C. Distributional Effects of Personalized Decision Support*

Our second policy experiment is a hypothetical decision support tool modeled on a randomized field experiment conducted by Kling, Mullainathan, Shafir, Vermeulen, and Wrobel (2012) [henceforth KMSVW]. Their study is motivated by the observation that while Medicare enrollees can learn about their PDP options and potential savings by calling 1-800-Medicare or using various online cost calculators, a minority of enrollees report doing so, as seen in Table A1. KMSVW attribute this to "comparison friction" which they define as the wedge between available information and consumers' use of it. KMSVW tested an intervention in which several hundred treatment group enrollees were sent a decision support letter containing personalized information about their potential personal cost savings from switching to their lowest cost available plan. The letter also identified the

[38] For profitability, we assume that there is sufficiently little variation in costs of plan operations and management per enrollee within the set of plans offered by each insurer that it does not affect the ranking of plans by revenue per enrollee.

name of the low cost insurer and contact information to initiate a switch. KMSVW estimate a 7 percentage point increase in the rate at which the treatment group switched to the plan identified in the letter relative to a control group that received a general letter with no personalized decision support, and an 11.5 percentage point increase in the overall switching rate for the treatment group.

We estimate the welfare effects of a prospective national rollout of the decision support tool in which the government mails letters to all existing enrollees that would be worded similarly to the one sent to KMSVW's treatment group. Because the information relies on prior drug claims, the policy would not affect new enrollees. Such a policy may affect welfare via several pathways. First, providing enrollees with personalized information may make them better off by mitigating psychological biases and/or reducing information costs. In our model, this would be realized as increases in the switch rate and cost savings. Because KMSVW's decision support tool does not embed information about risk protection and quality, however, the net effect on welfare is ambiguous. Second, an important feature of the information campaign—if it were implemented by the government—is that it would necessarily be based on incomplete information about enrollees' drug needs. While CMS has full information about existing enrollees' individual claims over their prior years in the PDP market, individuals may have private information about their own drug needs over the upcoming year. If enrollees with private information about changes in their drug needs choose to switch plans based on outdated information provided by CMS then these misinformed individuals could experience welfare losses.[39]

We cannot perfectly anticipate how much or which consumers will respond to information treatments, and such effects are likely to depend on specific aspects of the implementation of the policy. Given this limitation, we use the point estimates and confidence intervals of the information experiment in KMSVW to calibrate $V_{ij}^{n1}$ and $V_{ij}^{s1}$. Specifically, in the ME scenario we multiply the estimated inertia parameters by $\omega_1(1+\omega_2 1\{j=j^*\})$

[39] In principle such a phenomenon could occur if the free but imperfect information from CMS reduces individuals' efforts to acquire private information about their own future drug needs. Carlin, Gervais, and Manso (2013) explore these ideas more generally.

as shown in (15) and (16), where $1\{j = j^*\}$ is an indicator for whether plan $j$ is the individual's minimum cost plan that would be featured in the letter. We calibrate $\omega_1$ to generate a 7 percentage point increase in the rate at which the treatment group switches to their lowest cost plan relative to the baseline we observe in the data, and we calibrate $\omega_2$ to simultaneously generate an 11.5 percentage point increase in the overall switch rate subject to the constraints that $0 \leq \omega_1, \omega_2, \omega_1 + \omega_2 \leq 1$.

$$(15)\ V_{ijt}^{n1} = \hat{\alpha}_{it}^{n} c_{ijt} + \hat{\beta}_{it}^{n} \sigma_{ijt}^{2} + \hat{\gamma}_{it}^{n} q_{jt} + \omega_1 (1 + \omega_2 1\{j = j^*\}) \big(\hat{\eta}_{it}^{n} \Delta B_{ijt} + \hat{\delta}_{it}^{n} \Delta P_{ijt}\big).$$

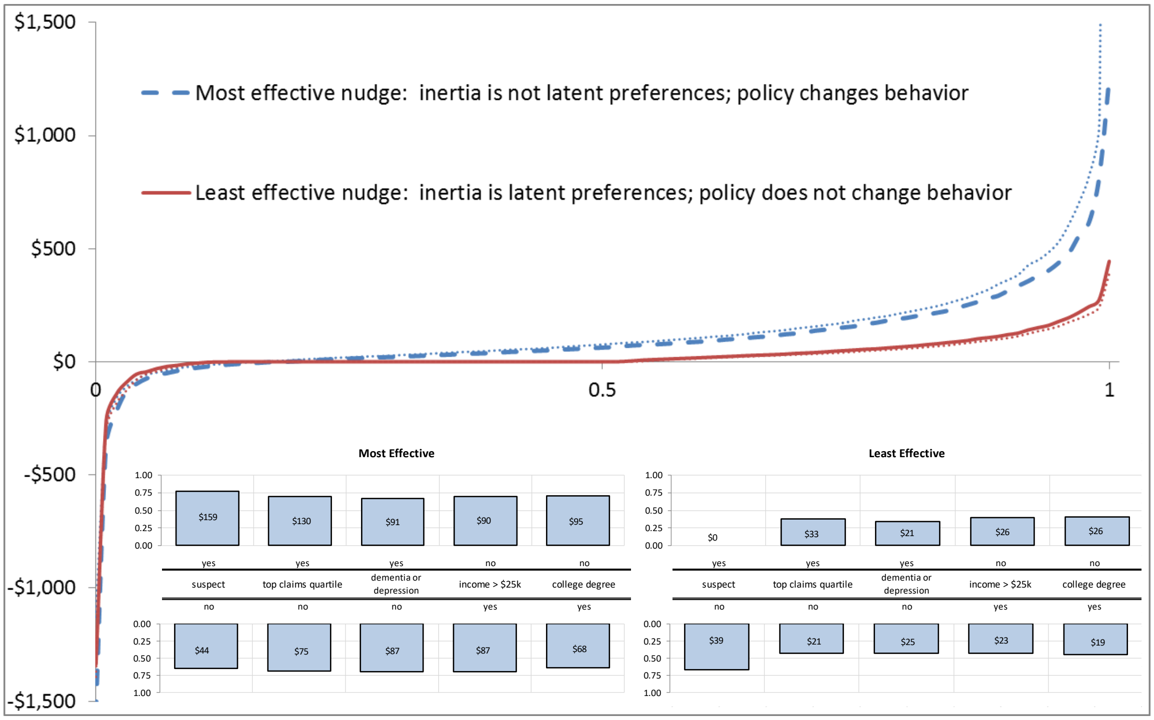
$$(16)\ V_{ijt}^{s1} = \hat{\alpha}_{it}^{s} c_{ijt} + \hat{\beta}_{it}^{s} \sigma_{ijt}^{2} + \hat{\gamma}_{it}^{s} q_{jt} + \omega_1 (1 + \omega_2 1\{j = j^*\}) \big(\hat{\eta}_{it}^{s} \Delta B_{ijt} + \hat{\delta}_{it}^{s} \Delta P_{ijt}\big).$$

$$(17)\ V_{ijt}^{s1} = \hat{\alpha}_{it}^{s} c_{ijt} + \hat{\beta}_{it}^{s} \sigma_{ijt}^{2} + \hat{\gamma}_{it}^{s} q_{jt} + \hat{\eta}_{it}^{s} \Delta B_{ijt} + \hat{\delta}_{it}^{s} \Delta P_{ijt}.$$

In the LE scenario, there is assumed to be no change in the behavior of the suspect group so we use (15) and (17), in which case $\omega_1$ and $\omega_2$ will have to be larger than in the ME scenario in order to induce sufficient switching among the non-suspect group to replicate the treatment effects estimated by KSMVW.

Figure 3 summarizes the distributional effects of the decision support tool using KMSVW's point estimates. In the ME scenario 81 percent of consumers are made better off by the policy. As shown in the bar charts in Figure 3, those who made suspect choices under the status quo policy are more likely to win and experience larger gains than those who did not (significant at 1%) and those with the highest number of drug claims are expected to have larger average gains than those with fewer claims (significant at 1%), but we do not find any other notable differences across demographic groups. In the LE scenario, the share of consumers with welfare gains declines to 48 percent because the suspect group is assumed to ignore the information treatment. Thus, they are unaffected by the policy.

FIGURE 3: DISTRIBUTION OF WELFARE EFFECTS FROM PERSONALIZED DECISION SUPPORT




Note: The figure shows CDFs of the expected change in welfare from a personalized decision support tool that is based on the field experiments of Kling et al. (2012). The model is calibrated to reproduce their estimated treatment effects on the rates at which people switch plans. The small dotted lines represent the nonparametric 95% upper bound on the most effective nudge and the 95% lower bound on the least effective nudge based on a 100 replication bootstrap. The bar charts show the fractions of consumers with welfare gains by demographic group and the numbers above or below each bar report average consumer surplus within the groups.

To reveal the mechanisms underlying the welfare losses, Table A10 shows that under both scenarios, those with welfare losses had much larger changes in actual OOP drug spending between 2009 and 2010. This is because the low cost plan featured in the information treatment is the one that minimizes their expenditures based on their 2009 drug claims. Some of the people who experience significant health shocks would have spent substantially more in their government recommended minimum cost plans than in the plans that they actually chose for themselves in 2010. These individuals are more likely to have made non-suspect choices. This illustrates the potential welfare losses that can arise from a nudge based on incomplete information. More broadly, this suggests a tradeoff between the potential benefits of simplifying the presentation of information and the potential costs of deemphasizing important details about the assumptions underlying that information.

This tradeoff may occur in many settings. For instance, in work subsequent to ours, Abaluck and Gruber (2016) describe a decision support tool that was intended to help Oregon school district employees choose health insurance plans but actually failed to correctly identify cost-minimizing plans for many employees because the tool relied on simplifying assumptions that failed to capture the complexity of plan benefit rules.

TABLE 6: SUMMARY OF OUTCOMES FROM THE PERSONALIZED DECISION SUPPORT TOOL AND SENSITIVITY TO DECISION MAKERS' EXPECTATIONS

| | Unbiased Expectations | | Myopia | |
|---|---|---|---|---|
| | most effective | least effective | most effective | least effective |
| Δ expected welfare / enrollee ($) | 102.9<br>(9.2) | 28.1<br>(3.7) | 158.2<br>(12.2) | 62.4<br>(3.1) |
| % enrollees with expected welfare gain | 81.1<br>(1.0) | 48.4<br>(1.0) | 91.8<br>(0.8) | 54.4<br>(1.1) |
| % enrollees with expected welfare loss | 18.8<br>(1.0) | 12.4<br>(0.7) | 2.1<br>(0.7) | 0.0<br>(0.0) |
| % enrollees switching to the advertised plan | 8.0<br>(0.1) | 8.0<br>(0.1) | 8.0<br>(0.1) | 8.0<br>(0.1) |

Note: The table shows the sensitivity of outcomes to the assumed form of decision makers' expectations for their own drug needs in the upcoming year. The baseline scenario that corresponds to figures 3 and 4 (perfect foresight) assumes that decision makers accurately forecast changes in their drug needs. The myopia scenario assumes that decision makers expect their future drug needs to be identical to the prior year. Standard errors from a 100 replication bootstrap are in parentheses.

The first two columns of Table 6 provide summary statistics for the outcomes under the ME and LE scenarios while maintaining our model's assumption that consumers have unbiased expectations of their actual drug use in the upcoming year. The average welfare gains range from $28 to $103. The unbiased expectations assumption could cause us to understate the policy's benefits. If consumers are myopic in the sense that they expect their drug use to be the same as the prior year then the information treatment has less scope to reduce some consumers' welfare. The last two columns of Table 6 demonstrate this and show that when we repeat the estimation and simulation based on the assumption that consumers are myopic when they enroll in insurance plans, then between 54% and 92% of consumers benefit from the policy and the average change in welfare is an increase of between \$62 and \$158. This scenario replaces our primary suspect choice indicator (Table 2, row 3) with the alternative one based on ex ante costs (Table 2, row 4).

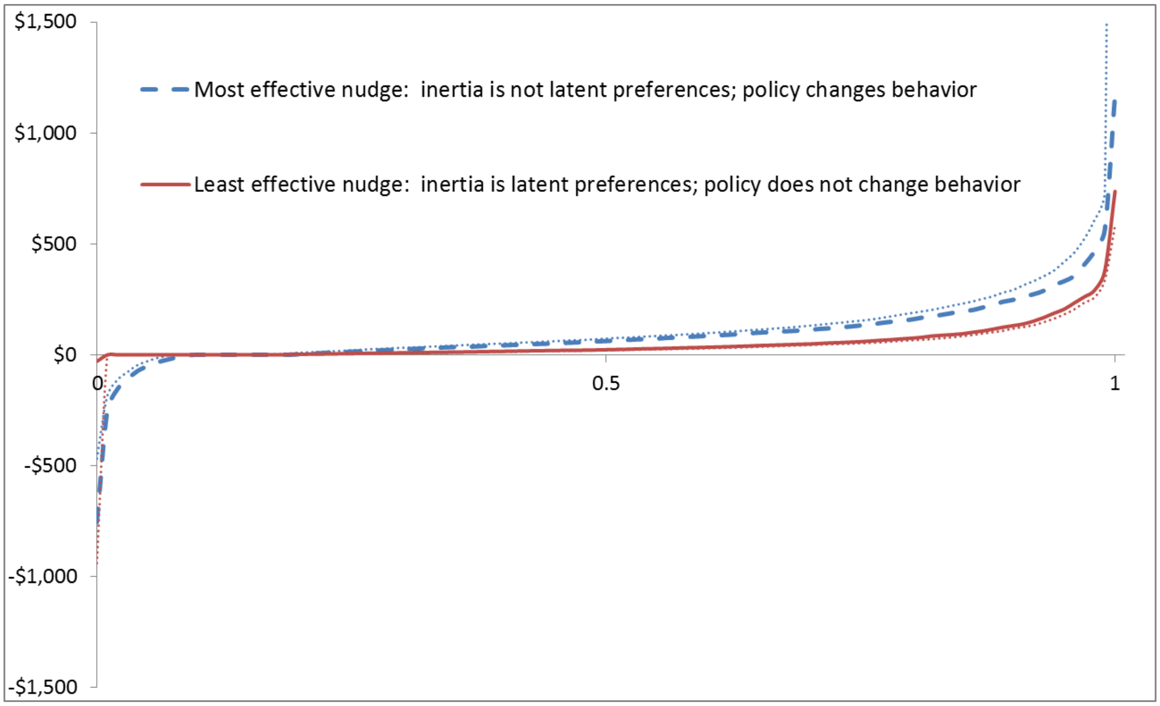
Finally, while KMSVW provide the only empirical evidence available to calibrate the amount of plan switching that would be triggered by the decision support tool, we need not focus exclusively on their point estimates. Appendix Table A11 shows that the results in Table 6 are qualitatively unchanged if we instead calibrate our model to upper or lower bounds on a 90% confidence interval on KMSVW's reported point estimates.

### *D. Distributional Effects of Default Assignment to a Low Cost Plan*

Our final policy experiment replaces CMS's current approach to defining each person's default plan for reenrollment as the plan they previously chose for themselves with a policy that would set the default plan to be the one that minimizes CMS's expectation for each enrollee's costs. We envision the policy being implemented as a stronger version of the decision support tool. Instead of informing enrollees of their minimum cost options, the enrollees would be automatically assigned to those options unless they chose to opt out by overriding the reassignment and choosing a different plan. As before, we assume that CMS would predict each enrollee's minimum cost plan using their drug claims from the prior year. Consistent with CMS's current approach, first-time enrollees would still be required to make active decisions.

In the ME scenario, the policy completely erases inertia for enrollment in the new low-cost default. Nevertheless, some consumers may still prefer their original plans if those plans provide greater quality or variance reduction. Assuming it is costless for enrollees to opt out and continue in their old plans, under ME assumptions the policy could reduce consumer welfare from (mis)assignment to plans requiring higher expenditures due to changes in drug needs or by reducing the probability of switching to higher cost plans that also provide higher utility due to superior risk protection and/or quality. Figure 4 shows that for a large share of consumers the net change is dominated by the aggregate effect of lower expenditures and the elimination of inertia. Overall, just over 80% of consumers have gains in expected welfare in both scenarios, and this is accompanied by reductions in insurer revenue of \$42 to \$128 as shown in Table 7.[40]

FIGURE 4: DISTRIBUTION OF WELFARE EFFECTS FROM ASSIGNMENT TO A DEFAULT PLAN




Note: The figure shows CDFs of the expected change in welfare from automatically assigning people to default plans, assuming it is costless to opt out. People are automatically assigned to the plan that would minimize their expenditures based on their prior year of drug use. The small dotted lines represent the nonparametric 95% upper bound on the most effective nudge and the 95% lower bound on the least effective nudge based on a 100 replication bootstrap.

In the LE scenario, being assigned to a default plan does not eliminate the hassle cost of learning to navigate a plan offered by a different insurer (e.g. prior authorization paperwork, new pharmacy networks, new customer service protocols). To account for this we recalibrate the model so that the policy reduces the cost of switching to the low-cost default from $\hat{\eta}_{it}\Delta B_{ijt} + \hat{\delta}_{it}\Delta P_{ijt}$ to $(\hat{\eta}_{it} - \hat{\delta}_{it})\Delta B_{ijt}$. Under this interpretation, the welfare-relevant hassle costs are the difference in the estimated cost of switching between brands relative to switching within brands. The continued presence of navigation costs reduces the share of enrollees choosing their assigned default to 14%.[41] The right half of Table 7 shows

[40] We do not find any differences in average gains or the probability of gain across observed consumer attributes, so we suppress the complementary bar chart for brevity.

[41] This approach may still overstate benefits to the extent that $\hat{\eta}$ and $\hat{\delta}$ represent latent preferences. As we increase the post-policy cost of switching to the new default option to $\hat{\eta}\Delta B_{ijt} + \hat{\delta}\Delta P_{ijt}$ the benefits to consumers approach zero. The extreme case in which $\hat{\eta}$ and $\hat{\delta}$ are entirely latent preferences is equivalent to reverting to the pre-policy equilibrium in which case the policy has no effect on consumer welfare.

that the share of consumers who benefit, their average welfare gain, and the implications for government spending and insurer revenue are virtually unchanged if we repeat the estimation and simulation under the assumption that consumers have myopic expectations of their own drug needs for the upcoming year.

TABLE 7: SUMMARY OF OUTCOMES FROM THE DEFAULT ASSIGNMENT RULE AND SENSITIVITY TO DECISION MAKERS' EXPECTATIONS

| | Unbiased Expectations | | Myopia | |
|---|---|---|---|---|
| | most effective | less effective | most effective | less effective |
| Δ expected welfare / enrollee ($) | 88.7<br>(9.2) | 50.4<br>(16.6) | 116.7<br>(13.2) | 66.1<br>(26.6) |
| % enrollees with expected welfare gain | 81.0<br>(1.1) | 82.8<br>(1.0) | 82.5<br>(1.1) | 83.2<br>(1.0) |
| % enrollees with expected welfare loss | 9.3<br>(1.0) | 0.5<br>(0.4) | 7.9<br>(0.9) | 0.0<br>(0.3) |
| % enrollees switching to the default plan | 40.0<br>(1.1) | 14.0<br>(0.8) | 44.0<br>(1.4) | 15.3<br>(0.9) |
| opt out cost needed to set average Δ in expected welfare to zero ($) | 134.7<br>(16.2) | 64.6<br>(27.3) | 197.9<br>(28.9) | 88.0<br>(46.9) |

Note: The table shows the sensitivity of outcomes to the assumed form of decision makers' expectations for their own drug needs in the upcoming year. The first four rows assume no opt out cost. See the text for additional details and definitions. Standard errors from a 100 replication bootstrap are in parentheses.

Table 7 also illustrates the importance of the design of the opt-out feature. People may incur some disutility from the time and effort required to pay attention to the new policy, learn how the opt-out feature works, determine whether they prefer their newly assigned default to their old plan and, if not, to exercise their opt out option. Under the assumption that everyone faces the same disutility parameter from opting out we solve for the mean opt-out cost needed to set the average change in expected welfare to zero. It ranges from a low of $65 in the LE scenario with unbiased expectations to a high of $198 in the ME scenario under myopia. When people incur such utility losses from opting out, some of them choose the newly assigned default even when it is welfare reducing relative to their prior plan in the absence of opt out costs.

### *E. Robustness of the Policy Experiment Results*

The validation section demonstrates that incorporating signals of consumers' knowledge to stratify the sample into suspect and non-suspect choices substantially improves the model's performance. To explore how this stratification affects predictions for the welfare effects of counterfactual policies, we repeat each policy experiment using a conventional "pooled" model in which we make no distinction between choice types, similar to Lucarelli, Prince and Simon (2012). We use the parameters shown in the first columns of Table 4 for the welfare calculations. For consistency, we continue to define the least effective scenario as the case where inertia represents welfare-relevant factors, and the most effective scenario as the case when inertia is irrelevant for welfare. Likewise, for the decision support tool, we calibrate the inertia discount factors in the pooled model to reproduce the point estimates for switching rates from KMSVW.

The first two rows of Table 8 contrast the pooled model's predictions for changes in consumer welfare with the predictions based our primary approach to defining suspect choices (repeated from Tables 5-7). Panel A reports the change in expected welfare per enrollee and Panel B reports the fraction of enrollees with expected welfare gains. The columns match the policy scenarios summarized in prior tables and figures. In four out of six scenarios, the pooled model understates the average welfare gain relative to the stratified model. The difference is especially large in the least effective scenario for the decision support tool. In this case, the conventional pooled model presumes that nobody can be made better off by being nudged to switch plans, whereas the stratified model recognizes that the decision support tool may be welfare improving for those in the suspect group who are induced to switch.

Differences in the models' average predictions mask even larger differences in their expected distributional implications. Table 9 reports the changes in expected welfare per enrollee for the average individual in the suspect and non-suspect groups using estimates from the pooled model (first column of Table 4) and stratified model (last two columns of Table 4). Averaging over both groups, conditional on model, yields the mean welfare changes reported in the first two rows of Table 8. The results show that the stratified model tends to predict larger welfare gains for the consumers that we assign to the suspect group and smaller gains for those we assign to the non-suspect group. For instance, the stratified model predicts \$6 increase in expected welfare in the most effective scenario for the menu restriction. This small average change obscures the expected \$75 gain for the mean suspect consumer and a \$37 loss for the mean non-suspect consumer. In contrast, the pooled model's predicted \$11 reduction in average welfare reflects mean losses of \$6 and \$14 for consumers in the suspect and non-suspect groups. These differences illustrate how generalizing the conventional model to allow for heterogeneity in the decision process may improve our ability to characterize distributional welfare effects of policies.

Next, to assess the importance of our choice among the available signals that could be used to identify suspect choices, Table 8 rows (3) through (6) repeat our analysis using alternative combinations of suspect choice indicators from Table 2. Row (3) modifies our primary approach (row 2) by replacing the ex post measure of costs with the ex ante measure. Row (4) uses a more inclusive definition of suspect choices based on the union of dominated plan choices, the knowledge question, and being able to reduce expenditures by more than 50%. Row (5) defines suspect choices based solely on dominated plans whereas row (6) defines suspect choices based solely on the MCBS knowledge question. These results collectively show that altering how suspect choices are defined has little effect on our main qualitative results. The reason is that when we classify a greater share of choices as suspect, the difference between $\theta^s$ and $\theta^n$ declines. More people benefit from certain simplifications to choice architecture, but the average gain among those who benefit is smaller. These effects offset each other in a way that leads to small increases in expected welfare in some scenarios and small decreases in expected welfare in others.

For our final set of robustness checks, we refine the sample in multiple ways and report results using our primary definition of suspect choices. In Row (7) we exclude 3,358 choices made by enrollees who first entered the market mid-year. A potential concern in that they may have been forward looking with respect to the following year's drug needs at the time they made their enrollment decisions, especially as they neared or entered the open enrollment period for the following year. Dropping them has little effect on our results. In Row (8) we drop 4,044 choices made by enrollees (44% of our sample) who had help choosing a plan or relied on a proxy to choose a plan for them. The logit estimates and subsequent policy implications are similar to the full sample. This suggests that while the research value of having access to better information on how family, friends, and advisors influence decision making is self-evident, in our context of Medicare Part D it does not alter the predicted effects of policy reforms. Finally, in Row (9) we include data from 2006, the inaugural year of the Medicare Part D program. Again, this only produces minimal changes in our estimates relative to our primary results in Row (2).

TABLE 8—ASSESSING THE ROBUSTNESS OF THE RESULTS

| | | Menu Restriction | | Decision Support | | Default Assignment | |
|---|---|---|---|---|---|---|---|
| | | most effective | least effective | most effective | least effective | most effective | least effective |
| | | *A. Change in Expected Welfare per Enrollee ($)* | | | | | |
| (1) | Pooled model | -11 | -108 | 125 | -71 | 66 | 56 |
| (2) | Primary approach to defining suspect choices | 6 | -107 | 103 | 28 | 89 | 50 |
| | Alternative suspect choice definitions | | | | | | |
| (3) | primary approach with ex ante drug costs | 10 | -118 | 158 | 62 | 117 | 66 |
| (4) | primary approach or potential savings > 50% | 26 | -91 | 92 | 22 | 89 | 48 |
| (5) | dominated plans only | -4 | -109 | 104 | 21 | 68 | 49 |
| (6) | knowledge test only | -15 | -133 | 126 | 36 | 81 | 55 |
| | Alternative samples | | | | | | |
| (7) | exclude mid-year enrollment decisions | -24 | -107 | 84 | 33 | 36 | 43 |
| (8) | exclude people who get help choosing plans | -2 | -97 | 100 | 30 | 70 | 44 |
| (9) | include choices for 2006 | 5 | -115 | 114 | 33 | 77 | 49 |
| | | *B. Enrollees with Expected Welfare Gain (%)* | | | | | |
| (1) | Pooled model | 14 | 0 | 84 | 0 | 75 | 84 |
| (2) | Primary approach to defining suspect choices | 23 | 1 | 81 | 48 | 81 | 83 |
| | Alternative suspect choice definitions | | | | | | |
| (3) | primary approach with ex ante drug costs | 24 | 1 | 92 | 54 | 82 | 83 |
| (4) | primary approach or potential savings > 50% | 30 | 3 | 78 | 43 | 82 | 83 |
| (5) | dominated plans only | 20 | 2 | 82 | 66 | 77 | 83 |
| (6) | knowledge test only | 19 | 0 | 84 | 58 | 79 | 83 |
| | Alternative samples | | | | | | |
| (7) | exclude mid-year enrollment decisions | 21 | 1 | 76 | 50 | 69 | 79 |
| (8) | exclude people who get help choosing plans | 23 | 2 | 81 | 50 | 79 | 80 |
| (9) | include choices for 2006 | 23 | 1 | 82 | 49 | 80 | 83 |

TABLE 9: PREDICTED CHANGES IN WELFARE AMONG SUSPECT AND NON-SUSPECT GROUPS FOR POOLED AND STRATIFIED MODELS OF DECISION-MAKING

| | Menu Restriction | | Decision Support | | Default Assignment | |
|---|---|---|---|---|---|---|
| | most effective | least effective | most effective | least effective | most effective | least effective |
| | *Δ expected welfare / enrollee ($)* | | | | | |
| Pooled model, suspect only | -6 | -118 | 137 | -77 | 66 | 58 |
| Baseline model, suspect only | 75 | -115 | 181 | 0 | 157 | 49 |
| Pooled model, non-suspect only | -14 | -102 | 118 | -68 | 66 | 55 |
| Baseline model, non-suspect only | -37 | -102 | 53 | 46 | 45 | 52 |

## VIII. Caveats and Opportunities for Future Research

Our analysis relies on three important maintained assumptions. First, our assessment of counterfactual choice outcomes embeds the assumption that the demand for prescription drugs is perfectly inelastic, ignoring moral hazard. Second, we have abstracted from systematic unobserved preference heterogeneity within the suspect and non-suspect groups. Third, we do not model supply-side adjustments to the set of plans and plan attributes, including premiums. These assumptions are common in the literature and serve as caveats to our policy conclusions and opportunities for further research.

### *A. Moral Hazard*

Several articles have estimated how changes in insurance generosity due to Part D altered peoples' subsequent drug consumption under varying assumptions about consumers' information (Einav, Finkelstein and Schrimpf 2015, Dalton, Gowrisankaran and Town 2018). Incorporating such moral hazard into models of consumers' PDP choices is complex. Heterogeneity in demand elasticities exist across people as well as across drugs (Einav, Finkelstein and Polyakova 2017) and even within a person across health states. Modeling such heterogeneity could potentially change the distributional effects of the policies that we analyze. Further complicating the implications for consumer surplus is the fact that taxpayer subsidies pay for a substantial share of the cost of higher drug consumption that occurs under more generous plans. This implies that, unlike Erickson and Starc

(2016), we cannot a priori sign the average bias in our prospective consumer surplus estimates that occurs due to our assumption of no moral hazard for prescription drugs.

Embedding moral hazard in drug consumption into a model of PDP choice is a potentially important direction for future research. We are unaware of any model of consumer choice among health insurance plans in general or Part D specifically that has incorporated moral hazard to allow the discrete bundle of medical care products consumed to vary within-person across plans in a way that would affect utility through expenditures and health.[42] Excluding such moral hazard has been a standard maintained assumption in research on consumer decision making in Part D (e.g. Abaluck and Gruber 2011, Ketcham, Kuminoff and Powers 2016, Ho, Hogan and Scott-Morton 2017) and in other health insurance markets (e.g. Erickson and Starc 2016). Handel and Kolstad (2015) perhaps come closest to relaxing this assumption. While they also excluded medical consumption from their empirical model of plan choice they performed an ex post analysis of how much moral hazard must exist to rationalize a person's plan choice.

The omission of within-person variation in drug consumption across plans is one specific example of a more general concern about the endogeneity of plan premiums due to unobserved quality differences. This is a common but typically unaddressed concern about empirical models of consumers' choices of insurance plans including prior work on Part D. While imperfect, our approach mitigates such concerns by its incorporation of heterogeneity by observed consumer attributes, the use of individual-specific drug claims, the inertia parameters and the brand dummies. Together these account for what might otherwise be omitted quality. The general concern in insurance markets is that the estimated parameters on premiums (included here in the total cost measure) would be biased upward (that is, toward zero or even positive) if higher premium plans have higher unobserved quality. In this case, our estimate of the average welfare changes under the prospective policies will be biased toward zero. The effects of such bias on the distribution of winners and losers, however, is uncertain a priori.

[42] Prior reduced form analysis of spending in Part D by two of the authors incorporated estimates of the average price elasticity of demand for prescription drugs (Ketcham, Lucarelli, Miravete, and Roebuck 2012, Ketcham, Lucarelli, and Powers 2015).

*B. Latent Preference Heterogeneity*

Although we utilize rich demographic data to characterize heterogeneity in preferences for PDP attributes, our model excludes unobserved heterogeneity aside from the Type I EV preference shocks. To investigate the scope for additional forms of latent preference heterogeneity to affect our results, we repeated estimation of the models in Table 4 after adding independent, normally distributed random coefficients for variance and plan quality. These coefficients allow for latent heterogeneity in the marginal rates of substitution between observable measures of plan cost, variance and quality. The results are reported in Appendix Table A12. For the suspect group, the standard deviations for the random coefficients are statistically indistinguishable from zero. We find evidence that the quality coefficient varies within the non-suspect group even after conditioning on their observable demographics, but its mean is nearly identical to our main specification, as are all of the other model parameters. Because the quality coefficient is relatively unimportant in explaining welfare effects of counterfactual policies (e.g. Figure 2) this heterogeneity has little scope to change our conclusions. Further, the random coefficients yield virtually no improvement in model fit. The decline in the log-likelihood function value from adding MCBS demographic variables is approximately 50 times larger than the subsequent decline from adding random coefficients for the non-suspect group. Our findings suggest that in the context of Part D, the incremental benefit of accounting for unobserved preference heterogeneity is low when our models already embed heterogeneity on a wide set of observed demographics.

In contrast with these low benefits of extending the model to allow for heterogeneity on unobserved preferences, the complexity from doing so is high. With such a model, welfare analysis for individuals in the suspect group would require us to define welfare-relevant reference points within the joint distribution of random parameters estimated for the non-suspect group. For instance, the welfare measures in (9) and (9') could be evaluated at the mean non-suspect parameter values, or at other moments of the distribution of non-suspect parameters, or by integrating over the distribution of non-suspect parameters. Keane et al. (2018) extend our framework to implement this approach, using a finite mixture of mixed logit models (with normal mixing) that allows for estimation of preference parameter vectors for a finite set of latent decision-making types. Such approaches are likely to be especially useful in settings where rich data on consumer demographics are not available.

### *C. Supply-Side Adjustments*

A full equilibrium approach to simulating counterfactual equilibria would allow for dynamic interactions between consumers' plan choices and insurers' decisions regarding entry and exit and plan design.[43] Others have modeled how Part D insurers may alter premiums, but not other plan attributes, in response to prospective changes in the subsidy structure (Decarolis, Polyakova and Ryan 2015), switching costs (Polyakova 2016), inertia (Fleitas 2017) and consumer inattention (Ho, Hogan and Scott Morton 2015).[44] None have modeled changes to choice architecture in Part D per se. Handel (2013) evaluated changes in choice architecture in health insurance offerings at a single firm. He concluded that forcing active choices by eliminating the ability to default to the same plan led to higher switching rates but worsened consumer welfare as greater adverse selection led to changes in premiums. In his approach, consumers are myopic regarding premium adjustments: consumers choose plans, then insurers adjust their premiums in response to the average cost of their enrollees, and welfare is calculated. In practice, however, the dynamics of premium setting are more complex: insurers forecast their costs based on their expected enrollees' health care utilization, they set premiums, and then consumers enroll in plans based on the posted premiums. The strong but imperfect link between premiums and enrollment decisions is difficult to model without knowing which types of information consumers and insurers incorporate into their choices for each plan year. Furthermore, even given any assumption about such information, the equilibrium outcome is unlikely to be unique, if it

[43] A related limitation is that we do not model people's decisions to participate in the PDP market. Choice architecture policies may influence which people enroll in PDPs versus choosing a Medicare Advantage plan, an employer-sponsored plan, or being uninsured. Such decisions may affect not only the individuals' welfare, but also have important average and distributional effects on other PDP enrollees as premiums and other plan attributes adjust, e.g. due to adverse selection.

[44] Polyakova (2016) found that eliminating switching costs would lower adverse selection and increase consumer surplus primarily through lower premiums. Fleitas (2017) underscored the welfare gains from reducing inertia in Part D given insurers' dynamic price setting by firms. Similarly Ho, Hogan and Scott Morton (2015) concluded that eliminating consumer inattention would lead to lower levels and lower growth of prescription drug insurance premiums.

exists (Stiglitz, Yun, and Kosenko 2018), and different outcomes will yield different welfare implications.

Due to these complexities, we do not attempt a full equilibrium analysis. Instead we leverage our data and our demand model to yield descriptive evidence about the precursors that plans respond to in their coverage and pricing decisions. To accomplish this, we calculate changes in insurer revenue per enrollee, holding premiums fixed, after we estimate each consumers' probabilities of choosing each plan under each prospective policy. This metric provides insights about the strength of insurers' incentives to respond to prospective government policies by adjusting premiums and other PDP attributes without having to assume a parametric form for the PDP production function or having to model how it arises from interactions between competing insurers, drug companies and the government. Our expectation is that policies that reduce insurers' revenues at the status quo premiums are likely to lead to premium increases, reducing consumers' surplus. Conversely, policies that increase insurers' revenues are likely to be eroded due to the competitive design of the CMS bidding process or otherwise targeted by regulators. In fact, prior work found that plan premiums are set near their marginal costs (Decarolis, Polyakova and Ryan 2015),

Equation (18) defines the change in insurer revenue per enrollee:

$$(18)\ \Delta\pi = \frac{1}{N}\sum_i \sum_{k\in K} \psi_{ik}^1\, \pi_{ik}^1 - \frac{1}{N}\sum_i \sum_{j\in J} \psi_{ij}^0\, \pi_{ij}^0,$$

where $\pi_{ij}^0$ and $\pi_{ik}^1$ measure insurer revenue per enrollee before and after the policy.[45] The change in revenue per enrollee is determined by whether the policy mitigates or exacerbates adverse selection based on predicted changes to choice probabilities (Handel 2013). As an additional metric, we report the expected change in the amount of within-plan variation across people's total drug costs under each prospective policy. This provides additional insights about the expected effects of each policy on adverse selection and subsequent pooling or separating equilibria.

[45] Empirically, we define insurer revenue per enrollee as the total premium (paid partly by enrollees and partly by the government) less residual drug expenditures, defined as total expenditures less the sum of consumers' OOP costs and government payments for consumers who exceed the threshold for catastrophic spending. We assume the average cost of plan management and operations per enrollee is unchanged by the policy so that it cancels out of the difference in (15).

TABLE 10—PREDICTED CHANGES IN INSURER REVENUES AND WITHIN-PLAN VARIATION IN DRUG EXPENDITURES

| | Menu Restriction | | Decision Support | | Default Assignment | |
|---|---|---|---|---|---|---|
| | most effective | least effective | most effective | least effective | most effective | least effective |
| Δ insurer revenue per enrollee ($) | -8 | 10 | -11 | 0 | -128 | -42 |
| Δ within-plan standard deviation of E[spend/enrollee] | | | | | | |
| mean over all plans | 45 | 56 | -43 | -7 | -104 | -24 |
| 10th percentile of plans | -15 | 13 | -110 | -88 | -304 | -73 |
| 25th percentile of plans | 4 | 15 | -75 | -54 | -197 | -44 |
| 50th percentile of plans | 20 | 34 | -47 | -14 | -122 | -24 |
| 75th percentile of plans | 60 | 67 | -21 | 0 | -72 | -6 |
| 90th percentile of plans | 178 | 177 | 21 | 15 | 84 | 2 |

Table 10 shows the results.[46] In the case of the menu restriction in which CMS retains the plans with the largest enrollment, more than three quarters of the remaining plans experience increased variation in drug expenditures. This is consistent with the hypothesis that eliminating a third of all plans would lead to increased risk pooling by reducing the scope for adverse selection. At the same time, the change in insurers' net revenue per enrollee is $10 or less. These small changes lead us to expect small changes in average plan premiums under this policy. Together these results suggest that even when supply-side responses are taken into account we should expect that the median consumer overall, and within each of the subgroups considered above, would experience welfare losses from the particular menu restriction we considered despite the improved risk pooling.

The metrics for decision support suggest a different outcome. As with a menu restriction, insurers' average net revenues even holding premiums constant remain nearly unchanged, so we expect small changes in average plan premiums. However, decision support promotes adverse selection, substantially reducing the amount of within-plan variation in drug costs across enrollees for a large majority of plans. The distributional implications of this are that we should expect that people with higher drug costs end up in plans with higher premiums under decision support while those with lower cost end up in plans with lower premiums. Because our partial equilibrium welfare calculations account for these changes,

[46] Appendix Table A14 shows that the signs and magnitudes of the changes in insurer revenue per enrollee summarized in Table 9 are robust to the range of alternative estimation samples and alternative suspect choice definitions summarized in Table 8.

we expect that the median consumer would benefit from decision support even after supply-side changes are taken into account.

The results for default assignment, holding premiums constant, show that it would substantially reduce insurers' net revenue and increase adverse selection, with more than three quarters of plans experiencing a reduction in the variance of enrollee expenditures, leading to a more substantial reduction in insurer revenue of between $42 and $128 per enrollee. Under the ME scenario, 40% to 44% of enrollees remain in their new default plans. These plans transfer enough of consumers' OOP costs to the insurance companies that expected revenue per enrollee declines by more than the increase in expected consumer welfare. Hence, the policy exacerbates adverse selection as in Handel (2013). Those making non-suspect choices would be more likely to lose from the policy once supply-side adjustments are taken into account. For those in the non-suspect group, the average reduction in insurers' net revenues is about twice as large as the enrollees' partial equilibrium gains under the ME scenarios. Hence we expect that premium adjustments could more than offset the other welfare gains. For those in the suspect group, we also observe that the reduction in insurers' net revenue amounts to about 40 to 80 percent of the magnitude of the gain in consumer welfare. As a result, adjustments to premiums or other plan attributes to prevent insurers' losses would partly offset the gains to those in the suspect group. Taken together, these results suggest that once we account for premium adjustments, the gains from default assignment are likely to be smaller than what is expected under our partial equilibrium results above, but qualitatively similar.

## IX. Summary

We developed a repeated choice multinomial logit model for analyzing the equity and efficiency of choice architecture policies in a differentiated product market where some consumers' choices may not reveal their preferences. Specifically we used administrative and survey data to first identify which consumers appear to make informed and informative decisions. We then estimated separate models of decision making for the informed and misinformed groups. Model validation showed that this approach improved model performance. We then used parameters from the former to assess the partial equilibrium distributions of the welfare effects of prospective policies for the latter. Finally, we reported bounds on welfare that are robust to extreme assumptions about the latent mechanisms underlying consumer inertia and the effects of counterfactual polices on consumer behavior. A comparison against a pooled logit model showed that our approach that incorporates signals of consumers' knowledge yields different expectations about the distributions of the welfare effects than standard approaches that exclude such signals.

The results from our policy experiments show that the US government's 2014 proposal to simplify the choice process in prescription drug insurance markets by reducing the number of drug plans would reduce welfare for the median consumer by up to 16% of consumer expenditures and potentially increase transfers to insurers. In contrast, our results suggest that providing personalized information about the potential savings from switching plans or assigning people to low-cost default plans would benefit the median consumer. Under the most optimistic scenario and holding plan premiums and other attributes constant, these gains are 11% of consumer expenditures. Comparing the decision support and default assignment policies suggests that defaults have higher downside risk for consumers due to opt-out costs and larger losses in insurer revenue. These factors have the potential to erode the consumer welfare gains observed in our partial equilibrium approach. More generally, because both of these policies emphasize cost minimization, insurers may respond by simultaneously lowering plans' costs, quality and risk protection in ways that have ambiguous effects on consumer welfare. Importantly, these qualitative findings persist across a range of approaches to identifying choices that we believe may not reveal consumers' preferences to us as analysts. However, the results are conditional on our assumptions of preference stability across consumers who make choices identified as suspect versus non-suspect, of no moral hazard in prescription drug consumption, of no responses of suppliers in terms of entry or exit or plan design, and of no changes in the composition of people participating in the PDP markets.

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## SUPPLEMENTAL APPENDIX: FOR ONLINE PUBLICATION

### *A. Supplemental Tables and Figures*

TABLE A1—SUMMARY STATISTICS FOR THE MCBS-ADMINISTRATIVE SAMPLE

| | Overall | 2006 | 2007 | 2008 | 2009 | 2010 |
|---|---|---|---|---|---|---|
| number of enrollees | 10,867 | 1,748 | 1,975 | 2,167 | 2,366 | 2,611 |
| <u>Medicare Current Beneficiary Survey</u> | | | | | | |
| high school graduate (%) | 79 | 77 | 77 | 78 | 80 | 80 |
| college graduate (%) | 22 | 21 | 21 | 22 | 23 | 25 |
| income>$25k (%) | 55 | 52 | 53 | 53 | 56 | 57 |
| currently working (%) | 13 | 14 | 12 | 13 | 12 | 13 |
| married (%) | 55 | 57 | 55 | 54 | 56 | 56 |
| has living children (%) | 93 | 93 | 93 | 93 | 93 | 93 |
| uses the internet (%) | 35 | 33 | 32 | 34 | 37 | 38 |
| searched for CMS info: internet (%) | 27 | 22 | 24 | 27 | 30 | 30 |
| searched for CMS info: 1-800-Medicare (%) | 17 | 29 | 23 | 17 | 12 | 8 |
| makes own health insurance decisions (%) | 62 | 63 | 62 | 63 | 63 | 62 |
| gets help making insurance decisions (%) | 27 | 27 | 26 | 26 | 26 | 28 |
| insurance decisions made by proxy (%) | 11 | 10 | 12 | 11 | 11 | 10 |
| <u>CMS Administrative Data</u> | | | | | | |
| mean age | 78 | 77 | 77 | 78 | 78 | 79 |
| female (%) | 63 | 62 | 63 | 63 | 63 | 63 |
| white (%) | 93 | 93 | 92 | 93 | 93 | 94 |
| dementia including Alzheimer's (%) | 9 | 6 | 8 | 9 | 11 | 12 |
| depression (%) | 10 | 8 | 9 | 10 | 11 | 11 |
| mean number of drug claims | 34 | 28 | 34 | 36 | 35 | 35 |
| mean number of available plans | 51 | 43 | 56 | 55 | 51 | 47 |
| mean number of available brands | 22 | 19 | 24 | 23 | 23 | 21 |
| has a default plan (%) | 65 | 0 | 80 | 83 | 83 | 77 |
| switches out of the default plan (%) | 11 | 0 | 11 | 16 | 15 | 13 |
| active enrollment decisions (%) | 46 | 100 | 31 | 33 | 32 | 36 |
| mean premium ($) | 407 | 330 | 355 | 398 | 459 | 493 |
| mean out-of-pocket costs ($) | 851 | 683 | 847 | 883 | 936 | 907 |
| mean potential savings, ex post ($) | 333 | 435 | 326 | 277 | 316 | 313 |

<u>Note</u>: The table reports means for key variables for the sample of Medicare Part D enrollees found in both the MCBS and cost calculator samples in the given year. See the text for details.

## TABLE A2— COMPARING MCBS SAMPLE MEANS WITH ADMINISTRATIVE DATA

| | 2006 | 2007 | 2008 | 2009 | 2010 |
|---|---|---|---|---|---|
| | Medicare beneficiary survey sample | | | | |
| age | 77 | 77 | 78 | 78 | 78 |
| % female | 62 | 62 | 62 | 62 | 62 |
| white (%) | 94 | 93 | 93 | 94 | 94 |
| Alzheimer's or dementia (%) | 7 | 8 | 9 | 10 | 11 |
| Depression (%) | 9 | 8 | 10 | 11 | 11 |
| number of available brands | 20 | 24 | 23 | 23 | 20 |
| number of available plans | 43 | 56 | 55 | 50 | 47 |
| premium ($) | 363 | 362 | 406 | 476 | 513 |
| out-of-pocket costs ($) | 1,010 | 842 | 873 | 920 | 903 |
| mean potential savings, ex post ($) | 546 | 347 | 295 | 332 | 337 |
| | Random 20% Sample of all Part D Enrollees | | | | |
| age | 76 | 76 | 76 | 76 | 76 |
| % female | 63 | 64 | 63 | 63 | 62 |
| white (%) | 93 | 92 | 92 | 92 | 93 |
| Alzheimer's or dementia (%) | 7 | 9 | 9 | 10 | 10 |
| Depression (%) | 9 | 9 | 10 | 10 | 11 |
| number of available brands | 19 | 24 | 22 | 23 | 20 |
| number of available plans | 43 | 56 | 55 | 50 | 47 |
| premium ($) | 362 | 369 | 415 | 487 | 516 |
| out-of-pocket costs ($) | 994 | 890 | 857 | 892 | 886 |
| mean potential savings, ex post ($) | 521 | 355 | 298 | 337 | 333 |

Note: The top half of the table reports means based on enrollees in the merged administrative-MCBS sample that we use for estimation. The bottom half of the table reports means based on a random 20% sample of all individuals who enrolled in Medicare Part D for the entire year. The two data sets differ in that our merged sample includes individuals who enrolled during the middle of the year. We drop these individuals before calculating sample means in order to ensure comparability between the two data sets.

## TABLE A3—KNOWLEDGE TEST AND DOMINATED PLAN RESULTS BY ACTIVE & PASSIVE CHOICES

| | Percent of enrollees | | | | | |
|---|---|---|---|---|---|---|
| | 2006 | 2007 | 2008 | 2009 | 2010 | 2007-2010 |
| Actively enrolling in a plan: | | | | | | |
| that is dominated | 19 | 6 | 6 | 4 | 5 | 5 |
| while not answering knowledge question correctly | 44 | 6 | 8 | 6 | 9 | 7 |
| Passively reenrolling in a plan that was: | | | | | | |
| dominated when actively chosen | | 12 | 12 | 12 | 10 | 11 |
| actively chosen while not answering knowledge question correctly | | 31 | 26 | 23 | 19 | 24 |
| Suspect choices (union of the first four rows) | 54 | 48 | 45 | 40 | 38 | 42 |

Note: The table reports the share of choices triggering each indicator, by year. The MCBS knowledge question asks whether the enrollee's out of pocket costs are the same under every available drug plan. The correct answer is coded as yes for enrollees who filed drug claims in both the prior and current years if their out of pocket costs did in fact vary across plans in both years. The last row reports the share of enrollees satisfying the criteria in either of the first two rows.

TABLE A4—ASSOCIATION BETWEEN MCBS KNOWLEDGE QUESTION AND MARKET OUTCOMES

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Pass Knowledge Test | | | Pass Knowledge Test | | | Pass Knowledge Test | | | Pass Knowledge Test | | |
| | yes | no | p-val: equal means | yes | no | p-val: equal means | yes | no | p-val: equal means | yes | no | p-val: equal means |
| Conditional on demographics | no | no | | yes | yes | | no | no | | yes | yes | |
| Active choices only | no | no | | no | no | | yes | yes | | yes | yes | |
| Number of plan choices | 7,560 | 3,307 | | 7,560 | 3,307 | | 3,330 | 1,433 | | 3,330 | 1,433 | |
| Percent choosing dominated plans | 18.5 | 18.3 | 0.598 | 16.5 | 17.7 | 0.000 | 16.3 | 18.9 | 0.016 | 16.7 | 18 | 0.000 |
| Mean potential savings ($) | 314 | 363 | 0.020 | 282 | 330 | 0.000 | 296 | 393 | 0.036 | 305 | 373 | 0.000 |

Table A4 reports the percentages of enrollees in dominated plans and their mean potential savings, conditional on the accuracy of answers to the MCBS knowledge question. The first six columns report results for all choices. Columns 1-3 show that potential savings is $49 higher for the average enrollee who answers the knowledge question incorrectly ($363 compared to $314) and that this difference is statistically significant at the 2% level. In contrast, there is virtually no difference in the probability of choosing a dominated plan. To isolate the association between knowledge and decision making separately from demographics, we repeat the comparison using residuals from regressions of the percent choosing dominated plans and mean potential savings on indicators for high school degree, college degree, income over $25,000, current working, married, living children, has used the internet to get information on Medicare programs, has used 1-800-Medicare to get information, gets help making health insurance decisions, the number of plans available, female, $70 \leq age \leq 74$, $75 \leq age \leq 79$, $80 \leq age \leq 84$, $85 \leq age$, has dementia, has depression, number of claims, year dummies and region dummies. Columns 4-6 show that after removing the variation in outcomes associated with a linear function of demographics, the percent choosing dominated plans is 1.2 percentage points higher for those answering the knowledge question incorrectly, potential savings is $48 higher, and both differences are statistically significant at the 0.1% level. Columns 7-12 show that the association between knowledge and decision making is stronger if we focus exclusively on active choices. Conditioning on demographics, the probability of actively choosing a dominated plan is 1.3 percentage points higher for the uninformed group and potential savings is $68 higher.

TABLE A5—LOGIT MODELS WITH ADDITIONAL DEMOGRAPHIC INTERACTIONS

| | All Choices | | Non-Suspect choices | | Suspect choices | |
|---|---|---|---|---|---|---|
| expected cost | -0.288 | [0.021]*** | -0.391 | [0.035]*** | -0.196 | [0.025]*** |
| variance | 0.066 | [0.176] | -0.389 | [0.274] | 0.445 | [0.172]*** |
| quality (CMS index) | 0.053 | [0.087] | 0.097 | [0.114] | -0.051 | [0.140] |
| within-brand switch | -3.306 | [0.108]*** | -3.246 | [0.151]*** | -3.397 | [0.154]*** |
| between-brand switch | -5.183 | [0.093]*** | -4.937 | [0.126]*** | -5.601 | [0.139]*** |
| cost x 1{ income > $25k } | 0.018 | [0.021] | 0.033 | [0.034] | 0.014 | [0.025] |
| cost x 1{ bottom tercile of claims } | -0.173 | [0.034]*** | -0.196 | [0.039]*** | -0.089 | [0.053]* |
| cost x 1{ top tercile of claims } | 0.084 | [0.021]*** | 0.130 | [0.035]*** | 0.030 | [0.024] |
| cost x 1{ help } | -0.012 | [0.022] | -0.011 | [0.036] | -0.024 | [0.026] |
| cost x 1{ sought CMS info } | -0.046 | [0.023]** | -0.078 | [0.033]** | 0.035 | [0.030] |
| variance x 1{ college graduate } | 0.001 | [0.186] | -0.135 | [0.236] | 0.928 | [0.295]*** |
| variance x standardized age | -0.004 | [0.086] | -0.046 | [0.113] | -0.032 | [0.118] |
| variance x 1{ female } | 0.146 | [0.174] | -0.111 | [0.232] | 0.519 | [0.233]** |
| variance x 1{ help } | 0.014 | [0.176] | 0.088 | [0.240] | -0.249 | [0.274] |
| variance x 1{ sought CMS info } | -0.226 | [0.178] | 0.068 | [0.241] | -0.604 | [0.262]** |
| quality x 1{ income > $25k } | 0.160 | [0.092]* | 0.181 | [0.120] | 0.097 | [0.148] |
| quality x 1{ help } | -0.034 | [0.094] | -0.102 | [0.122] | 0.108 | [0.152] |
| quality x 1{ sought CMS info } | 0.278 | [0.096]*** | 0.248 | [0.123]** | 0.302 | [0.164]* |
| switch within brand x standardized age | -0.162 | [0.069]** | -0.138 | [0.092] | -0.172 | [0.103]* |
| switch within brand x 1{ income > $25k } | -0.368 | [0.125]*** | -0.335 | [0.165]** | -0.363 | [0.183]** |
| switch within brand x 1{ help } | 0.321 | [0.122]*** | 0.257 | [0.169] | 0.462 | [0.181]** |
| switch within brand x 1{ sought CMS info } | 0.122 | [0.131] | 0.258 | [0.167] | -0.200 | [0.208] |
| switch within brand x 1{ nonwhite } | -0.811 | [0.297]*** | -1.214 | [0.450]*** | -0.578 | [0.397] |
| switch brand x standardized age | -0.121 | [0.055]** | -0.168 | [0.073]** | 0.029 | [0.081] |
| switch brand x 1{ income > $25k } | -0.368 | [0.103]*** | -0.368 | [0.137]*** | -0.409 | [0.160]** |
| switch brand x 1{ help } | 0.247 | [0.103]** | 0.222 | [0.139] | 0.360 | [0.157]** |
| switch brand x 1{ sought CMS info } | 0.280 | [0.102]*** | 0.170 | [0.134] | 0.270 | [0.164]* |
| switch brand x 1{ nonwhite } | -0.794 | [0.240]*** | -1.369 | [0.351]*** | -0.108 | [0.341] |
| pseudo $R^2$ | 0.66 | | 0.64 | | 0.71 | |
| number of enrollment decisions | 9,831 | | 5,465 | | 4,366 | |
| number of enrollees | 3,511 | | 2,166 | | 1,675 | |

Note: The table reports parameter estimates from logit models estimated from data on all choices; from non-suspect choices only; and from suspect choices only. All models include indicators for insurers. Robust standard errors are clustered by enrollee. *,**, and *** indicate that the p-value is less than 0.1, 0.05, and 0.01 respectively.

TABLE A6—RISK PREMIUMS FOR 50-50 BETS FOR NON-SUSPECT CHOICES

| Risk premium as a fraction of the bet | Size of Bet |
|---|---|
| 0.01 | 100 |
| 0.11 | 1,000 |
| 0.21 | 2,000 |
| 0.31 | 3,000 |
| 0.39 | 4,000 |
| 0.46 | 5,000 |
| 0.52 | 6,000 |
| 0.58 | 7,000 |
| 0.62 | 8,000 |
| 0.66 | 9,000 |
| 0.69 | 10,000 |

To assess the estimates from the logit model for non-suspect choices, we compare its implied risk premiums in a manner comparable with prior literature. Specifically, deriving the risk premium from the logit model as a 1st order approximation to a CARA model yields the following expression for the risk aversion coefficient:

$$\rho_{it} = \frac{-2\beta_{it}/1{,}000{,}000}{\alpha_{it}/100}, \text{ where } U_{ijt} = \alpha_{it}\acute{c}_{ijt} + \beta_{it}\acute{\sigma}_{ijt}^{2} + \gamma_{it}\acute{q}_{ijt} + \eta_{it}\Delta\acute{B}_{ijt} + \delta_{it}\Delta\acute{P}_{ijt} + \epsilon_{ij1}.$$

The estimates in Table 4 for the reference individual in the non-suspect group yields $\rho = .000217$. Table A6 translates this into a risk premium for various 50-50 bets. These results are broadly consistent with the range of prior results, e.g. as reported in Table 5 of Cohen and Einav (2007). Cohen and Einav find the mean_consumer would be indifferent between a 50-50 bet of winning $100 and losing $76.5, whereas the median consumer is virtually risk neutral. In contrast, our results imply the mean non-suspect consumer is indifferent between a 50-50 bet of winning $100 and losing $98.9 although Cohen and Einav argue that preferences likely differ between their automobile insurance context other contexts like drug insurance. In the health insurance context, Handel (2013) finds that the median individual is indifferent between a 50-50 bet of winning $100 and losing $94.6. In the model preferred by Handel and Kolstad (2015), the mean consumer is indifferent between a 50-50 bet of winning $1,000 and losing $913. This controls for friction and inertia. In comparison, our results imply indifference between winning $1,000 and losing $892.

TABLE A7—VALIDATION OF LOGIT MODELS STRATIFIED BY SUSPECT VS NON-SUSPECT AGAINST ANALOG POOLED MODEL

| | In-sample fit (2008) | | | | | | Out-of-sample fit (2009) | | | | | | | | Weighted absolute errors | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | suspect | | | non-suspect | | | suspect | | | | non-suspect | | | | in-sample | | out-of-sample | |
| | data | \|model error\| | | data | \|model error\| | | data | \|model error\| | | | data | \|model error\| | | | \|model error\| | | \|model error\| | |
| | | s=ns | s | | s=ns | ns | | s=ns | s | ns | | s=ns | s | ns | s=ns | s≠ns | s=ns | s≠ns |
| Percent of consumers choosing: | | | | | | | | | | | | | | | | | | |
| gap coverage | 14 | 1 | 0 | 10 | 2 | 2 | 15 | 4 | 3 | 5 | 7 | 1 | 2 | 0 | 2 | 1 | 2 | 1 |
| dominated plan | 33 | 9 | 8 | 14 | 8 | 7 | 37 | 9 | 8 | 10 | 24 | 1 | 2 | 0 | 9 | 8 | 5 | 4 |
| min cost plan within brand | 46 | 7 | 5 | 64 | 9 | 12 | 42 | 9 | 4 | 6 | 58 | 3 | 9 | 6 | 9 | 9 | 6 | 5 |
| Mean consumer expenditures ($) | | | | | | | | | | | | | | | | | | |
| premium + OOP | 1,385 | 14 | 0 | 1,266 | 12 | 0 | 1,578 | 29 | 13 | 41 | 1,374 | 17 | 35 | 4 | 14 | 0 | 23 | 9 |
| overspending on dominated plans | 49 | 17 | 14 | 28 | 13 | 14 | 54 | 26 | 23 | 29 | 17 | 7 | 5 | 7 | 16 | 15 | 16 | 15 |
| Percent of consumer switching plans | 15 | 4 | 0 | 23 | 3 | 0 | 13 | 6 | 2 | 10 | 22 | 4 | 8 | 1 | 4 | 0 | 5 | 2 |

Table A7 reports results from a logit model validation exercise. The purpose is to determine whether the models estimated separately by suspect and non-suspect choices outperform the pooled model, and whether the suspect model better predicts suspect choices than the non-suspect model does and vice versa. For this exercise the estimation sample is 2008 while the prediction sample is 2009. We chose these two years because they incorporate the largest year-to-year change in the choice set in our data—a central aspect to out-of-sample validation methods (Keane and Wolpin 2007). In particular, the number of plans available fell by 10%, although three new brands entered the market, precluding our use of brand indicators in the models. The results show that both in-sample and out-of-sample predictions are closer to the data along a number of policy-relevant outcomes when we base the predictions on separate models for the given type of choice. Blue shading is used to indicate the moments where our preferred model that distinguishes between suspect and non-suspect choices outperforms the pooled model. Red shading indicates moments where the pooled model performs better. We summarize the results in the main text.

TABLE A8—CHARACTERISTICS OF PEOPLE WHO ALWAYS, SOMETIMES, OR NEVER MAKE SUSPECT CHOICES

| | Always suspect | Sometimes suspect | Never suspect |
|---|---|---|---|
| number of enrollees | 3,311 | 1,194 | 4,616 |
| Medicare Current Beneficiary Survey | | | |
| high school graduate (%) | 77 | 79 | 80 |
| college graduate (%) | 18 | 23 | 26 |
| income>$25k (%) | 51 | 53 | 59 |
| currently working (%) | 12 | 9 | 14 |
| married (%) | 52 | 52 | 58 |
| has living children (%) | 92 | 93 | 94 |
| uses the internet (%) | 28 | 37 | 41 |
| searched for CMS info: internet (%) | 21 | 29 | 33 |
| searched for CMS info: 1-800-Medicare (%) | 11 | 18 | 16 |
| makes own health insurance decisions (%) | 60 | 61 | 65 |
| gets help making insurance decisions (%) | 27 | 29 | 26 |
| insurance decisions made by proxy (%) | 13 | 10 | 10 |
| CMS Administrative Data | | | |
| mean age | 79 | 78 | 77 |
| female (%) | 64 | 71 | 59 |
| white (%) | 91 | 96 | 94 |
| dementia including Alzheimer's (%) | 13 | 10 | 8 |
| depression (%) | 11 | 13 | 9 |
| mean number of drug claims | 37 | 39 | 32 |
| mean number of available plans | 52 | 53 | 52 |
| mean number of available brands | 23 | 23 | 23 |
| has a default plan (%) | 85 | 79 | 78 |
| switches out of the default plan (%) | 9 | 33 | 12 |
| active enrollment decisions (%) | 24 | 54 | 34 |
| mean premium ($) | 454 | 406 | 422 |
| mean out-of-pocket costs ($) | 946 | 1,032 | 825 |
| mean potential savings, ex post ($) | 339 | 325 | 282 |

TABLE A9—LOGIT ESTIMATES FOR PEOPLE WHO ALWAYS, SOMETIMES, OR NEVER MAKE SUSPECT CHOICES

| | Always suspect | | Sometimes suspect: suspect choice | | Sometimes suspect: non-suspect choice | | Never suspect | |
|---|---|---|---|---|---|---|---|---|
| expected cost | -0.218 | [0.024]*** | -0.103 | [0.041]** | -0.393 | [0.068]*** | -0.381 | [0.033]*** |
| variance | 0.491 | [0.116]*** | 1.125 | [0.344]*** | -1.100 | [0.296]*** | -0.338 | [0.136]** |
| quality (CMS index) | -0.280 | [0.138]** | 1.101 | [0.306]*** | -0.033 | [0.233] | 0.088 | [0.121] |
| within-brand switch | -3.623 | [0.194]*** | -2.673 | [0.284]*** | -2.051 | [0.357]*** | -3.475 | [0.173]*** |
| between-brand switch | -6.101 | [0.180]*** | -4.283 | [0.267]*** | -3.353 | [0.254]*** | -5.253 | [0.153]*** |
| cost x 1{ bottom tercile of claims } | -0.130 | [0.044]*** | -0.054 | [0.088] | -0.170 | [0.107] | -0.209 | [0.043]*** |
| cost x 1{ top tercile of claims } | 0.031 | [0.027] | -0.023 | [0.051] | 0.062 | [0.081] | 0.153 | [0.040]*** |
| cost x 1{ sought CMS info } | 0.015 | [0.028] | 0.030 | [0.054] | -0.075 | [0.065] | -0.064 | [0.037]* |
| quality x 1{ income > $25k } | 0.161 | [0.168] | -0.166 | [0.336] | -0.206 | [0.289] | 0.262 | [0.134]** |
| quality x 1{ sought CMS info } | 0.207 | [0.193] | 0.337 | [0.346] | 0.372 | [0.328] | 0.218 | [0.135] |
| switch within brand x standardized age | -0.070 | [0.123] | -0.413 | [0.185]** | 0.034 | [0.178] | -0.185 | [0.114] |
| switch within brand x 1{ income > $25k } | -0.519 | [0.225]** | 0.149 | [0.330] | -0.061 | [0.392] | -0.536 | [0.200]*** |
| switch within brand x 1{ help } | 0.538 | [0.216]** | 0.431 | [0.355] | 0.565 | [0.336]* | 0.159 | [0.204] |
| switch within brand x 1{ sought CMS info } | -0.453 | [0.268]* | -0.057 | [0.345] | 0.117 | [0.345] | 0.380 | [0.201]* |
| switch within brand x 1{ nonwhite } | -0.351 | [0.445] | -0.893 | [0.779] | 0.473 | [1.174] | -1.103 | [0.514]** |
| switch brand x standardized age | 0.092 | [0.104] | -0.133 | [0.140] | 0.206 | [0.129] | -0.325 | [0.086]*** |
| switch brand x 1{ income > $25k } | -0.244 | [0.210] | -0.664 | [0.279]** | -0.388 | [0.309] | -0.444 | [0.158]*** |
| switch brand x 1{ help } | 0.563 | [0.195]*** | 0.283 | [0.311] | 0.482 | [0.274]* | 0.167 | [0.166] |
| switch brand x 1{ sought CMS info } | 0.046 | [0.222] | 0.248 | [0.277] | -0.300 | [0.287] | 0.290 | [0.154]* |
| switch brand x 1{ nonwhite } | 0.177 | [0.370] | 0.106 | [0.681] | 0.419 | [1.377] | -1.291 | [0.376]*** |
| pseudo $R^2$ | 0.75 | | 0.54 | | 0.46 | | 0.68 | |
| number of enrollment decisions | 3,311 | | 560 | | 634 | | 4,614 | |

Note: The table reports parameter estimates from logit models estimated from data on all choices; from non-suspect choices only; and from suspect choices only. All models include indicators for insurers. Robust standard errors are clustered by enrollee. *,**, and *** indicate that the p-value is less than 0.1, 0.05, and 0.01 respectively.

TABLE A10—CHARACTERISTICS OF WINNERS AND LOSERS FROM THE DECISION SUPPORT TOOL

| | Most effective nudge | | Least effective nudge | |
|---|---|---|---|---|
| | enrollees with welfare gains | enrollees with welfare losses | Enrollees with welfare gains | Enrollees with welfare losses |
| % making suspect choices | 42 | 25 | 0 | 0 |
| $\mid oop_{2010} - oop_{2009} \mid$ | 356 | 600 | 324 | 648 |

Table A10 shows that enrollees with welfare losses are more likely to come from the non-suspect group and to have larger changes in OOP drug spending between the policy year and the prior year used to determine the minimum cost plan. The text accompanying Figure 3 provides additional details.

TABLE A11—SUMMARY OF OUTCOMES FROM THE PERSONALIZED DECISION SUPPORT TOOL UNDER ALTERNATIVE CALIBRATION TARGETS FOR SWITCHING RATES

| | Switch rate: baseline | | Switch rate: lower | | Switch rate: higher | |
|---|---|---|---|---|---|---|
| | most effective | least effective | most effective | least effective | most effective | least effective |
| Δ expected welfare / enrollee ($) | 102.9 | 28.1 | 40.7 | -0.1 | 155.0 | 65.5 |
| % enrollees with expected welfare gain | 81.1 | 48.4 | 63.5 | 43.3 | 86.5 | 50.0 |
| % enrollees with expected welfare loss | 18.8 | 12.4 | 36.3 | 17.5 | 13.4 | 10.9 |
| % enrollees switching to the advertised plan | 8.0 | 8.0 | 3.9 | 3.9 | 12.1 | 12.1 |
| Calibration targets | | | | | | |
| consumers switching to featured plan (%) | 7.0 | 7.0 | 2.9 | 2.9 | 11.1 | 11.1 |
| consumers switching, overall (%) | 11.5 | 11.5 | 4.8 | 4.8 | 18.2 | 18.2 |

Table A11 summarizes the sensitivity of predicted outcomes from the personalized decision support tool to different assumptions about the rate of plan switching that would be triggered by the policy. Each scenario reports results for the case of unbiased expectations. The first two columns match results reported in Table 6, in which the reductions in inertia parameters were calibrated to match the switching rates observed in a randomized field experiment conducted by Kling, Mullainathan, Shafir, Vermeulen, and Wrobel (2012) [henceforth KMSVW]. The remaining columns report comparable results from calibrating the model at higher and lower switching rates that correspond to bounds on a 90% confidence interval around KMSVW's estimate for the rate of overall plan switching. Because they do not report confidence intervals on the rate of switching to the featured plan we scale this parameter up or down in proportion to the overall switch rate.

TABLE A12—LOGIT MODELS WITH AND WITHOUT INTERACTIONS AND RANDOM PARAMETERS

| | Non-Suspect choices | | | | | |
|---|---|---|---|---|---|---|
| | multinomial logit | | multinomial logit | | mixed multinomial logit | |
| cost | -0.382 | [0.018]*** | -0.377 | [0.029]*** | -0.382 | [0.030]*** |
| cost x 1{ bottom tercile of claims } | | | -0.194 | [0.039]*** | -0.194 | [0.040]*** |
| cost x 1{ top tercile of claims } | | | 0.128 | [0.035]*** | 0.129 | [0.035]*** |
| cost x 1{ sought CMS info } | | | -0.074 | [0.032]** | -0.073 | [0.032]** |
| variance [mean] | -0.440 | [0.115]*** | -0.433 | [0.118]*** | -0.454 | [0.115]*** |
| variance [standard deviation] | | | | | 0.516 | [0.332] |
| CMS quality index [mean] | 0..267 | [0.078]*** | 0.056 | [0.104] | 0.054 | [0.104] |
| CMS quality index [standard deviation] | | | | | 0.665 | [0.168]*** |
| quality x 1{ income > $25k } | | | 0.202 | [0.118]* | 0.199 | [0.118]* |
| quality x 1{ sought CMS info } | | | 0.241 | [0.122]** | 0.239 | [0.122]* |
| switch within-brand | -3.299 | [0.080]*** | -3.239 | [0.152]*** | -3.253 | [0.153]*** |
| switch within brand x standardized age | | | -0.138 | [0.093] | -0.138 | [0.093] |
| switch within brand x 1{ income > $25k } | | | -0.364 | [0.169]** | -0.370 | [0.170]** |
| switch within brand x 1{ help } | | | 0.271 | [0.170] | 0.275 | [0.171] |
| switch within brand x 1{ sought CMS info } | | | 0.262 | [0.167] | 0.266 | [0.168] |
| switch within brand x 1{ nonwhite } | | | -1.211 | [0.450]*** | -1.221 | [0.452]*** |
| switch brand | -5.049 | [0.068]*** | -4.923 | [0.128]*** | -4.992 | [0.136]*** |
| switch brand x standardized age | | | -0.167 | [0.073]** | -0.170 | [0.074]** |
| switch brand x 1{ income > $25k } | | | -0.411 | [0.139]*** | -0.419 | [0.141]*** |
| switch brand x 1{ help } | | | 0.233 | [0.141]* | 0.243 | [0.143]* |
| switch brand x 1{ sought CMS info } | | | 0.178 | [0.133] | 0.182 | [0.135] |
| switch brand x 1{ nonwhite } | | | -1.371 | [0.348]*** | -1.387 | [0.353]*** |
| LLF value | -7506.43 | | -7371.92 | | -7369.03 | |
| number of enrollment decisions | 5,248 | | 5,248 | | 5,248 | |
| number of enrollees | 2,175 | | 2,175 | | 2,175 | |

The middle column shows our primary specification for non-suspect choices that uses observable demographics to characterize preference heterogeneity. The first column reports results from a model without interactions and the last column reports results from a mixed logit model that adds independent, normally distributed random parameters for variance and quality.

TABLE A12 CONTINUED—LOGIT MODELS WITH AND WITHOUT INTERACTIONS AND RANDOM PARAMETERS

| | Suspect choices | | | | | |
|---|---|---|---|---|---|---|
| | multinomial logit | | multinomial logit | | mixed multinomial logit | |
| cost | -0.184 | [0.013]*** | -0.197 | [0.021]*** | -0.197 | [0.021]*** |
| cost x 1{ bottom tercile of claims } | | | -0.089 | [0.053]* | -0.089 | [0.053]* |
| cost x 1{ top tercile of claims } | | | 0.027 | [0.024] | 0.027 | [0.024] |
| cost x 1{ sought CMS info } | | | 0.037 | [0.030] | 0.037 | [0.030] |
| variance [mean] | 0.615 | [0.137]*** | 0.621 | [0.126]*** | 0.659 | [0.194]*** |
| variance [standard deviation] | | | | | 0.201 | [0.354] |
| CMS quality index [mean] | 0.136 | [0.098] | -0.012 | [0.124] | -0.014 | [0.124] |
| CMS quality index [standard deviation] | | | | | -0.023 | [0.231] |
| quality x 1{ income > $25k } | | | 0.095 | [0.147] | 0.092 | [0.147] |
| quality x 1{ sought CMS info } | | | 0.326 | [0.165]** | 0.328 | [0.165]** |
| switch within-brand | -3.532 | [0.089]*** | -3.396 | [0.155]*** | -3.397 | [0.155]*** |
| switch within brand x standardized age | | | -0.179 | [0.103]* | -0.179 | [0.103]* |
| switch within brand x 1{ income > $25k } | | | -0.373 | [0.183]** | -0.372 | [0.183]** |
| switch within brand x 1{ help } | | | 0.474 | [0.181]*** | 0.473 | [0.181]*** |
| switch within brand x 1{ sought CMS info } | | | -0.200 | [0.208] | -0.201 | [0.208] |
| switch within brand x 1{ nonwhite } | | | -0.587 | [0.396] | -0.586 | [0.396] |
| switch brand | -5.586 | [0.080]*** | -5.591 | [0.141]*** | -5.593 | [0.141]*** |
| switch brand x standardized age | | | 0.025 | [0.081] | 0.023 | [0.081] |
| switch brand x 1{ income > $25k } | | | -0.429 | [0.163]*** | -0.425 | [0.164]*** |
| switch brand x 1{ help } | | | 0.383 | [0.160]** | 0.382 | [0.160]** |
| switch brand x 1{ sought CMS info } | | | 0.263 | [0.165] | 0.263 | [0.165] |
| switch brand x 1{ nonwhite } | | | -0.107 | [0.341] | -0.108 | [0.341] |
| LLF value | -4456.72 | | -4426.74 | | -4426.72 | |
| number of enrollment decisions | 3,871 | | 3,871 | | 3,871 | |
| number of enrollees | 1,560 | | 1,560 | | 1,560 | |

The middle column shows our primary specification for suspect choices that uses observable demographics to characterize preference heterogeneity. The first column reports results from a model without interactions and the last column reports results from a mixed logit model that adds independent, normally distributed random parameters for variance and quality.

TABLE A13—ROBUSTNESS OF PREDICTED CHANGES IN INSURER REVENUE PER ENROLLEE

| | | Menu Restriction | | Decision Support | | Default Assignment | |
|---|---|---|---|---|---|---|---|
| | | most effective | least effective | most effective | least effective | most effective | least effective |
| (1) | Pooled model | 11 | 11 | 0 | 0 | -111 | -44 |
| (2) | Primary approach to defining suspect choices | -8 | 10 | -11 | 0 | -128 | -42 |
| | Alternative suspect choice definitions | | | | | | |
| (3) | primary approach with ex ante drug costs | -7 | 9 | -34 | -17 | -130 | -42 |
| (4) | primary approach or potential savings > 50% | -23 | 9 | -20 | 2 | -144 | -40 |
| (5) | dominated plans only | -5 | 9 | -9 | 3 | -126 | -42 |
| (6) | knowledge test only | 9 | 11 | 0 | -2 | -111 | -43 |
| | Alternative samples | | | | | | |
| (7) | exclude mid-year enrollment decisions | -6 | 13 | -8 | 3 | -120 | -37 |
| (8) | exclude people who get help choosing plans | -7 | 11 | -12 | -2 | -125 | -41 |
| (9) | include choices for 2006 | -12 | 11 | -17 | 0 | -130 | -38 |

Table A13 reports the sensitivity of predicted changes in insurer revenue per enrollee to our estimation sample and the criteria used to define suspect choices. The ordering of rows matches our sensitivity analysis for demand side estimates reported in Table 8.

## *B. MCBS Knowledge Test Questions*

TABLE B1—MCBS KNOWLEDGE TEST QUESTIONS, YEARS ASKED AND COMMENTS

| **MCBS Knowledge Question** | **Years Asked, 2006-2010** | **Comments** |
|---|---|---|
| "Your OOP costs are the same in all Medicare prescription drug plans. True or False" | 2006-2010 | This is the question we use. |
| "Everyone with Medicare can choose to enroll in the voluntary Medicare Prescription drug coverage regardless of their income or health." | 2006-2010 | Our sample is only of enrollees, so this is unhelpful to distinguish between suspect and non-suspect choices among enrollees. |
| "Everyone in Medicare has at least two Medicare Prescription drug plans to choose from." | 2006-2007 | It is unclear why knowing about other people's situations is useful for assessing an individual's choice. |
| "Medicare prescription drug plans can change the costs of prescription drugs only once per year." | 2006-2007, 2009 | This is neutral to the choice among plans. |
| "If you join a Medicare prescription drug plan, you must go to pharmacies that are part of the plan." | 2006-2007 | This question has no clear correct answer, as people are free to fill prescriptions anywhere, and sometimes may be the same cost outside the plan. Further, plans use tiered networks where "part of the plan" is not a simple binary variable. |
| "Medicare prescription drug plans can change the list of prescription drugs that they cover at any time during the year." | 2006, 2007, 2010 | Unclear how this relates to evaluating plan choice. Further, while the answer is strictly true, the changes are highly regulated and restricted. |
| "Most people with Medicare must choose a Medicare prescription drug plan by May 15, 2006, or pay a penalty if they choose to join later." | 2006 | This is unhelpful in assessing the choices of those that are already enrolled, as in this study. |
| "If you have limited income and resources, you may get extra help to cover prescription drugs for little or no cost to you." | 2006-2010 | This is unhelpful in assessing the choices of those that are not eligible for these subsidies, as in this study. |

| | | |
|---|---|---|
| "All Medicare prescription drug plans cover the same list of prescription drugs." | 2007-2010 | Potentially useful on its own but superseded by and redundant with the question we use, as coverage is important to the extent it affects out of pocket costs. |
| "The 'Medicare Prescription Drug Plan Finder' is a tool on the Medicare website that helps beneficiaries compare Medicare prescription drug plans in their area. In the past year, (has anyone/[have you/has (SP)]) visited the Medicare website to compare the quality and performance of Medicare prescription drug plans (for [you/(SP)])?" | 2009-2011 | This question evaluates knowledge seeking rather than knowledge per se. |
| Overall, how easy or difficult do you think the Medicare program is to understand? Would you say it is very easy to understand, somewhat easy to understand, somewhat difficult to understand, or very difficult to understand? | 2006-2010 | A general self-assessment, not about Part D specifically, and not a question about specific knowledge. |
| "Can anyone on Medicare sign up for Part D, the Medicare prescription drug insurance program; or can only low-income people on Medicare sign up for Part D; or is neither statement true?" | 2007-2008 | Not useful for assessing the choices of the non-low income enrollees that we study. |
| "How satisfied are you in general with the availability of information about the Medicare program when you need it [for (SP)]?" | 2006-2010 | A general self-assessment, not about Part D specifically, and not a question about specific knowledge. |
| "How interested are you in getting (more) information [for (SP)] about Medicare?" | 2009-2010 | A general question about knowledge seeking, but with ambiguous interpretation, as saying "no" could mean either that they are informed or uninformed. |
| A set of additional questions about where people receive their information about Medicare generally | 2006-2010 | General questions about knowledge seeking. |
| A set of questions about Part D specifically. | 2006 | Only available in 2006, severely limiting the sample and generalizability. |