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ABSTRACT

Neoclassical and psychological models of consumer behavior often make divergent predictions for the welfare effects of paternalistic policies, leaving wide scope for researchers' choice of a model to influence their policy conclusions. We develop a framework to reduce this model uncertainty and apply it to administrative data on consumer decision making in Medicare Part D. Consumers' choices for prescription drug insurance plans can be explained by Abaluck and Gruber's (AER 2011) model of utility maximization with psychological biases or by a neoclassical version of their model that precludes such biases. We evaluate these competing hypotheses using nonparametric tests of utility maximization and a trio of model validation tests. We find that 79% of enrollment decisions in Medicare Part D from 2006-2010 satisfied basic axioms of consumer preference theory under the assumptions of full information, zero transaction cost, and no measurement error. The validation tests provide evidence against widespread psychological biases. In particular, we find that precluding psychological biases improves the structural model's out-of-sample predictions for consumer behavior.

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The classical view of consumer theory maintains that people choose best for themselves. Yet the notion of consumer sovereignty has long evoked criticism. As early as 1966, George Stigler wrote that critics "say that people typically do not maximize anything—that the consumer is lazy or dominated by advertisers or poor arithmetic" (p.2). Since then, economists have found numerous examples of people leaving money on the table, even when the financial stakes are high, e.g. enrollment in retirement savings plans (Madrian and Shea 2001), access to credit (Woodward and Hall 2012, Agarwal and Mazumder 2013), health insurance (Handel 2013) and prescription drug insurance (Abaluck and Gruber 2011). These results are often interpreted as evidence that people make choices that do not maximize their own utility. To explain these results, some researchers have applied the framework of Kahneman, Wakker, and Sarin (1997) in which the "decision utility" (DU) function that guides consumer choice in the marketplace diverges from the "hedonic utility" (HU) function that measures their satisfaction from consuming the purchased goods. Perceived divergences between DU and HU are viewed as a rationale for paternalistic policies intended to increase welfare by guiding people to make better choices, defined as those that come closer to maximizing HU (Camerer et al. 2003).

One high profile example of this approach is Abaluck and Gruber (2011), henceforth AG. They sought to evaluate the quality of consumer decision making in the market for prescription drug insurance plans (PDPs) under Medicare Part D. AG began by showing that, ex post, over 70% of enrollees could have reduced their PDP expenditures without increasing their exposure to risk. They used this nonparametric evidence to motivate a parametric test of whether people's PDP choices were consistent with maximizing a particular HU function that depends on PDP quality in addition to the mean and variance of cost. For the purpose of this test, AG defined the benchmark HU function as a first-order Taylor approximation to a constant absolute risk aversion model and then used data on consumers' PDP choices to estimate a linear and additively separable DU function.[1] Differences between the HU and DU functions were interpreted as evidence that consumers "simply err" due to heuristics or "lack of cognitive ability" (p.1209), creating "welfare loss due to consumer mistakes" (p.1194) that could be avoided by policies allowing "less scope for choosing the wrong plan" (p.1209). Specifically, AG found that their estimated DU function violated three restrictions implied by their chosen HU function, which they interpreted as evidence that consumers make three mistakes: (1) they underweight out-of-pocket costs

[1] AG refer to DU as the "positive utility function" and HU as the "normative utility function."

relative to plan premiums; (2) their choices depend on financial attributes beyond the extent to which those attributes affect their own costs; and (3) they underweight the variance-reducing aspects of plans. AG used these findings—along with the additional assumption that econometric errors in their multinomial logit model represent idiosyncratic mistakes made by consumers—to conclude that consumer mistakes yielded a welfare loss equivalent to 27% of out-of-pocket expenditures on plan premiums and prescription drugs in 2006. Our replication of their analysis shows that just over two thirds of this estimated welfare loss is due to AG's interpretation of the econometric error terms as consumer mistakes.

In this article we develop a methodology for determining when a structural model of decision making can be used to infer the quality of consumers' decisions and use it to assess AG's conclusions. Our research is motivated by the broad interest in evaluating the quality of consumer decision making and its implications for welfare, the policy relevance of health insurance market design, and the challenges inherent in both tasks. In particular, a key challenge with using testable restrictions on parametric models to assess consumer decision making quality is that such tests conflate two distinct explanations for violations of the parametric restrictions. Varian (1983, p.99) summarized the issue as follows: "This procedure suffers from the defect that one is always testing a joint hypothesis: whatever restrictions one wants to test plus the maintained hypothesis of functional form." This raises the question: do violations of AG's restrictions reflect optimization mistakes made by consumers; do they represent a rejection of AG's parametric model for utility; or some combination of the two? We disentangle these hypotheses and test them separately using five years of administrative data from the Centers for Medicare and Medicaid Services (CMS).

We begin by adapting Varian's (1983) nonparametric tests of utility maximization to the PDP markets from 2006 to 2010. During this period the average consumer chose from more than 40 PDPs that differed in terms of expected cost, variance, and quality. We replicate AG's nonparametric analysis (based solely on the mean and variance of cost) and then extend their analysis to recognize that consumers may also care about features of plan quality as in AG's parametric model. Like AG, we control for aspects of PDP quality that consumers observe but analysts do not using the brand name of the firm selling the insurance, so that latent "quality" includes all between-firm differences in PDP attributes besides our measures of mean and variance of ex post costs. We find that between 2006 and 2010 79% of consumers made PDP choices that were con-

sistent with maximizing some utility function satisfying the basic axioms of consumer preference theory under the assumptions of full information and perfect foresight about future drug needs.[2]

A potential limitation of our nonparametric test is that it reveals whether choices are consistent with maximizing *any* utility function that satisfies the basic axioms, even if that implies extreme tradeoffs between PDP attributes. For example, analysts may find it improbable that the average consumer would be willing to pay over $1,000 per year for latent features of PDP quality. We address this potential concern by developing a measure of the willingness to pay for firm-specific quality that is sufficient to rationalize the choice made by each consumer. Over our five year study period the median sufficient willingness to pay is $47, or 4% of out-of-pocket expenditures. We also show that a majority of consumers have the option to choose an inferior PDP offered by their chosen brand and yet most of them avoid doing so. Further, the odds of choosing an inferior plan decline between 2006 and 2010 despite increasing availability of inferior plans. In summary, our nonparametric analysis reveals that AG's evidence of choice inconsistencies is not robust to alternative specifications for utility. This motivates the need to test the fit and predictive power of their structural model against alternative models that make different predictions for the welfare effects of paternalistic policies.

When we estimate AG's multinomial logit model using CMS data we replicate the results that they interpret as consumer mistakes. However, we also find that AG's evidence of mistakes persists for the 25% of consumers who chose plans on Lancaster's (1966) efficient frontier in terms of cost and variance. We then design and implement three tests of AG's structural model of PDP choice.

Our first test estimates AG's model after adding placebo attributes to each PDP. The results imply that consumers are willing to pay about as much for the placebo attributes as they are willing to pay for most of the real financial attributes that AG interpret as consumer mistakes. This is evidence that AG's parametric test of utility maximization is vulnerable to economically important type I errors. Our second test leverages heterogeneity in the PDP menu across 32 CMS markets to investigate whether between-market variation in the signs and magnitudes of the measures that AG interpret as mistakes can be explained by between-market variation in the factors that AG hypothesize to cause mistakes. We find that their measures for mistakes, and the as-

[2] The share of consumers making consistent choices increases if we relax these assumptions to recognize that some consumers are forward looking over multiple years, that consumers differ in the way they form expectations about their future drug needs, or that consumer utility may depend on higher order moments of the distribution of expenditures.

sociated welfare losses, often vary by an order of magnitude or more across regions; they also vary in sign. This variation appears to be unrelated to institutional and demographic factors often found to be correlated with financial literacy and decision making quality, such as age, dementia, and the number of choices available. We interpret these results as evidence of potential model misspecification. Our last test compares the out-of-sample predictive power of AG's model to their benchmark model that assumes consumers maximize expected utility. Despite having less econometric flexibility, the model that assumes people do not make any of AG's three explicit mistakes performs about as well, and often better, at predicting how people make choices when they are faced with different PDP options.

Overall, we find that AG's evidence of welfare-reducing optimization mistakes is driven primarily by their assumptions about the parametric form of utility and by interpreting econometric errors as consumer mistakes. Our analysis of the CMS data provides evidence that consumers pay attention to how the financial attributes of PDPs affect their own costs.[3] We also find that a simpler version of AG's model that assumes people maximize expected utility often makes better out-of-sample predictions. While these empirical results do not prove that people always make fully informed enrollment decisions in Medicare Part D, they do suggest that welfare-reducing mistakes may not be as large or as widespread as AG concluded.

I. Testing the Consistency of Consumer Choices in Medicare Part D

In this section we explain key aspects of Medicare Part D and the distinction between parametric and nonparametric tests of utility maximization in a differentiated product market. The purpose is to provide context for our nonparametric analysis in Sections II and III and our parametric analysis in sections IV and V.

A. A Standard Model of Prescription Drug Plan Choice

The Center for Medicare and Medicaid Services (CMS) divides the nation into 34 regions, each of which offers a distinct set of PDP options.[4] During the annual open enrollment, consumers choose a PDP for the following year. Consider the enrollment period in a single region. Consumers are free to choose among $j=1,\ldots,J$ plans that differ in terms of the premium, p_j, and a

[3] The CMS data mitigate measurement errors present in the data used by AG and consequently overturn AG's finding that consumers ignore the individual benefits of purchasing gap coverage, which led AG to conclude that "individuals consider plan characteristics in making their choices—but not how those plan characteristics matter for themselves" (p 1191).
[4] For the list of regions see: http://www.q1medicare.com/PartD-2013MedicarePartDOverview-Region.php.

vector of variables defining drug costs, c_j, that includes the deductible and the price structure for each available level of coverage. PDPs may also differ in a vector of quality attributes, q_j. Examples include customer service, pharmacy networks, the ease of obtaining drugs by mail order and the presence of supply-side controls such as prior authorization requirements. These characteristics determine the time and effort required for a consumer to obtain her eligible benefits under the plan.

Consumer *i*'s expenditures under plan *j* equal the premium plus the out-of-pocket (OOP) costs of any drugs she purchases. Expenditures can be written as $p_j + oop(c_j, x_{ij})$, where x_{ij} is a vector of drug quantities. In general, OOP costs are a nonlinear function of drug purchases due to the plans' designs. The consumer's health depends on a random shock, w_i, that she realizes after choosing a plan, and on her drug consumption: $h_{ij} = h(w_i, x_{ij})$. Utility is a function of the consumer's health, the quality of her PDP, and her consumption of a composite numeraire good, m_i. It is useful to decompose the utility maximization problem into two stages, following Cardon and Hendel (2001).[5] In the first stage the consumer selects a plan, and in the second stage she experiences a health shock and purchases drugs. The second stage problem of optimal drug consumption can be written as:

$$(1) \qquad U_{ij}^* = U^*(h_{ij}, q_j, m_i) = \max_{x_{ij}} U[h(w_i, x_{ij}), q_j, m_i]$$

$$\text{subject to } m_i = y_i - p_j - oop(c_j, x_{ij}),$$

where U_{ij}^* is the indirect utility that consumer *i* experiences from plan *j* at her optimal level of drug consumption conditional on that plan. Optimal drug consumption may differ from plan to plan due to variation in drug prices and plan quality.

The consumer's expected utility from plan *j* is defined by integrating over her perceived distribution of health shocks, characterized by density function $f_i(s_i)$.

$$(2) \qquad V(h_{ij}, q_j, y_i - p_j - oop_{ij}) \equiv E(U_{ij}^*) = \int U^*(h(s_i, x_{ij}), q_j, m_i) f_i(s_i) ds_i.$$

Comparing expected utility over the *J* plans leads to the first stage problem of choosing the utility maximizing plan:

[5] Zeckhauser (1970) represents the earliest predecessor known to us. McGuire (2012) follows Goldman and Philipson (2007) with a slightly different approach in which the consumer chooses the level of medical care and an optimal coinsurance rate, where premia are a function of those. Cardon and Hendel's approach is also used in Handel (2013) and Einav et al. (2013).

(3) $$\max_j \ \{ V(h_{ij}, q_j, y_i - p_j - oop_{ij}) \}.$$

In principle, each consumer's PDP choice and subsequent OOP expenditures can be observed, along with p_j, q_j, and c_j for every plan. The challenge is to use this information to test whether consumers make choices that are consistent with utility maximization under full information.

B. Parametric and Nonparametric Tests of Utility Maximization

Consistent with Varian (1983), we define a *nonparametric test* of utility maximization as a test of whether the data could have been generated by maximizing a utility function that satisfies basic axioms of consumer theory (e.g. completeness, transitivity, nonsatiation). In contrast, a *parametric test* assesses whether the data were generated by maximizing a particular utility function. Both tests require data on every variable that enters the maximization problem in (3). First, the analyst must make an assumption about which moments of the consumer's perceived joint distribution of health outcomes and OOP expenditures enter the indirect utility function in (2). Second, the analyst must make an assumption about the form of $f_i(s_i)$ to construct those moments for each of the *J* available plans.

Given these assumptions and data on every plan, the analyst can nonparametrically test whether people choose plans that lie on what Lancaster (1966) defined as the "efficiency frontier" in characteristics space. A plan is on the frontier if and only if it is not dominated by another plan on every characteristic. Any choice on the frontier is consistent with the basic axioms of consumer theory, and any choice off the frontier violates a basic axiom. To conduct a parametric test, the analyst must further specify the form of utility, and the test results indicate whether consumers' choices are consistent with maximizing that particular utility function. Hence, parametric and nonparametric tests present a tradeoff between Type I and Type II errors. A nonparametric test that fails to reject the hypothesis of utility maximization may imply that consumers are willing to make tradeoffs between attributes that some researchers view as too extreme to be anything other than a mistake. In contrast, a parametric test that rejects the hypothesis of utility maximization may actually be rejecting the researcher's assumption for the parametric form of utility in the sense that consumers' choices maximize other utility functions with attribute tradeoffs that researchers or policy makers would view as reasonable.[6]

[6] This assumes the analyst has data on every variable in the maximization problem in (3) and there is no measurement error. Nonparametric tests

If model misspecification can be ruled out, then a parametric test would be decisive and the results could be used to identify mistakes in the choice process and guide welfare-improving policies. The innate inability of any analyst to know the parametric form of consumers' utility, apart from revealed preference logic, motivates our research design. First, we use nonparametric tests to reveal whether the results from a parametric test are idiosyncratic to the analyst's chosen functional form or whether they are robust across the full scope of utility functions that satisfy the axioms of consumer preference theory. Second, we design tests for misspecification of a parametric model of the choice process.

II. Using CMS Data to Reassess the Facts on Plan Choice

A. Data on drug plan choice and prescription drug expenditures

We worked with the Centers for Medicare and Medicaid Services (CMS) to obtain data on Medicare beneficiaries' demographics, medical conditions, prescription drug use, PDP choices, and the set of plans available to them. Additionally, we incorporate institutional knowledge from CMS to develop the best available calculator for the costs each person would have incurred in each plan that was available to her.[7] We begin with a random 20% sample of every Medicare beneficiary and then impose a few eligibility criteria. Most importantly, we follow AG in limiting our analysis to people who chose a standalone PDP and did not receive a federal low-income subsidy. Following AG's methodology, we also excluded those for whom we could not calculate the plan-specific variance in costs.[8]

Table 1 summarizes our CMS data on plan spending and compares it to the AG data. The AG data were obtained from Source Healthcare Analytics (then named Wolters Kluwer Health (WKH)) which collects data primarily via contracts with pharmacies. The WKH data have two main limitations for studying PDP choice. First, they do not typically capture all of the prescriptions filled by a given individual in a given year. As a result, the actual average OOP costs ($994) are approximately 50% higher than reported by AG ($666).[9] Second, the WKH data do

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are only affected by measurement error in the case where the error changes the ordering of goods in attribute space. Parametric tests are more vulnerable to measurement error due to the cardinal nature of the testing procedure.

[7] The "cost calculator" is described in Ketcham, Lucarelli and Powers (2014). The correlation between calculated spending and actual spending ranges from .92 in 2006 to .98 in 2009. AG do not report comparable statistics for their calculator.

[8] We adopt AG's approach to defining variables except where noted otherwise. Their approach to defining variance was based on grouping people into 1000 different cells based on their prior year's total drug spending, days' supply of branded drugs, and days' supply of generic drugs. Whereas they used a random sample of 200 people from everyone in their data to define a cell's variance regardless of region or plan type, we use the full set of people enrolled in a PDP in the same region.

[9] We cannot determine the exact percentage because the WKH data do not identify whether a consumer was enrolled in a PDP for the entire year. The $995 average cost measure reported for the 2006 CMS data in Table 1 is calculated for consumers who were enrolled the entire year. If we

not report which plan each person selects or even whether a person selects a PDP at all. AG attempted to infer each person's chosen plan based on their OOP costs but found that 50% could not be matched to any plan and 21% were matched to multiple plans.[10] To overcome this problem, AG randomly assigned people to one of the multiple potential matches with probabilities chosen to reproduce each plan's national market shares.[11] This assignment rule helps to explain the biases in AG's data evident from Table 1 and in their findings on gap coverage explained below.

TABLE 1—COMPARING CMS AND AG DATA ON PLAN CHOICE, 2006-2010

	AG's data	Our CMS data				
	2006	2006	2007	2008	2009	2010
number of consumers used in estimation	95,742	479,657	582,619	618,220	630,282	643,335
number of plans	702	1,348	1,607	1,719	1,632	1,513
number of states	47	50	50	50	50	50
number of contract id's	36	73	77	78	71	68
number of brands		69	76	75	71	66
mean age	75	76	76	76	76	76
% female	60	63	64	63	63	62
average premiums ($)	287	362	369	415	487	516
average out-of-pocket costs ($)	666	994	892	858	892	886

Note: The table compares summary statistics reported in AG using the Wolters Kluwer Health to our data from Centers for Medicare and Medicaid Services. We focus on consumers who were enrolled for the entire year, using a 20% sample in 2006 and a 10% sample in 2007-2010.

The final novelty of our data is our approach to defining brands. AG used CMS contract IDs to create dummies meant to capture unobserved PDP quality. As the name implies, contract IDs exist for contracting purposes between insurers and CMS; they are not observed by consumers and in many cases do not match the brand names seen by consumers. Instead, we use company and plan names to create an alternative set of brand dummies that we expect to be meaningful to consumers.[12]

add all the consumers who enrolled for only part of the year, the figure drops to $890. In this article we restrict our attention to people who were enrolled in a single PDP for the full 12 months, as we expect the choice process may differ for part year enrollees. Excluding them also makes 2006 more comparable to later years, as open enrollment extended through May, creating a large cohort of part-year enrollees for 2006 not found in other years.

[10] This inference is difficult as a drug can have more than 32 different OOP costs within a single PDP because OOP costs can differ with the 4 phases of coverage, pharmacy type, pharmacy network status, quantity dispensed, and other attributes. Due to this complexity, AG assigned a person to a plan if they could match (defined as a price within 5%) as few as 50% of the person's observed OOP costs to the formulary of that plan every month from June 2006 through December 2006.

[11] The use of national market shares is problematic because a given plan's market share often varies dramatically across CMS regions due, in part, to variation in the set of competing plans offered in each region.

[12] Both approaches yield approximately the same number of brands in every CMS region and year, as can be seen from Table 1. CMS made company and plan names available to researchers in July, 2014. Table A1 provides an example of how we use these new variables to create brand dummies that differ from those used by AG. Replacing contract id with our brand dummies increases the pseudo R^2 for AG's parametric model from 0.32 to 0.37. Some of AG's key results are sensitive to which brand indicators are used e.g. as evident by comparing the region-specific results in Figures 3 and A3. We report the robustness of our main results to using AG's dummies in Table A11.

B. Evaluating AG's Conclusions about Enrollment in Gap Coverage

AG used the choice of plans with gap coverage in 2006 as their leading result in support of their conclusion that consumers fail to pay proper attention to plan attributes. Specifically, they observed that "the percentage choosing donut hole coverage is *virtually flat* throughout the spending distribution…[with] *the same proportion* of individuals in the tenth and eighty-fifth percentile of the spending distribution choose donut hole [i.e. gap] coverage" (p 1192, emphasis added). They interpreted the lack of a positive relationship between total spending and enrollment in gap coverage as evidence of consumer mistakes because the expected benefits of gap coverage are higher for someone in the 85th percentile than in the 10th percentile.[13]

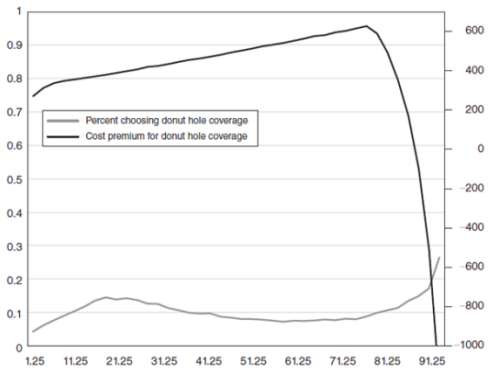
AG's evidence of this effect is provided in panel A of Figure 1 (reproduced from AG Figure 3). The horizontal axis measures the quantile of total (OOP plus third party paid) expenditures on prescription drugs. The right vertical axis measures the "cost premium" for gap coverage, defined by the amount an individual would save by choosing the lowest cost available plan *with* gap coverage instead of the lowest cost available plan *without* gap coverage. The top locus depicts the average cost premium, conditional on drug expenditure quantile. The left vertical axis and the lower locus indicate the percent of people in each expenditure quantile who choose a plan with gap coverage. If people were paying attention to plan attributes, preferred lower-cost plans, and had some foresight about their drug expenditures, then *ceteris paribus* we would expect the probability of selecting gap coverage to be inversely associated with the cost premium. The failure to observe such an upward-sloping relationship in panel A is what led AG to conclude that individuals do not consider how plan characteristics influence their own drug expenditures.

The CMS data overturn AG's finding that consumers failed to consider how gap coverage mattered for themselves, as shown in Panel B. The probability of selecting gap coverage increases throughout the spending distribution up to the catastrophic coverage limit, and the rate of increase rises in the gap, where the thresholds at which people enter and exit the gap are demarcated by the vertical lines. The probability of selecting gap coverage is strongly, positively associated with total drug spending and inversely associated with the cost premium. In contrast

[13] In 2006, the standard PDP design was required to insure annual prescription drug costs with actuarial equivalence to a schedule that varied with the beneficiaries' OOP and gross drug cumulative annual spending. This included no coverage between $2,500 and 5,100 in total (OOP plus insurer-paid) costs. Many insurers deviated from this standard plan and offered "enhanced" plans with coverage in the "gap" for generic drugs or generic and brand drugs.

to the statistic that AG report, the percentage of people choosing gap coverage at the eighty-fifth percentile is actually 4 times larger than the percentage at the tenth percentile: 23.3% compared to 5.7%. This is consistent with people paying attention to the effects of gap coverage on their own spending because gap coverage *saves* enrollees an average of $185 at the eighty-fifth percentile and *costs* them an average of $283 at the tenth percentile. Evaluating the right tail of the spending distribution in panel B provides further evidence that people are more likely to choose gap coverage if their cost savings from doing so are higher. For people who exit the gap and enter the catastrophic coverage phase, the cost premium again begins to rise and enrollment in gap coverage declines.[14]

A. Gap coverage in 2006: AG



B. Gap coverage in 2006: CMS data

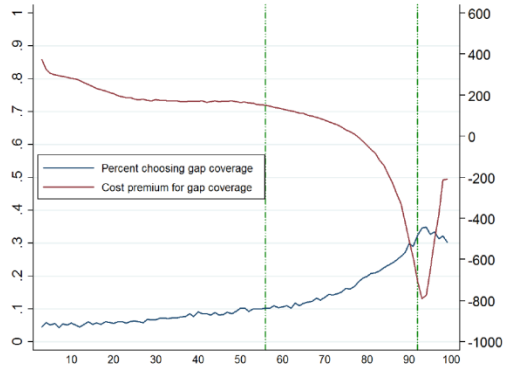


FIGURE 1: PERCENT CHOOSING GAP COVERAGE AND ADDED COST BY EXPENDITURE QUANTILE

Overall, panel B provides evidence that people in fact did consider how gap coverage mattered for themselves in 2006.[15] This finding is especially noteworthy because the market was new. People appear to have anticipated their own future drug consumption and considered how this peculiar plan attribute, not commonly found in other insurance products, would ultimately affect their costs.[16]

Several aspects of AG's data and methodology may have contributed to their inability to de-

[14] We do not know whether AG found the same result because their figure was truncated at the 91.25th percentile. Also in contrast with panel A, the cost premium declines as expenditures rise. The premium peaks well under $400 at the 0th percentile, whereas AG indicated that it peaks above $600 around the 80th percentile. This divergence could be caused by the fact that AG's cost calculator assumes every drug had a uniform negotiated price across all plans, whereas the large insurers with popular gap plans, *ceteris paribus*, would be able to negotiate lower prices for a given drug.

[15] The experiences of insurers provides additional support for this result. Specifically, the largest provider of full gap coverage, Humana, was reported to lose $20 million (33%) on their full gap plan in 2006 due to the heavy use of gap coverage among that plans' enrollees. As a result of these losses, for 2007 they withdrew full gap coverage from the market.

[16] Figure A1 shows that enrollment in gap coverage increased in regions and years where the benefits from gap coverage were larger. Table A2 shows that similar patterns exist for other plan attributes. For example, enrollees' paid $112 below average in OOP costs; they selected a plan with 17% less variability in OOP spending; and chose a plan with a 6% higher quality rating.

tect the positive relationship between the benefits of gap coverage and enrollment in plans with that coverage. Perhaps the most important derives from the fact that AG could not observe people's actual PDP choices in their data. In the common case that their choice imputation procedure yielded more than one potentially chosen PDP, AG randomly assigned people to plans with the probability of assignment chosen to reproduce national market shares. As the CMS data reveal, the probability of selecting gap coverage is increasing in drug expenditures. Consequently, random assignment would have flattened the curve by placing upward bias on the probability of gap coverage at low expenditure quantiles and downward bias on the probability of gap coverage at high expenditure quantiles. In fact, AG noted that their inability to match people to plans was especially severe for Humana (p. 1188), one of the largest providers of gap coverage in 2006 and, "the insurer that offered the most generous [gap] coverage" (p.1206).[17]

AG emphasized that failure to find a positive slope in Figure 1A suggests choice inconsistencies. One might also ask whether anything less than full enrollment in gap coverage at expenditure quantiles with a negative cost premium is evidence of choice inconsistencies. Inspecting the micro data underlying the quantile averages in panel B reveals this is not the case: even among people who ended the year in the gap, the cost premium for gap coverage is *positive* for 64%. Hence if every person chose the plan that minimized their ex post costs, then 36% of people in the gap would have gap coverage. Looking at the full spending distribution, 14% of people could have saved more than $100 by switching into or out of gap coverage (9.9% by switching to a plan with gap and 4.2% by switching to a plan without gap). Raising the threshold to $300 lowers these percentages to 6.5% and 1.1%, respectively; raising it to $750 lowers them to 2.5% and 0.0%.[18] Uncertainty about future drug consumption provides a potential explanation for why people left money on the table on the basis of this metric, which relies on ex post drug consumption to define the cost premium. The extent to which gap coverage raises or lowers an individual's costs can vary widely over small movements in the ex post spending distribution around the gap and catastrophic thresholds, as can be seen from the quantile averages in the figure. Furthermore, the analysis in Figure 1 is unconditional on plan variance and quality. For example,

[17] Many states provide subsidies to populations that do not qualify for the federal low income subsidies. Individual use of these subsidies is not recorded in CMS or WKH data. The following states did not offer these programs from 2006-2010: AL, AR, CO, FL, GA, IA, ID, KS, KY, MI, MN, MS, NE, NH, SC, SD, TN, TX, UT, VA, WV, WY. When we reproduce Figure 1.B using only these states the share of consumers enrolled in gap coverage at the 85th percentile of the expenditure distribution is more than 5 times as large as the share at the 10th percentile (27% compared to 5%). This provides further evidence that people pay attention to how gap coverage benefits themselves as the benefits are smaller in states with these subsidies in ways not incorporated into the CMS data, the cost calculator or the gap cost premium.

[18] Table A3 provides results from additional thresholds in 2006 and 2007. The results show that the share with potential savings at these thresholds in 2007 was about half the share in 2006.

among the 4.2% who chose a gap plan but could have saved at least $100 by choosing a non-gap plan, the gap plan may maximize utility through its ability to reduce variance or through any other features of plan quality provided by plans with gap coverage. Our nonparametric tests reveal the extent to which the data are consistent with this explanation.

III. Nonparametric Tests of Consistency with Utility Maximization

The generalized axiom of revealed preference (GARP) is often used to test whether data are consistent with utility maximization. However GARP is not directly applicable to PDP choice because consumers are not free to choose continuous combinations of plan characteristics and the budget constraint is nonlinear.[19] Nevertheless, similar axioms imply that a utility maximizing consumer will choose a plan that lies on Lancaster's (1966) efficiency frontier. Suppose a consumer is risk averse and has preferences that are *complete*, *transitive*, and *strongly monotonic* over the attributes of PDPs.[20] Under these mild restrictions, optimization under full information implies the consumer will never choose a plan, *k*, that lies below the frontier in the sense that when we compare it to another feasible plan, *j*, plan *k* has: (i) equal or higher expected OOP costs, (ii) equal or more variable OOP costs; (iii) equal or lower values for every dimension of perceived plan quality; and (iv) at least one of these inequalities is strict. In this case plan *j dominates* plan *k* for any utility maximizing consumer. Therefore, calculating the share of people who choose dominated plans provides a nonparametric test of choice consistency that is robust to any utility function satisfying completeness, transitivity, and strong monotonicity. These mild restrictions are consistent with a broad class of preferences that allow utility to be nonlinear and nonseparable in plan attributes. They also allow for flexible forms of heterogeneity in risk aversion and in relative preferences for plan quality. For example, a utility maximizing consumer with strong preferences for more extensive formulary coverage may choose a plan offered by an insurer that places fewer restrictions on drugs, even if our measures of the mean and variance of ex posts costs are higher under that insurer's plans.

Table 2 reports the share of people choosing plans on the efficiency frontier each year from 2006 through 2010. As we move from row 1 to row 5 we expand the set of attributes assumed to

[19] See Kariv and Silverman (2013) for an overview of the challenges in testing whether data are consistent with utility maximization when consumers are free to choose continuous bundles of homogeneous goods with linear budget constraints.

[20] Completeness says that consumers can compare any two plans. Transitivity says that if plan A is preferred to plan B, and plan B is preferred to plan C, then plan A must be preferred to plan C. Strong monotonicity says that, all else constant, consumers prefer plans with more of any positive attribute.

enter utility. We start in row 1 with the naïve assumption that mean ex post cost is the only attribute that people value. Specifically, we follow AG's assumption that consumers should have perfect foresight on their future drug needs and set $E(oop_{ij})$ for year *t* equal to plan *j*'s cost of purchasing the drugs that consumer *i* actually purchased that year. The share of people choosing frontier plans in this single dimension ranges from 6% to 10% each year. In row 2 we add $var(oop_{ij})$. Accounting for the variance raises the fraction of people on the frontier to between 24% and 36%. The fraction in 2006 is 25%, just below the 30% reported by AG. Importantly, this is where AG stopped testing.

Row 3 adds an index of overall plan quality developed by CMS.[21] This raises the share of people choosing frontier plans to 33% to 46%. Row 4 replaces the CMS index with brand dummies, just as AG do in their parametric model.[22] The difference is that the nonparametric test allows people to have heterogeneous preferences for unobserved features of PDP quality that vary from brand to brand.[23] When we add these dummies, 73% to 82% of choices are consistent with maximizing a well behaved utility function. The share of people choosing frontier plans increases further if we allow utility to depend on higher order moments of the OOP distribution, if we allow the demand for drugs to be less than perfectly inelastic, if we introduce forward looking behavior and switching costs, or if we allow for heterogeneity in expectations. Row 5 illustrates this point by relaxing the assumption that every person knows their future drug consumption. Some people may expect their future drug consumption to be the same as their past consumption, for example. We allow this possibility by calculating two separate measures of $E(oop_{ij})$—one based on the person's drug consumption in year *t* and one based on her consumption in year *t-1*. Adding both variables to the utility function recognizes that uncertainty about drug consumption may cause a person's expectations to be a weighted average of these two cases.[24] This further increases the rate of consistent choices to as high as 89% in 2009.[25]

[21] CMS did not construct the quality index for 2006. AG used the 2008 CMS quality index for 2006. We use 2007 ratings, the earliest available, for 2006. We use updated ratings for each subsequent year, e.g. the 2008 ratings are used for 2008.

[22] The CMS quality index is essentially redundant at this point because there is minimal variation across plans within a brand for a given year. Adding it as an additional attribute to rows 4 and 5 has virtually no effect on the results.

[23] Appendix Table A4 shows that the results are very similar if we follow AG in using contract ID's to define brand.

[24] Formally, define consumer *i*'s expected OOP costs for plan *j* during year *t* at the time of enrollment as $E[OOP_{ijt}] = \alpha_i OOP_{ijt} + (1 - \alpha_i)OOP_{ijt-1}$, where α_i is between 0 and 1. When we admit that we do not know α_i, we cannot conclude that plan *j* dominates plan *k* unless $E[OOP_{ijt}] < E[OOP_{ikt}]$ for every feasible value of α_i. Therefore, plan *k* is only dominated if $OOP_{ijt} < OOP_{ikt}$ and $OOP_{ijt-1} < OOP_{ikt-1}$.

[25] We cannot perform this test for 2006 because we do not have data on drug consumption for 2005.

TABLE 2—NONPARAMETRIC TEST OF CHOICE CONSISTENCY

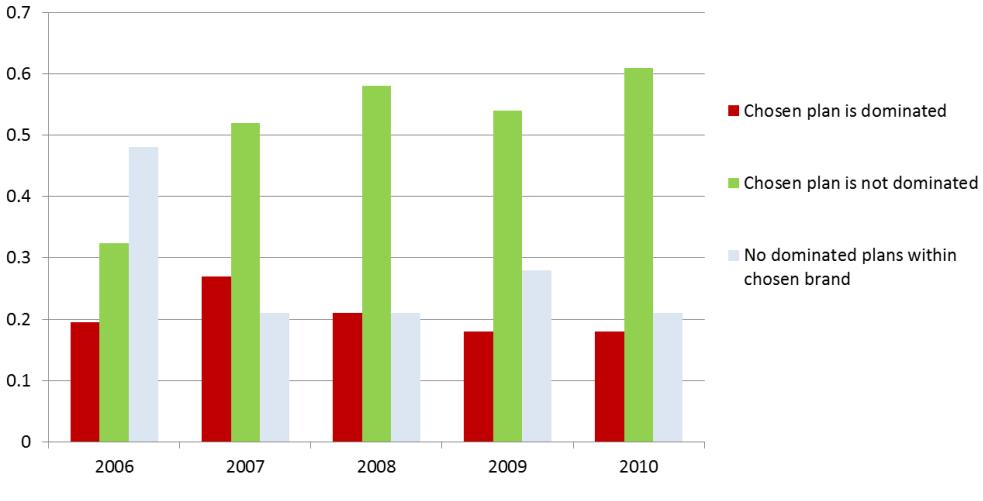
	Plan attributes affecting utility	Assumption on expected drug expenditures in year *t*	% Consumers choosing frontier plans					
			2006	2007	2008	2009	2010	2006-2010
(1)	E[cost]	year t drug consumption	7	7	10	6	8	8
(2)	E[cost], var(cost)	year t drug consumption	25	24	24	26	36	27
(3)	E[cost], var(cost), CMS quality	year t drug consumption	35	33	46	42	45	41
(4)	E[cost], var(cost), brand	year t drug consumption	80	73	79	82	82	79
(5)	E[cost], var(cost), brand	year t or t-1 drug consumption		80	86	89	87	86

Note: The table reports the share of people choosing undominated plans on their efficiency frontier as a function of plan attributes and modeling assumptions. See the text for details.

Table 2 reveals that understanding the roles of PDP attributes captured by the brand dummy variables is essential to determining whether most people are making choices that are consistent with expected utility maximization. This raises three questions. *First, is it plausible for people to have heterogeneous brand preferences?* We think the answer is yes. Brands differ in their formulary design for specific drugs in ways not reflected in our measures of mean and variance of ex post OOP costs. For example, brands with high cost sharing (e.g. high copays or lack of coverage altogether) on certain drugs may be unattractive to people who have a high likelihood of purchasing those drugs and irrelevant to people who do not. These aspects are not fully captured by the measured mean and variance of ex post costs. Likewise brands differ in their reliance on supply-side controls such as prior authorization and "fail first" requirements, which are also not incorporated into the mean and variance of ex post costs. Brands also differ in terms of customer service, ease of obtaining drugs by mail order and pharmacy networks. Each of these differences has heterogeneous effects across consumers that cannot be captured by CMS's homogenous star ratings.[26] People also differ in their past experiences with particular insurance companies, e.g. while they were covered through their employer prior to age 65. In fact, when Medicare beneficiaries were asked about the factors affecting their choice of PDP in a 2006 survey 90% of respondents stated that company reputation was "important" or "very important" to their choice (MedPAC 2006). Other factors that respondents commonly identified as important or very important included having a preferred pharmacy in the plan's network (84%) and signing up with the same company as a spouse (42%). These factors vary across brands and people, but not across plans within a brand, making brand dummies the natural proxy measure of these horizontally differentiated attributes.

[26] Furthermore, CMS did not assign star ratings until 2007 so this information would not have been available to enrollees during the 2006 enrollment cycle.

Second, what causes the share of people choosing plans on the efficiency frontier to rise when brand dummies are added in row 4 of Table 2? The first mechanism is the quality of decision making. Each year, the majority of people (52% in 2006 and 72- 79% in later years) had the opportunity to choose a plan offered by their chosen brand that was dominated in terms of mean and variance of ex post cost. The first two bars within each year in Figure 2 summarize their actual choices: the first bar reports the share of people who chose dominated plans; the second bar reports the share of people who avoided doing so and chose a frontier plan. Comparing the relative sizes of the two groups across years reveals that the odds of avoiding dominated plans increased over time. Among those offered a dominated plan, the share choosing a plan on their frontiers climbed steadily from 62% in 2006 to 77% in 2010. To put both the levels and trends in perspective, if these people had chosen randomly within their chosen brand then the percent of them in a frontier plan would have been 54%, 59%, 59%, 56% and 54% for 2006-2010 respectively.[27] That said, random choice does not represent a lower-bound. If consumers' choices embed biases and sophisticated firms design products to profit from those biases (Gabaix and Laibson 2006, Spiegler 2011, Miravete 2013), then we might expect consumers to do worse than random, not better as shown in the data.



	2006	2007	2008	2009	2010
Percent offered a dominated plan	52%	79%	79%	72%	79%
Percent of those who avoid dominated plans	62%	66%	73%	75%	77%

FIGURE 2: CONSUMERS GROUPED BY THE OPPORTUNITY TO CHOOSE A DOMINATED PLAN

[27] The improvement is similar when we focus on 66-year olds who are making their first full-year PDP choice and are therefore less susceptible to any state dependence. Hence, the improvement in choice quality over time is at least partly driven by active choices.

The second mechanism is the lack of opportunity to choose a dominated plan. The last group in Figure 2 includes people whose chosen brand does not offer them a dominated plan. After 2006, the share of people in this group ranged from 21-28%. Their brands offer either a single plan or multiple plans on the person's frontier, e.g. one low-cost, high-variance plan and one high-cost, low-variance plan. If consumer utility depends on horizontally differentiated attributes that are captured by the brand indicators then these peoples' choices are de facto consistent with maximizing utility functions that satisfy the basic axioms.

The fact that we can explain most consumers' PDP choices as reflecting preferences for latent features of PDP quality that vary from brand to brand raises a final question. *Are the brand preferences required to rationalize choices so large that we should view them as evidence of mistakes?* Analysts may wish to move away from testing for consistency of choices with the axioms of consumer theory and instead place an upper bound on how much a fully informed utility-maximizing consumer would be willing to pay for unobserved features of PDP quality associated with their preferred brand. To investigate how such thresholds affect the results, we calculate the willingness to pay for all between-brand differences besides measured mean and variance of ex post costs that would be sufficient for a consumer's PDP choice to maximize a utility function that satisfies the basic axioms. This measure of "sufficient willingness to pay" (SWTP) is defined as the cost of the consumer's chosen plan less the highest-cost plan on the portion of her cost-variance frontier that dominates her chosen plan.[28] Intuitively, SWTP is the amount of money that a person leaves on the table by choosing a plan off the cost-variance frontier.

TABLE 3—SUFFICIENT WILLINGNESS TO PAY FOR BRAND

	2006	2007	2008	2009	2010	2006 - 2010
			All consumers			
median SWTP for brand ($)	80	44	81	41	26	47
percent of consumers with SWTP > $500	7.3	2.2	2.1	1.2	2.0	2.4
percent of consumers with SWTP > $1,000	1.6	0.6	0.1	0.1	0.2	0.4
	Consumers off the cost-var frontier, but on the cost-var-brand frontier					
percent of all consumers	56	49	55	55	47	52
mean SWTP for brand ($)	232	126	169	116	139	147
median SWTP for brand ($)	129	65	136	75	85	89

Note: The table reports the willingness to pay for brand that is sufficient for consumers' PDP choices to maximize a utility function that satisfies the basic axioms of consumer preference theory. See the text and appendix for details.

[28] The appendix includes a diagram of the SWTP calculation (Figure A2). This is the minimum WTP to trade the bundle of all omitted PDP attributes of the most expensive plan on the segment of the cost-variance frontier that dominates the chosen brand for the bundle provided by the chosen brand. Central to the logic of this statistic is the fact that by definition, choice of *any* plan on the frontier is a consistent choice.

Table 3 summarizes the SWTP distribution. When we pool the data over all consumers and years, the median SWTP for brand is \$47, or 4% of total expenditures.[29] The last three rows show results for people who chose a plan off their cost-variance frontier but on their cost-variance-brand frontier; i.e. the people who are added when we move from row 2 to row 4 in Table 2. Their mean SWTP is \$232 in 2006 and it ranges from \$116 to \$169 thereafter. These means are heavily affected by small shares of consumers who exceed the \$500 and \$1,000 thresholds in the second and third rows of the table. Annual median SWTP ranges from \$65 to \$136. Although interpretations may differ, we find it plausible that informed consumers would be willing to pay these amounts for the bundle of unobserved PDP attributes that differ between brands.[30]

In summary, during the first five years of Medicare Part D, 79% of consumers made choices consistent with maximizing *some* well-behaved utility function that depends on the mean and variance of ex post cost and all other attributes that differ between brands. These results suggest that AG's parametric evidence of choice inconsistency is primarily driven by their assumptions about the parametric form of the representative consumer's utility function.

IV. Using CMS Data to Replicate and Extend AG's Main Results

A. Replication of AG's Main Parametric Results and Further Analysis

AG assume that consumers' decision utility (DU) function is a first order Taylor approximation to a CARA model that is linear and additively separable in plan characteristics,

$$DU_{ij} = p_j\alpha + \mu_{ij}\beta_1 + \sigma_{ij}^2\beta_2 + c_j\beta_3 + q_j\gamma + \epsilon_{ij}, \tag{4}$$

where p_j is the plan premium, $\mu_{ij} = E(oop_{ij})$, $\sigma_{ij}^2 = var(oop_{ij})$, q_j is a vector of quality variables (CMS quality index or brand dummies), and c_j is a vector of financial plan characteristics that directly affect oop_{ij}. This includes the plan deductible, an indicator for whether the plan provides full coverage of brand name drugs in the gap, an indicator for whether the plan only covers generic drugs in the gap, a count of the top 100 drugs covered by the plan, and a cost sharing index

[29] This includes 21% of consumers whose chosen plans are dominated by another plan within their chosen brand by treating their SWTP as infinite. If we instead focus on the 79% of consumers who choose plans on their cost-variance-brand efficiency frontier (i.e. the consumers represented by the second and third bars in Figure 2) then the median SWTP is \$32 or 3% of expenditures.

[30] Researchers can also calculate SWTP under stronger restrictions on the shape of the utility function. For example, if we assume that utility is separable in the omitted variables captured by the brand indicators, then SWTP can be measured by the amount of money that would be saved by switching to the *least* expensive plan on the portion of the cost variance frontier that dominates the chosen brand. Adding the separability assumption causes the median SWTP to increase from \$47 to \$138 and the share with SWTP over \$1000 to increase from 0.4% to 0.9%.

that measures the average percentage of expenditures covered by the plan between the deductible and the gap. Finally, ϵ_{ij} is a random person-plan specific shock that is assumed to be drawn from a type I extreme value distribution.

TABLE 4— REPLICATION OF AG AND SENSITIVITY TO ALTERNATIVE SPECIFICATIONS

	(1)	(2)	(3)	(4)	(5)
Premium [hundreds]	-0.499 (-0.006)***	-0.562 (0.002)***	-0.402 (0.002)***	-0.099 (0.001)***	-0.620 (0.007)***
OOP costs [hundreds]	-0.096 (-0.002)***	-0.102 (0.001)***	-0.108 (0.001)***	-0.099 (0.001)***	-0.410 (0.002)***
Variance (millions)	–0.0006 (0.0010)	-0.00005 (0.000)	-0.0001 (0.000)	-0.00001 (0.000)	-0.001 (0.000)***
Deductible (hundreds)	-0.163 (0.0070)	-0.020 (0.003)***	0.051 (0.003)***		0.180 (0.008)***
full gap coverage	1.762 (0.0280)	1.909 (0.015)***	1.162 (0.015)***		1.649 (0.038)***
generic gap coverage	0.300 (0.0180)	0.533 (0.009)***	0.356 (0.009)***		0.727 (0.029)***
Cost sharing	1.189 (0.0740)	-0.334 (0.025)***	0.683 (0.024)***		-1.987 (0.090)***
Number of top 100 drugs on formulary	0.059 (0.0020)	0.190 (0.002)***	0.175 (0.001)***		0.386 (0.008)***
Brand definition	contract id	contract id	brand name	brand name	brand name
Pseudo R^2	--	0.32	0.37	0.36	0.60
number of consumers	95,742	464,543	464,543	464,543	117,078
Consumers on the cost-variance frontier	30%	25%	25%	25%	100%
Expected welfare loss (% of costs)					
ε ≡ 0	27.0	27.8	38.9	139.5	23.4
ε is unrestricted	--	9.2	7.4	0.0	10.0

Note: Column 1 is copied directly from column 3 of AG's Table 1. Column 2 reports results from estimating the same econometric specification using our CMS data. Column 3 replicates the model but uses company and plan names instead of contract IDs to define the brand dummies. Column 4 shows results from a model that requires each of AG's parametric restrictions be met. Column 5 repeats column 3 but on the subset of consumers that chose a plan on their cost-variance frontier. *** Significant at the 1% level.** Significant at the 5% level. * Significant at the 10% level.

AG rely on revealed preference logic to interpret their estimate for $-\alpha$ as the marginal utility of income and their estimate for γ as the marginal utility from plan quality. In contrast, they rely on their assumption for the hedonic utility (HU) function to define appropriate values for the β parameters: (1) $\beta_1 = \hat{\alpha}$ because consumers should assign equal weight to premiums and expected OOP costs; (2) $\beta_2 < 0$ because consumers should be risk averse; and (3) $\beta_3 = 0$ because financial attributes have no direct effect on HU under AG's assumption that μ_{ij} and σ_{ij}^2 are the only mo-

ments of the *ex post* cost distribution that consumers should care about. AG interpret violations of these restrictions as evidence that consumers made optimization mistakes, as opposed to evidence of omitted variables, model misspecification, measurement error, or finite sample bias.

We replicate AG's model (AG Table 1 column 3) using our CMS data for 2006. Table 4 reports AG's estimates in column 1 and our estimates for their model in column 2. Comparing the two columns illustrates that we reproduce AG's three main findings: (i) the coefficient on premium is approximately five times larger than the coefficient on OOP costs; (ii) the negative coefficient on variance is statistically insignificant; and (iii) coefficients on financial characteristics, such as gap coverage, are nonzero.[31] Column 3 is the same as column 2 except that we replace AG's indicators for PDP contract id with indicators based on the brand name visible to consumers.[32] This increases the pseudo R^2 and reduces the premium-to-OOP ratio from 5.5 to 3.7. It also changes the signs on two of the five financial variables.

B. Replication of AG's Welfare Calculations and Further Analysis

In the second to last row of Table 4, we replicate AG's calculation of the partial equilibrium welfare gain from a hypothetical intervention "that would make individuals full informed and fully rational" (p. 1208). For the purpose of estimating welfare losses AG assume that consumers' HU function is

$$HU_{ij} = (p_j + \mu_{ij})\hat{\alpha} + \sigma_{ij}^2\tilde{\beta}_2 + q_j\hat{\gamma}, \tag{5}$$

where $\hat{\alpha}$ and $\hat{\gamma}$ are estimates from a logit model of equation (4) and the coefficient on variance $\tilde{\beta}_2$ is chosen to produce a coefficient of absolute risk aversion of 0.0003 as in AG (p.1208). Welfare is calculated by using (5) to measure the compensating variation generated by switching from the plan that maximizes each consumer's DU function in (4) to the plan that maximizes the HU function in (5) that AG have chosen for that consumer. The calculation is explained in our appendix. The most important detail is that AG assume that the Type I EV errors in (4) do not enter the HU function in (5). That is, AG interpret nonzero values for ε_{ij} as idiosyncratic optimization mistakes made by consumers.[33]

[31] Appendix Table A5 demonstrates that we also replicate the pattern of results in AG's more parsimonious specifications.
[32] Appendix Table A6 reports the results from the model in column 3 separately by year for 2006-2010. Table A7 shows that the premium-to-oop coefficient ratio in AG's most parsimonious specification is close to 1 in 2008-2010.
[33] AG refer readers seeking an explanation of their welfare calculations to Appendix D of the earlier NBER version of their paper. That appendix includes the $\varepsilon_{ij} \equiv 0$ assumption and the body of their NBER paper reports the same 27% welfare loss. Abaluck and Gruber (2013) make the same assumption that $\varepsilon_{ij} \equiv 0$.

To illustrate how AG's interpretation of ε_{ij} affects their welfare measure, the last row of Table 4 reports the welfare loss under the common interpretation of $\hat{\varepsilon}_{ij}$ as a combination of misspecification, measurement error, and tastes for unobserved product attributes, in which case $\hat{\varepsilon}_{ij}$ is assumed to enter HU. This calculation isolates the expected welfare loss from the three mistakes that AG emphasize. The difference between the last two rows isolates the component of the welfare loss due to AG's assumption that $\hat{\varepsilon}_{ij}$ represents consumer mistakes. When we set $\varepsilon_{ij} \equiv 0$ in our replication of AG's model in column 2, the average welfare loss is within one percentage point of the statistic they report: 27% of plan costs ($366). In contrast, when we allow $\hat{\varepsilon}_{ij}$ to enter utility the average welfare loss declines to 9% of plan costs, or $125 per person per year. Hence, approximately two thirds of the welfare loss that AG report is due to their assumption that $\hat{\varepsilon}_{ij}$ represents consumer mistakes. The share due to $\hat{\varepsilon}_{ij}$ rises to 81% in column 3 when we replace the contract id dummies used by AG with dummies for the insurance brand names seen by consumers.

In a wide variety of empirical contexts, logit models require $\hat{\varepsilon}_{ij} \neq 0$ for some consumers' observed choices to maximize the analyst's specification for utility (e.g. the markets for cars, houses, labor, health care). The decision to interpret $\hat{\varepsilon}_{ij} \neq 0$ as an optimization mistake in these cases predetermines that, all else constant, the average consumer will be found to make welfare-reducing mistakes. For example, in column 4 we estimate equation (5) after adding an error term. Despite this model precluding all three of AG's explicit consumer mistakes, the model still generates large losses under AG's welfare measure due to nonzero values of $\hat{\varepsilon}_{ij}$.[34]

C. Testing Consistency between Parametric and Nonparametric Results

To further investigate how assumptions about the shape of utility affect conclusions about choice inconsistencies, we estimate AG's model on the 25% of people who chose plans on their efficiency frontiers in cost-variance space. By definition, these choices are consistent with minimizing costs or with people maximizing utility by "choosing plans with higher mean expenditure to protect themselves against variance in expenditure" (AG p.1190). As shown in the last column of Table 4, AG's model and welfare measure continues to produce evidence of choice inconsist-

[34] The average welfare loss is larger in column 4 than in columns 1-3 primarily because the premium coefficient becomes smaller when we impose AG's constraints. The results also show that allowing the model to incorporate AG's three explicit consumer mistakes only improves model fit marginally, increasing the pseudo R^2 from 0.36 to 0.37.

encies. While the variance coefficient is negative and significant, the premium-to-OOP coefficient ratio still exceeds one (1.5) and all of the coefficients on financial plan attributes are still nonzero. Moreover, the welfare loss from these violations (10% of costs) is about as large as our estimates for the full sample in column 3. This shows that AG's evidence of welfare reducing mistakes is primarily identified by their assumption about the parametric form of utility, not by consumers making inconsistent choices by choosing plans off the efficiency frontier in cost-variance space.

D. Caveats to Parametric Tests of Utility Maximization

The research design that AG use to test whether consumers make welfare-reducing mistakes relies on two general principles. First, there can be no omitted variables. AG addressed this by stating, "we observe and include in our model all of the publicly available information that might be used by individuals to make their choices" (p.1194). Second, the analyst must know the true parametric forms of consumers' utility functions. AG addressed the possibility that their model could be misspecified by noting that two of their results are robust to several alternate specifications. They report 17 sets of estimates in their paper, describe robustness checks not shown in the paper, and devote an appendix to exploring other utility functions (e.g. CRRA vs. CARA). Nevertheless, these exercises do not validate AG's methodology for identifying choice inconsistencies: AG's 17+ specifications represent what Leamer (1983 p. 38) calls a "zero volume set in the space of assumptions." In other words, AG's robustness checks collectively represent an infinitesimally small share of the specifications for utility that are consistent with basic axioms of consumer preference theory. Of course, models are meant to abstract from reality. Our point is not that AG's model is less than perfect. Our point is that omitted variables, measurement error and misspecification of consumers' utility functions can be easily misinterpreted as optimization mistakes when common positive models are instead used as normative benchmarks as in AG. Hence a critical step in relying on this approach to assess the quality of consumer decision making is to test the validity of the chosen parametric specification.

V. Testing Parametric Specifications for Utility

Let β denote a parameter vector satisfying AG's restrictions on HU in (5): $\beta = [\beta_1, \beta_2, \beta_3]$ and let $\hat{\beta}_u$ denote an unrestricted estimate for β from AG's DU function in (4) for consumers in

market u. The difference between the two vectors can be written as

$$\hat{\beta}_u - \beta = f(g(z_u, a_u), \xi_u), \tag{6}$$

where the hat indicates that $\hat{\beta}_u$ is unrestricted and $f(\cdot)$ is a vector of functions. The sub-function $g(z_u, a_u)$ is a "consumer mistake function" that explains how optimization mistakes are caused by the two mechanisms that AG emphasize: complexity of the PDP menu, described by z_u, and the distribution of cognitive ability among consumers in the market, described by a_u (AG p. 1183-1184, 1209). The last term inside $f(\cdot)$ represents misspecification of the DU function, ξ_u. By attributing the difference between $\hat{\beta}_u$ and β to consumer mistakes, AG implicitly assume that $\xi_u = 0$. Our concern is that the difference between $\hat{\beta}_u$ and β could instead be caused by model misspecification.[35] Therefore, we design three ways to test the hypothesis that $\xi_u = 0$.

A. Test 1: Do Placebo Characteristics Appear to Affect Consumers' Decisions?

Our first test is a falsification test of AG's finding that consumers mistakenly allow redundant financial plan characteristics to affect their enrollment decisions. AG's interpretation of the non-zero coefficients on financial characteristics in the logit model as evidence of consumer mistakes is based on their assertion that their models have no omitted variables (p.1194). Our concern is that despite AG's best efforts, and the improvements we have made to the data, the estimated effects of the financial characteristics may still be driven by correlation with omitted measures of PDP cost, risk protection, and quality.

To provide an opportunity to falsify this hypothesis we replace c_j in equation (4) with $\tilde{c}_j$, where $\tilde{c}_j = [c_j, placebo_j]$. Ideally, the placebos should be correlated with premia and OOP spending, just like the financial characteristics in c_j. Unlike c_j, the placebos cannot be observed by consumers so that $placebo_j$ cannot directly affect consumers' choices. Under these conditions, the following restriction should hold:

$$\hat{\beta}_{4,u}/\hat{\alpha}_u = 0 \; \forall \; u, \tag{7}$$

where $\hat{\beta}_{4,u}$ is the estimated coefficient on $placebo_j$. A violation of this restriction would be evidence that the model is misspecified in ways that make it vulnerable to finding evidence of consumer biases where none exist.

[35] Given the large size of our CMS sample, we abstract from the potential effects of finite sample bias.

We create placebos from each plan's three digit identifier (ID). These IDs were developed by the CMS contractor BUC-CANEER using an encryption process. The IDs vary across plans and brands but are themselves meaningless and not seen by consumers. The full set of characters in the IDs is {8, 9, D, d, e, k, l, o, r, x}. Each character can appear up to three times in an ID code. Our placebos are counts of the number of times each character appears in each plan's ID. Like the financial characteristics, the placebos are mildly correlated with premia and OOP costs because they and the encrypted ID codes all vary systematically across plans and brands. For example, the correlation between OOP cost and full gap coverage is -0.05 compared to 0.04 for OOP cost and x-count and -0.01 for OOP cost and r-count.[36] Finding that the coefficients on x-count and r-count are zero would build confidence in AG's conclusion that consumers' choices are in fact influenced by financial attributes.

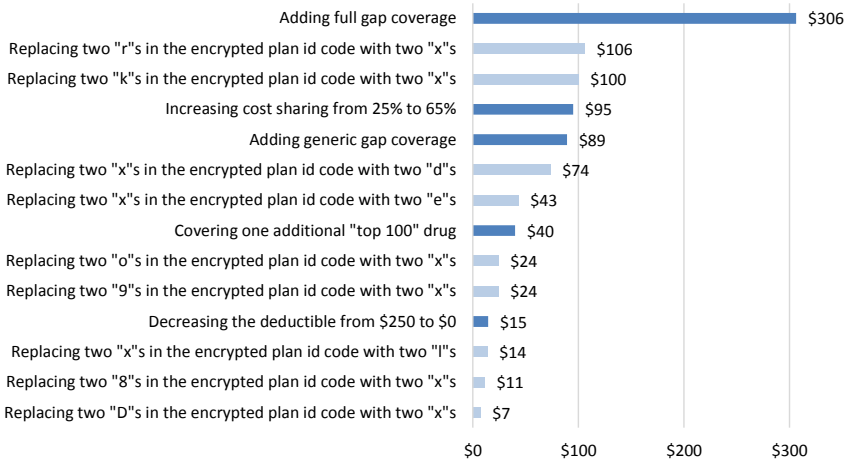


FIGURE 3: IMPLIED WILLINGNESS TO PAY FOR FINANCIAL AND PLACEBO PLAN ATTRIBUTES

Our estimates for $\hat{\beta}_{4,u}/\hat{\alpha}_u$ are statistically different from zero for every alphanumeric character.[37] We assess economic significance by calculating the WTP for changes in the financial and placebo attributes in 2006 using the same measures of WTP reported by AG (p.1198). To remain in-sample, we calculate the WTP for replacing two of one character with two of another, yielding WTP measures that are comparable to AG's measures of WTP for non-marginal changes in financial attributes. Under the hypothesis that AG's model is correctly specified the implied WTP for placebos should be closer to zero than the WTP for real financial attributes. Yet this is not the case. Figure 3 shows that all but one of the financial attributes have WTP measures that are ex-

[36] We report correlation coefficients in appendix Table A8.
[37] The model results are reported in Appendix Table A9.

ceeded by WTP for some of the placebos. For example, the DU coefficients imply that consumers are willing to pay \$106 to replace two r's with two x's in the plan ID. This measure is 20% larger than the implied WTP for generic gap coverage and seven times larger than the WTP for decreasing the deductible from \$250 to \$0. AG's model implies similarly large WTP measures for substitution patterns of other placebo attributes besides r's and x's.[38] These results show that consumers' PDP choices appear to be influenced by fake plan attributes in an economically significant way, with magnitudes similar to the WTP measures for real financial attributes.[39] This falsification test casts doubt on whether consumers are really making PDP choices based on a plan's cost sharing, generic gap coverage, coverage of the top 100 drugs, and deductible.[40]

B. Test 2: Are the Utility Parameters Stable Across Markets?

Our second test investigates whether between-market variation in the signs and magnitudes of estimated mistakes can be explained by between-market variation in the factors that AG hypothesize to be the sources of mistakes. Let $\widehat{\mathrm{B}}_u$ denote a normalized vector of estimates for the β parameters, where each element of the vector is divided by the estimated marginal utility of income; e.g. $\widehat{\mathrm{B}}_{1,u} = \hat{\beta}_{1,u}/\hat{\alpha}_u$. The purpose of this normalization is to enable comparison across markets. If observed violations are due to choice complexity and consumers' lack of cognitive ability as AG hypothesize (p.1183-1184, 1209), and we estimate the model in two separate markets, u and v, in which consumers with the same cognitive abilities face different PDP menus that are equally complex, then a well-specified model will yield two separate consistent estimates for the same normalized parameter vector:

$$\widehat{\mathrm{B}}_u = \widehat{\mathrm{B}}_v \; \forall \, u, v : \; a_u = a_v \text{ and } z_u = z_v. \tag{8}$$

This restriction follows directly from (6). Similarly, under AG's hypothesis, we would expect the magnitudes of violations to be smaller in regions where people have greater cognitive ability and choose from simpler menus. To test these hypotheses we exploit the way CMS divides the nation

[38] The results in the figure can be combined to evaluate the implied WTP for substitution of any placebo attributes, e.g. the results imply a WTP of \$114 for replacing two replacing two k's with two l's and a WTP of \$98 for replacing two o's with 2 d's.

[39] Abaluck and Gruber provided us with results for a slightly different approach to the placebo test, which for the sake of transparency we report in Table A10. In addition to yielding similarly large implied WTP for placebo attributes, their analysis yields implied WTP for financial attributes that differ widely from their original estimates and from our estimates.

[40] The relatively larger WTP for full gap coverage could mean that consumers find this plan feature inherently attractive, or it could mean that full gap coverage is more highly correlated with omitted variables, including observed but misspecified attributes, than our placebos. Some of the possible omitted variables include higher moments of the cost distribution as well as aspects of risk protection not captured by the AG variance measure. As one example, having gap coverage helps to smooth expenses across months, which may be important for retired people living on fixed monthly incomes.

into regions with distinct PDP menus. We estimate the model separately for 32 of these regions for 2006, excluding Alaska and Hawaii due to small samples. Holding cognitive ability and menu complexity fixed, instability of the coefficients could indicate that the model is misspecified in ways that vary from market to market ($\xi_u \neq \xi_v \neq 0$) such as latent heterogeneity in preferences and unobserved PDP quality.[41]

In every region, our estimates violate at least two of AG's three parametric restrictions, but the violations have inconsistent signs and magnitudes. Our data include between 1,462 and 42,441 people per region, so most of our estimates are statistically precise. Yet the estimated parameters are highly unstable. The ratio of the premium coefficient to the OOP coefficient provides a leading example. Figure 4 maps this ratio for each region. Focusing on the 24 markets in which the estimated marginal utility of income is positive and statistically different from zero, the premium-to-OOP ratio ranges from 1.1 in region 25 (IA, MN, MT, NE, ND, SD, WY) to 12.3 in region 2 (MA, CT, RI and VT). Taken literally, these results imply that the average consumer in region 25 would pay about $1 in higher OOP costs to reduce their plan premium by $1, whereas the average consumer in region 2 would pay about $12 in OOP costs to reduce their premium by $1.[42]

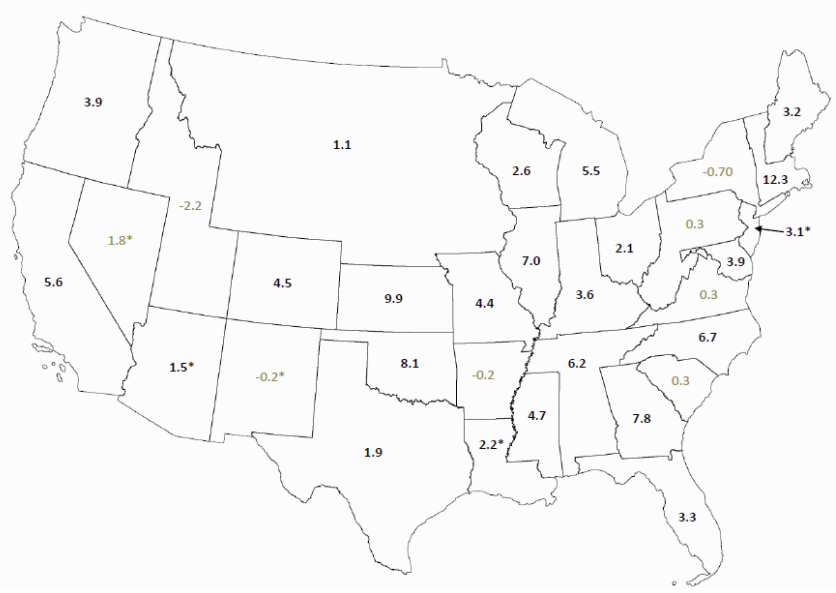


FIGURE 4—RATIO OF PREMIUM-TO-OOP COEFFICIENTS IN 2006, BY CMS REGION

Note: The figure reports the premium-to-OOP coefficient ratio obtained by estimating region-specific models equivalent to the national model in column 3 of Table 4. In regions with light-shaded numbers, we fail to reject the null hypothesis that the marginal utility of income is negative at the 5% level. Asterisks indicate that the premium-to-OOP ratio is statistically indistinguishable from 1 at the 5% level.

[41] This can also be seen from the axioms underlying the proof of McFadden's lemma 6 (McFadden 1974).
[42] Figure A3 shows that the range across regions widens substantially if we replace our brand name dummies with the contract ID proxy for brand used by AG. In that case, the premium-to-OOP ratio ranges from 1.2 in region 25 to 76.1 in region 4 (NJ).

All of the other parameter ratios are similarly unstable, as are the corresponding welfare losses. Most of the ratios span orders of magnitude and many include sign changes. The top part of Table 5 illustrates this instability by comparing results from our national model to results for the five largest CMS regions, which collectively represent 36% of the national sample. For example, under the interpretation that AG give to their national results the ratios imply that people in regions 11 and 25 mistakenly prefer plans with more cost sharing whereas people in regions 4, 17, and 22 mistakenly prefer plans with less cost sharing. Likewise, people in regions 4, 11, and 22 appear to be mistakenly attracted by generic gap coverage whereas people in regions 17 and 25 appeared to be mistakenly repelled by generic gap coverage.[43] The bottom part of Table 5 reports proxy measures for PDP menu complexity and the average consumer's cognitive ability. These measures do not appear to explain the variation in parameter ratios and welfare measures across the five regions.

Parameter ratios and welfare measures are similarly unstable across the other 27 CMS regions as evident from the complete region-by-region results and summary statistics in Tables A13 and A14. To summarize these results, we use the 24 regions with statistically significant positive estimates for the marginal utility of income to estimate a meta-regression of the conditional relationship between the premium-to-OOP ratio and proxy measures for menu complexity and cognitive ability,

$$\hat{\alpha}_u / \hat{\beta}_{OOP,u} = \varphi + \delta\, z_u + \omega a_u + \psi, \tag{9}$$

where the dependent variable is the premium-to-OOP ratio for region u, the z_u vector includes the number of plans, the number of brands, the number of plans with gap coverage and the number of plans with zero deductible, and a_u includes the mean consumer's age and the percent of consumers with dementia (including Alzheimer's disease). Our results fail to reject the joint hypothesis that $\delta = \omega = 0$ at the 10% level.[44] We obtain similarly insignificant results when we repeat the regression using AG's welfare measure as the dependent variable, regardless of whether

[43] We also observe similar instability in model parameters and welfare measures in Table 5 when use AG's proxy measures for brand dummies and when we limit the sample to white females under the age of 80 who have not been diagnosed with Alzheimer's disease, dementia, or depression. Appendix Tables A11 and A12 report these results.

[44] Measurement error introduced by using econometric estimates as the dependent variable in (9) may bias the standard errors downward, reinforcing our finding of statistical insignificance. As a robustness check on this finding, we repeat estimation of AG's model in (4) after adding terms that interact z_{iu} and a_{iu} with expected OOP expenditures (μ_{ij}), and then use the resulting estimates to predict the premium-to-oop ratio for each region. These predictions are reported in the last row of tables A13 and A14. Consistent with our findings from the meta-regression, there is little variation in the predicted ratios relative to what we observe in the region-specific estimates. Results from the model with interactions are reported in Table A16.

we allow ε_{ij} to enter utility. Results from the meta-regressions are reported in Table A15.

TABLE 5—SPATIAL VARIATION IN PARAMETER RATIOS, PDP MENUS, AND CONSUMERS, 2006

	United States	region 25	region 17	region 11	region 22	region 4
Estimated parameter ratios						
premium / OOP	3.7	1.1	7.0	3.3	1.9	3.1
	(0.0)	(0.0)	(0.2)	(0.2)	(0.1)	(1.1)
variance / premium	0.0	0.0	0.0	0.0	-0.8	0.6
	(0.0)	(0.0)	(0.0)	(0.0)	(0.2)	(0.5)
deductible / premium	-0.1	-3.6	0.0	0.1	-0.5	-7.5
	(0.0)	(0.2)	(0.0)	(0.0)	(0.1)	(2.9)
full gap / premium	-2.9	-2.9	-3.5	-3.3	0.6	0.5
	(0.0)	(0.1)	(0.1)	(0.1)	(0.5)	(1.9)
generic gap / premium	-0.9	0.8	1.6	-0.1	-2.8	-8.8
	(0.0)	(0.6)	(0.1)	(0.1)	(0.3)	(2.9)
cost share / premium	-1.7	-3.2	3.8	-10.9	5.0	48.1
	(0.1)	(0.9)	(0.2)	(0.6)	(0.6)	(18.0)
top 100 / premium	-0.4	-0.4	-0.4	-0.5	-0.7	-0.5
	(0.0)	(0.0)	(0.0)	(0.0)	(0.0)	(0.2)
Welfare loss (% of costs)						
ε ≡ 0	39	92	18	38	62	110
ε is unrestricted	7	24	9	9	3	68
PDP Menu						
# plans	43	41	42	43	47	44
# brands	19	23	18	20	22	20
# plans w/ gap coverage	7	7	6	8	6	6
# plans w/ no deductible	26	23	25	25	27	25
Consumers						
mean age	75	76	78	76	76	78
% with dementia	7.4	6.2	8.2	7.8	8.6	9.4
% off cost-var frontier	75	74	79	76	72	82
% off cost-var-brand frontier	26	34	15	15	18	8
mean potential savings	521	621	469	543	517	606
number	464,543	46,997	37,939	30,138	29,387	24,162

Note: The top portion of the table reports ratios of parameter estimates from our baseline model (Table 4 column 3) for selected CMS regions with standard errors in parentheses. Region 25 includes Minnesota, Montana, Nebraska, North Dakota, South Dakota, and Wyoming. Region 17 is Illinois; Region 11 is Florida; Region 22 is Texas; and Region 4 is New Jersey. Mean potential savings is the amount of money the average consumer would have saved by switching to their lowest cost alternative.

In summary, the parameters of AG's behavioral model vary greatly across CMS regions and these fluctuations produce order-of-magnitude differences in AG's measure of lost consumer welfare. These differences are not explained by regional variation in PDP menu complexity and proxy measures for the average consumer's cognitive ability. For this evidence to be interpreted

as supporting AG's hypotheses about consumer mistakes, the average consumers in different CMS regions would have to be reacting to similarity complex PDP menus by making mistakes that vary greatly in their severity (e.g. the magnitude of the premium-to-OOP ratio) and by making contradictory types of mistakes (e.g. changing signs of coefficients on financial attributes). While enrollees may differ in the extent to which they make fully informed decisions, we would expect this heterogeneity to be driven primarily by variation across consumers *within* CMS regions. Hence, we interpret the unexplained between-region variation in the parameters of AG's behavioral model as evidence of potential model misspecification.

C. Test 3: Does Allowing for Mistakes Improve our Ability to Predict Choices?

Our last test builds on best practices in validation techniques to judge the usefulness of structural models for informing policy (e.g. McFadden et al. 1977, Keane and Wolpin 2007). In his summary of the literature, Keane (2010) recommends evaluating the relative performances of competing models of utility maximization based on two criteria: (i) their ability to reproduce important features of the data on which they were estimated; and (ii) their ability to make accurate out-of-sample predictions for how agents will behave in different choice environments. We apply this logic in a new way and test whether allowing for specific mistakes improves a structural model's ability to predict people's choices. Specifically, we compare the accuracy of predictions made by AG's DU model that allows for consumer mistakes (equation 4) with AG's less flexible but otherwise identical expected utility maximization (EUM) model that maintains the assumption that consumers do not make any of the three explicit mistakes (equation 5). The two competing models correspond to columns 3 and 4 of Table 4, respectively. This comparison is relevant for policy because AG's EUM model assumes no welfare loss from the three mistakes, i.e. DU $\equiv$ HU (setting aside AG's interpretation of the error terms) whereas AG's DU model allows DU $\neq$ HU).

Because AG's DU model is more flexible it yields a better fit to the data on which it was estimated in the sense that it has a higher pseudo R-squared. The estimated coefficients also allow us to reject the nested EUM model at the 1% level of statistical significance based on a Wald test. Yet this rejection could be driven by model misspecification, such as correlation between the financial plan characteristics and omitted measures of PDP cost, risk protection, and quality. In this case AG's simpler EUM model could yield more accurate estimates for people's PDP

choice process and, therefore, more accurate predictions for how people would react to different PDP choice environments. This is the reason for comparing the two models based on the accuracy of their out-of-sample predictions in a choice environment that differs from the in-sample environment. As Keane and Wolpin (2007) explain, in the absence of a controlled or natural experiment an effective approach to assessing the out-of-sample predictive power of competing structural models is to use *nonrandom* holdout samples. The ideal holdout sample is nonrandom in the sense that it should describe a market in which consumers choose among products with combinations of attributes that differ from those of the products found in the market used to estimate the model. The model that better predicts how consumers behave under choice environments that *differ* from the environment under which the model was estimated would also be expected to yield more accurate estimates for consumer preferences and, therefore, to make better predictions for the welfare effects of policies that alter the choice environment along similar dimensions.[45]

Our validation test is based on s_u, a policy-relevant statistic describing market u that can be observed in the data as well as predicted as a function of model parameters. The better performing model is the one that yields a lower value for

$$\left|\hat{s}_u\left(\hat{\alpha},\hat{\beta},\hat{\gamma}\right) - s_u\right|, \tag{8}$$

where $\hat{s}_u\left(\hat{\alpha},\hat{\beta},\hat{\gamma}\right)$ is a prediction for s_u based on the AG's DU or AG's EUM model. Following standard practice, we call the predictions "in-sample" if they are based on estimates for α, β, and γ from market u and "out-of-sample" if they are based on estimates from a different market (i.e. a different CMS region). This is a high-power test in the sense that it is stacked against the less flexible EUM model. Comparing the two models in Table 4 illustrates that AG's benchmark EUM model constrains seven parameters of AG's DU model (the parameters of the mean and variance of OOP cost and the five financial attributes). The additional flexibility of AG's DU model increases its potential to reflect behaviors present in the data.

We use each model to predict seven outcomes broadly relevant to consumers and policymakers. The first is the share of consumers with some form of coverage in the gap. This clearly matters to policymakers given the scheduled transition to mandatory gap coverage in 2015. The sec-

[45] This validation test differs from our regional comparison of AG's DU parameters in Table 5 in two ways. First, it is unclear a priori how differences in DU parameters across two regions will affect the accuracy of predictions for policy relevant statistics that are highly nonlinear functions of those DU parameters. Second, the objective here is to compare AG's DU and EUM models, conditional on the fact that both use estimates from one region to predict outcomes for other regions.

ond is the share of consumers choosing plans that are dominated in the sense that they lie off the frontier in cost-variance-brand space. These consumers could have saved money by choosing a lower-variance plan offered by their chosen brand. Both consumers and policymakers are likely interested in reducing within-brand overspending. Third, we predict the share of consumers who choose the lowest cost plan within their chosen brand, conditional on their chosen brand offering multiple plans. This provides another proxy measure of ex post decision making quality. The next two statistics describe the median consumer's expenditures. We predict total expenditures (premium + OOP) as well as within-brand overspending on dominated plans. The last two statistics are the Herfindahl-Hirschman Index (HHI) and the market share of the most popular brand, commonly used by regulators as measures of the degree of market competition.

To implement the validation test we select a single region as the estimation sample and use the resulting parameters to predict choices in every other region. To be comprehensive we repeat this procedure 32 times, iteratively using each region as the estimation sample, and then analyze the combined results from the 32 in-sample predictions and all 992 possible out-of-sample predictions. To calculate an overall measure of model performance, the in-sample absolute prediction errors are weighted by the share of the CMS sample in each estimation region and the out-of-sample predictions are weighted by the share in the holdout regions.[46] The top half of Table 6 shows the results from models that rely on the CMS star ratings to proxy for plan quality, while the lower half is from models that use the brand indicators.[47]

The first column in Table 6 reports the national values in the data while the next two columns show the in-sample fit of AG's DU and EUM models in terms of mean absolute deviation. The DU model is constrained to reproduce the in-sample share of consumers with gap coverage, and when brand indicators are used to proxy for quality both the DU and EUM models are constrained to match the two market concentration measures. Among the remaining statistics, the DU model usually does better as expected due to its greater flexibility.

[46] In their comments on an earlier draft of this article, Abaluck and Gruber suggested using 31 regions as the estimation sample to predict choices in the single remaining holdout region, and then repeating this exercise for each of the 32 regions. This is not an ideal test of a model's out-of-sample predictive power. In the PDP context it is very similar to an in-sample test in the sense that the set of plans in the 31 estimation regions often comes very close to spanning the set of plans in the hold out region. This reduces the power of the test to reject the more flexible AG model in favor of the EUM hypothesis. Nevertheless, we implement this test and report results in Table A19. AG's EUM model does as well or better than their DU model on 11 out of 14 measures.

[47] Many brands are not present in any given region. In those cases we assign brand indicator coefficients via an auxiliary regression. Specifically, we regress the in-sample brand indicators on their CMS star ratings and use the estimated coefficient to impute any out-of-sample brand indicators that are not present in the estimation sample.

TABLE 6— NONRANDOM HOLDOUT SAMPLE TESTS OF MODEL VALIDATION, 2006

		In-sample fit		Out-of-sample fit	
		AG's DU	AG's EUM	AG's DU	AG's EUM
	data	|model error |		|model error |	
		Using CMS Star Ratings for Quality			
Percent of consumers choosing:					
gap coverage	13	0	6	7	6
dominated plan	20	3	4	7	6
min cost plan within brand	52	6	6	8	6
Median consumer expenditures ($)					
premium + OOP	1,255	13	37	76	65
overspending on dominated plans	0	65	53	64	50
Market concentration					
Hirfindahl-Hirschman index	25	9	14	9	13
market share of top brand	37	9	17	11	16
		Using Brand Indicators for Quality			
Percent of consumers choosing:					
gap coverage	13	0	4	7	7
dominated plan	20	1	3	7	7
min cost plan within brand	52	5	8	11	10
Median consumer expenditures ($)					
premium + OOP	1,256	13	18	75	73
overspending on dominated plans	0	74	66	82	66
Market concentration					
Hirfindahl-Hirschman index	25	0	0	10	10
market share of top brand	37	0	0	14	13

Note: | Model error | refers to the mean absolute deviation between the regional-level model predictions and data, weighted across regions by the number of people in the sample in the region. The results are based on every possible pairwise combination of regions in 2006 except that they exclude regions 33 and 34 (HI and AK) due to small samples, and the lower half also excludes region 26 (NM) because the generic gap indicator is collinear with the brand indicators for that region. Thus the values in the top half are based on the results from all 992 of the possible regional out-of-sample predictions while those in the lower half are based on 930 of them.

The last two columns of Table 6 summarize each model's out-of-sample predictive power. We expect both models to perform worse out of sample, but the DU model has a relatively larger reduction in predictive power. Importantly, the EUM model yields predictions that are as close or closer to the data for 12 of the 14 measures. The fact that AG's DU model does better at predicting the two measures of market concentration when star ratings are used to proxy for quality could be because the additional seven parameters of the DU model give it greater flexibility to capture latent features of PDP quality when brand indicators are omitted.

Because using brand indicators for quality requires imputing coefficients for brands that are offered in the prediction region but not the estimation region, we perform a robustness check in which we limit the validation exercise to region pairs for which one region's brands is nested in

the other's. This allows inclusion of brand indicators without imputation. The results of this exercise, reported in Table A17, affirm the national results in the lower half of Table 6, as do results from repeating the exercise in Table 6 using the root mean square error as the test statistic in lieu of the mean absolute deviation (Table A18). Altogether, these results demonstrate that AG's benchmark model without welfare reducing mistakes leads to predictions for policy-relevant statistics that typically match the data on people's PDP choices about as well, and often better, than predictions from AG's model with optimization mistakes.

VI. Conclusion

Neoclassical and psychological models of consumer behavior often make divergent predictions for the welfare effects of paternalistic policies. As Bernheim and Rangel (2009) observed, the lack of methodological consensus within economics results in analysts selecting models for policy evaluation based on ad hoc criteria that are inevitably controversial. Within this conflicted environment, researchers' assumptions about what they know about other peoples' preferences, constraints, and information have wide scope to influence the researchers' interpretations of results and their conclusions about the effects of various policies. One of our objectives for this article was to overcome this impasse by developing a broadly applicable three-step approach to evaluating the role and validity of modeling assumptions underlying conclusions about the quality of consumer decision making.

The first step is to integrate parametric and nonparametric tests of utility maximization by using internally consistent assumptions about which product attributes affect utility. The nonparametric results will reveal whether inferences based on the analyst's chosen parametric model are robust to alternative specifications for utility. If the results are not robust, then the next step is to calculate nonparametric measures of the sufficient willingness to pay for product attributes. These measures help to clarify the tradeoffs implied by the set of utility functions that *are* maximized by consumers' choices. Finally, we propose designing tests to disentangle consumer mistakes from the researcher's misspecification of their utility functions. Of particular value are head-to-head validation tests of models' out-of-sample predictive power that build on McFadden (1977) and Keane and Wolpin (2007). That is, a key step in establishing that consumers are making specific welfare-reducing mistakes is to first show that incorporating such mistakes into a model improves its predictions for how consumers react to changes in the choice environment.

In the context of Medicare Part D, our nonparametric tests revealed that Abaluck and Gruber's (2011) [AG] parametric evidence that enrollees made large welfare reducing optimization mistakes in 2006 is not robust to alternative specifications for utility. We showed that these alternative specifications imply that the median consumer must have been willing to pay at least $80 (6% of total out of pocket expenditures) in 2006 for all between-brand differences in plan quality. We also found that a simpler version of AG's model that maintains the hypothesis of expected utility maximization makes out-of-sample predictions that are typically as good, and often better, than their model with psychological biases. These empirical results cast doubt on the notion that the three mistakes in AG's model have an important influence on how people choose insurance plans. They also cast doubt on the notion that people would benefit from policies that would restrict their ability to choose insurance plans for themselves. That said, our results do not imply that everyone always makes fully informed enrollment decisions or that nobody would benefit from paternalistic policies. Rather, our results imply that evaluating the distributional welfare implications of paternalistic reforms to health insurance markets requires us to first develop a better understanding of how heterogeneous consumers obtain and process information about differentiated health insurance products with uncertain payoffs.

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SUPPLEMENTAL APPENDIX FOR ONLINE PUBLICATION

I. DERIVING ABALUCK AND GRUBER'S WELFARE MEASURES

AG begin by assuming that consumers' decisions are guided by a logit model that is linear and additively separable in PDP characteristics, as shown in (4) and repeated here for convenience.

(A1) $$\hat{u}_{ij} = \hat{\omega}_{ij} + \hat{\varepsilon}_{ij} = p_j\hat{\alpha} + \mu_{ij}\hat{\beta}_1 + \sigma_{ij}^2\hat{\beta}_2 + c_j\hat{\beta}_3 + q_j\hat{\gamma} + \hat{\varepsilon}_{ij}.$$

In contrast, the actual utility a consumer experiences from her selected PDP is instead defined by the following hedonic utility function that satisfies AG's three normative restrictions,

(A2) $$u_{ij} = \omega_{ij} + \varepsilon_{ij} = p_j\hat{\alpha} + \mu_{ij}\hat{\alpha} + \sigma_{ij}^2\tilde{\beta}_2 + q_j\hat{\gamma} + \varepsilon_{ij}, \text{ where } \tilde{\beta}_2 < 0.$$

Because AG assume the marginal utility of income is a constant revealed by $\hat{\alpha}$, a consumer's expected welfare from choosing plan *j* can be expressed as

(A3) $$E[CS_i] = \frac{1}{\hat{\alpha}} E\left[u_{ij} \middle| \hat{u}_{ij} > \hat{u}_{ik}\ \forall\ k\right].$$

PDP choice and welfare are both deterministic from the consumer's perspective. The expectation in (A3) simply reflects the analyst's inability to observe $\hat{\varepsilon}_{ij}$.

AG aim to calculate the partial equilibrium welfare gain from a hypothetical intervention "that would make individuals fully informed and fully rational" (p 1208). In other words, they want to calculate the welfare gain from a policy that would induce the consumer to choose the PDP that maximizes AG's normative utility function (A2) instead of (A1). Assuming the policy has no effect on the marginal utility of income, the welfare gain can be expressed in general terms as

(A4) $$\Delta E[CS_i] = \frac{1}{\hat{\alpha}} E\left[\max_k\{u_{ik}\} - \left(u_{ij} \middle| \hat{u}_{ij} > \hat{u}_{ik}\ \forall\ k\right)\right].$$

The analytical formula depends on the interpretation of the residual utility terms, $\hat{\varepsilon}_{ij}$ and ε_{ij}.

In appendix D of the NBER (2009) version of their paper, AG outline two different approaches to interpreting residual utility. The more conventional approach, laid out in earlier papers such as Leggett (2002), is to interpret $\hat{\varepsilon}_{ij}$ as the idiosyncratic utility from PDP characteristics that consumers observe but the analyst does not. Examples include proximity to in-network pharmacies, availability of mail-order pharmacies, individual-specific experience with the insur-

ers, coordination with spouses, disutility from prior authorization requirements, uncertainty about whether other plans will approve prior authorization requests, and so on. In this case the policy intervention has no effect on the utility residual because the same unobserved PDP attributes enter hedonic utility.[48] Thus $\varepsilon_{ij} = \hat{\varepsilon}_{ij}$.

In contrast, the approach that Abaluck and Gruber (2011, 2013) use for their published empirical analyses is to assume that the policy intervention also eliminates the utility residual: $\varepsilon_{ij} = 0$. That is, ε_{ij} itself is treated as an optimization mistake in addition to violations in the three parametric restrictions that they explicitly mention as reducing welfare (p.1208). This approach embeds at least three important assumptions. First, it assumes there are no omitted variables. The analyst must have data on every PDP attribute that affects consumers' hedonic utility. Second, it assumes (A1) and (A2) are correctly specified. The analyst must know the true parametric forms of decision utility and hedonic utility. Third, it assumes the policy intervention has no direct effect on utility. For example, the two policies suggested in AG may affect welfare due to distaste for being nudged or distaste for sacrificing control over plan choices to a surrogate decider. Together, these three assumptions are required for AG to treat $\hat{\varepsilon}_{ij}$ as an idiosyncratic optimization mistake that is eliminated by their hypothetical policy.

In the remainder of this section we derive analytical formulas for consumer welfare under each of the two approaches to interpreting residual utility. Whereas Abaluck and Gruber (2009) derive measures of baseline consumer surplus prior to any policy intervention, we derive the key statistic used in their welfare calculations (and ours)—the *change* in consumer welfare caused by the hypothetical policy "that would make individuals fully informed and fully rational".

Case 1. Residual Utility is an Optimization Mistake: $\boldsymbol{\varepsilon_{ij} \equiv 0\ \forall ij}$

In this case the analyst can calculate baseline consumer surplus for each individual by using the marginal utility of income to translate utils into dollars:

(A5) $$E[CS_i] = CS_i = \frac{\omega_{ij}}{\hat{\alpha}}.$$

After consumers are made to choose the plans that maximize AG's normative utility function the post-policy consumer surplus becomes

[48] As we point out in section IV, $\hat{\varepsilon}_{ij}$ may also reflect misspecification of the true parametric form of decision utility. In this case ε_{ij} may differ from $\hat{\varepsilon}_{ij}$ if the policy affects the marginal decision utility of one or more PDP attributes included in $\hat{\varepsilon}_{ij}$.

(A6) $$E[CS_i^*] = CS_i^* = \frac{1}{\hat{\alpha}} \max_k \{\omega_{ik}\}.$$

Hence the change in welfare generated by the hypothetical policy is

(A7) $$\Delta E[CS_i] = CS_i^* - CS_i = \frac{1}{\hat{\alpha}} \left[\max_k \{\omega_{ik}\} - \omega_{ij}\right].$$

Since $\varepsilon_{ij} \equiv 0$ the analyst can calculate actual consumer surplus instead of expected consumer surplus.

Case 2: Residual Utility Reflects Omitted Attributes $\hat{\varepsilon}_{ij} = \varepsilon_{ij}\ \forall ij$

In this case the analyst must integrate over the assumed Type I EV distribution for $\hat{\varepsilon}_{ij}$ to calculate expected consumer surplus prior to the policy. The resulting expression in (A8) depends on the standard log sum rule as well as the difference between decision utility and hedonic utility weighted by the probability of selecting each PDP (e.g. Small and Rosen 1981, Leggett 2002, Abaluck and Gruber 2009).

(A8) $$E[CS_i] = \frac{1}{\hat{\alpha}} \left[ln \sum_k e^{\hat{\omega}_{ik}} + \sum_j \left(\omega_{ij} - \hat{\omega}_{ij}\right) \frac{e^{\hat{\omega}_{ij}}}{\sum_k e^{\hat{\omega}_{ik}}} \right] + \hat{C}.$$

In the equation, $\hat{C}$ represents the constant of integration divided by $\hat{\alpha}$. It arises from the assumed Type I EV distribution for $\hat{\varepsilon}_{ij}$ and the fact that the level of utility is unknown.

The policy intervention eliminates the wedge between decision utility and hedonic utility, simplifying calculation of post-policy consumer surplus:

(A9) $$E[CS_i^*] = \frac{1}{\hat{\alpha}} [ln \sum_k e^{\omega_{ik}}] + C, \text{ where } = \hat{C} + \frac{\rho}{\hat{\alpha}}.$$

If the policy intervention has a direct effect on utility, defined here by ρ, then the post-policy constant of integration, C, differs from the pre-policy constant of integration.[49] On the other hand, if we follow AG in assuming that the policy has no direct effect on utility then $\rho = 0$ and $C = \hat{C}$. In this case, the change in expected consumer surplus is

(A10) $$\Delta E[CS_i] = E[CS_i^*] - E[CS_i] = \frac{1}{\hat{\alpha}} \left[ln \frac{\sum_k e^{\omega_{ik}}}{\sum_k e^{\hat{\omega}_{ik}}} - \sum_j \left(\omega_{ij} - \hat{\omega}_{ij}\right) \frac{e^{\hat{\omega}_{ij}}}{\sum_k e^{\hat{\omega}_{ik}}} \right].$$

[49] We assume that any direct effect of the policy on utility is additive and invariant to PDP choice so that $E[CS_i^*] = \frac{1}{\hat{\alpha}} \max_k \{\omega_{ik} + \varepsilon_{ij}\} = \frac{1}{\hat{\alpha}} [ln \sum_k e^{\omega_{ik}+\rho}] + \hat{C} = \frac{1}{\hat{\alpha}} [ln \sum_k e^{\rho} e^{\omega_{ik}}] + \hat{C} = \frac{1}{\hat{\alpha}} [ln(e^{\rho} \sum_k e^{\omega_{ik}})] + \hat{C} = \frac{1}{\hat{\alpha}} [ln(e^{\rho})] + \frac{1}{\hat{\alpha}} [ln \sum_k e^{\omega_{ik}}] + \hat{C} = \frac{1}{\hat{\alpha}} [ln \sum_k e^{\omega_{ik}}] + C.$

Equation (A10) isolates the combined welfare effect of imposing the three normative restrictions on utility that AG emphasize. In contrast, the 27% welfare gain that AG report in their conclusion is based on the calculation in (A7) that embeds their normative restrictions along with the added assumption that residual utility consists entirely of optimization mistakes. Therefore, comparing empirical results for (A7) and (A10) will reveal the extent to which AG's reported 27% potential welfare gain is driven by the particular optimization mistakes they emphasize relative to their novel interpretation of the Type I EV logit error term.

Leggett, Christopher G. 2002. "Environmental Valuation with Imperfect Information". *Environmental and Resource Economics*. 23: 343-355.

Small, Kenneth A. and Harvey S. Rosen. 1981. "Applied Welfare Economics with Discrete Choice Models." *Econometrica*. 49(1): 105-130.

II. Additional Results

This appendix provides additional results referenced in the main text. Table A1 provides an example of the difference between AG's definition for brand dummies that relies on CMS contract ID codes that are unobserved by consumers and our definition that relies on company and plan names observed by consumers. We define AARP and UnitedHealth as two distinct brands, whereas AG group one AARP plan and one UnitedHealth plan into one brand, and two AARP plans and one UnitedHealth plan into a separate brand.

Table A1—Example of the Difference between Contract ID and Brand name Dummy Variables

Plan Name	Brands #1 and #2 using:	
	contract ID	brand name
AARP MedicareRx Plan	1	1
AARP MedicareRx Plan - Enhanced	2	1
AARP MedicareRx Plan - Saver	2	1
UnitedHealth Rx Basic	2	2
UnitedHealth Rx Extended	1	2

Note: Example is from the Region 2 (CT, MA, RI and VT) in 2007.

Figure A1 reports the gap premium and gap enrollment rates for various alternative samples. Panel A shows that the divergence between AG's results and results from the CMS data widens when part-year enrollees are included in the CMS sample as they likely were in AG's sample. The remaining panels provide further evidence that people responded to how gap coverage mattered for themselves. Panel B depicts CMS region 25 which was the region with the largest number of (non-poor) PDP enrollees. It is comprised of Iowa, Minnesota, Montana, Nebraska, North Dakota, South Dakota, and Wyoming. People in these states had exclusive access to a plan with especially generous gap coverage, as seen from comparing the cost premia in panel B with that in Figure 1B. They responded by enrolling at much higher rates—up to 75% at the 98th expenditure quantile. Thus, enrollment in gap plans varied dramatically across regions with the regional rate of enrollment increasing in the generosity of coverage.

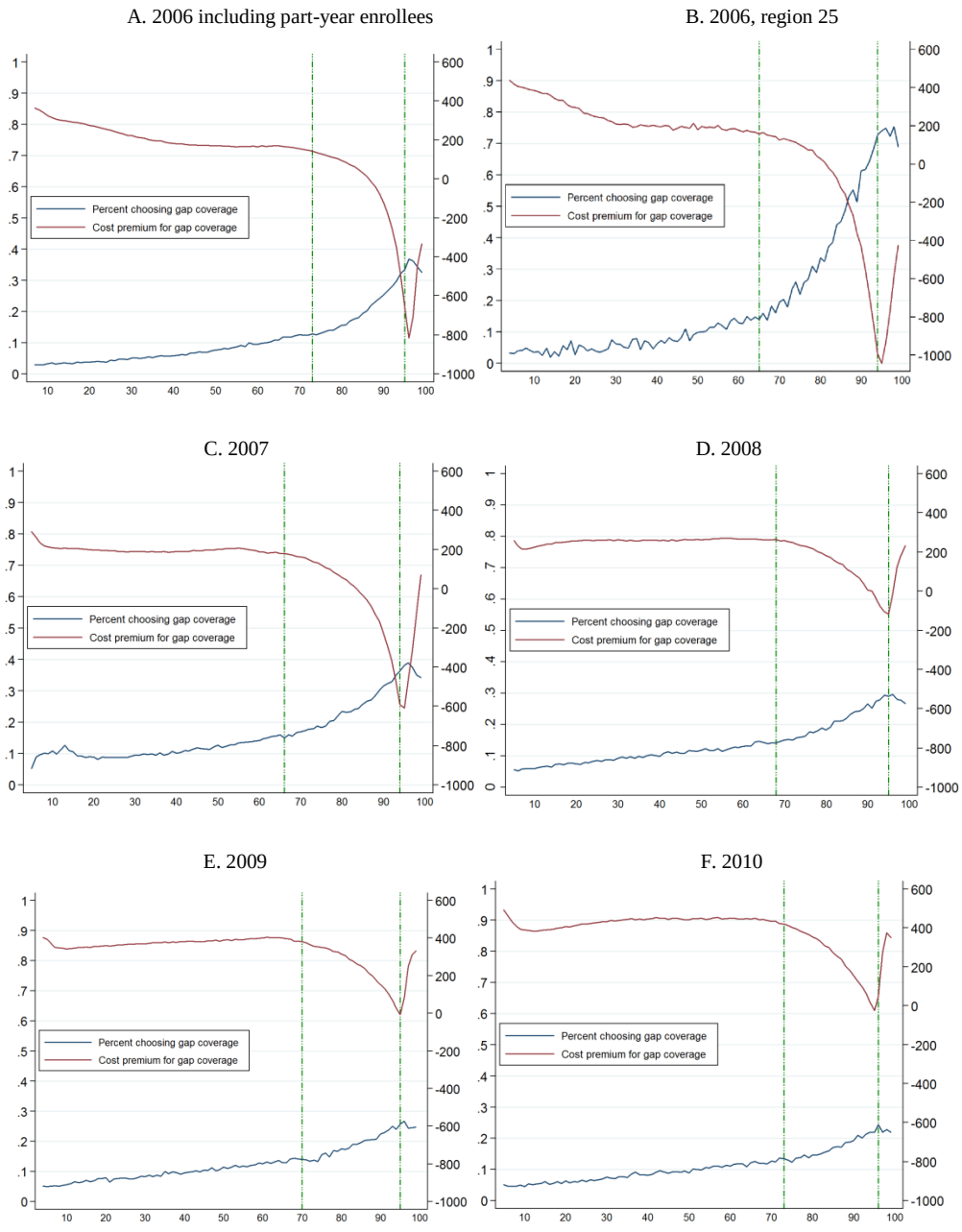

FIGURE A1: PERCENT CHOOSING GAP COVERAGE AND ADDED COST BY EXPENDITURE QUANTILE

Table A2 summarizes how the average consumer's chosen plan differs from other plans the consumer could have chosen. Each cell reports the difference between an attribute of the consumer's chosen plan and the mean value of that same attribute calculated over all of the plans that the consumer could have chosen but did not. For example, in 2006 the average consumer paid $112 less in out of pocket costs for prescription drugs under her chosen plan then she would have paid, on average, if she had enrolled in a different plan than was available to her.

TABLE A2—DIFFERENCE BETWEEN THE CHOSEN PLAN AND THE MEAN ALTERNATIVE

	2006	2007	2008	2009	2010
sample size	464,543	566,962	602,992	614,714	629,225
premium (difference in $)	-89	-73	-65	-62	-53
out of pocket costs (difference in $)	-112	-109	-164	-140	-187
variance of OOP costs (difference in percentage points)	-16	27	36	-5	-7
count of top 100 drugs covered (difference in number of drugs)	2	1	1	1	1
CMS quality index (difference in percentage points)	6	5	2	7	0

Note: Each row is calculated as the average over all people of the difference between the attribute of their chosen plan and the average of that same attribute calculated over all others plans in the individual's choice set. The unit of analysis is the individual person.

Table A3 provides the share of people in 2006 and 2007 that could reduce their spending by certain amounts by moving from their plan without gap coverage into the cheapest plan with gap coverage, or by moving from their plan with gap coverage into the cheapest plan without gap coverage.

TABLE A3—POTENTIAL SAVINGS FROM MOVING INTO OR OUT OF A GAP PLAN, 2006-2007

	2006		2007	
Percent who could save more than $X by moving	Into a gap plan	Out of a gap plan	Into a gap plan	Out of a gap plan
$100	9.9	4.2	6.6	8.6
$300	8.0	2.3	3.7	1.3
$500	4.2	0.1	2.2	0.2
$750	2.5	0.0	1.2	0.1
$1,000	1.4	0.0	0.6	0.0

Table A4 repeats the nonparametric analysis in Table 2 after replacing our brand dummies (based on company name) with AG's brand dummies (based on contract IDs).

TABLE A4— NONPARAMETRIC TEST OF CHOICE INCONSISTENCY WITH BRAND DUMMY VARIABLES DEFINED USING CONTRACT ID

	Plan attributes affecting utility	Assumption on expected drug expenditures in year *t*	% Consumers choosing frontier plans				
			2006	2007	2008	2009	2010
(1)	E[cost]	year t drug consumption	7	7	10	6	8
(2)	E[cost], var(cost)	year t drug consumption	25	24	24	26	36
(3)	E[cost], var(cost), CMS quality	year t drug consumption	35	33	46	42	45
(4)	E[cost], var(cost), brand	year t drug consumption	74	77	80	87	87
(5)	E[cost], var(cost), brand	year t or t-1 drug consumption		81	86	91	90

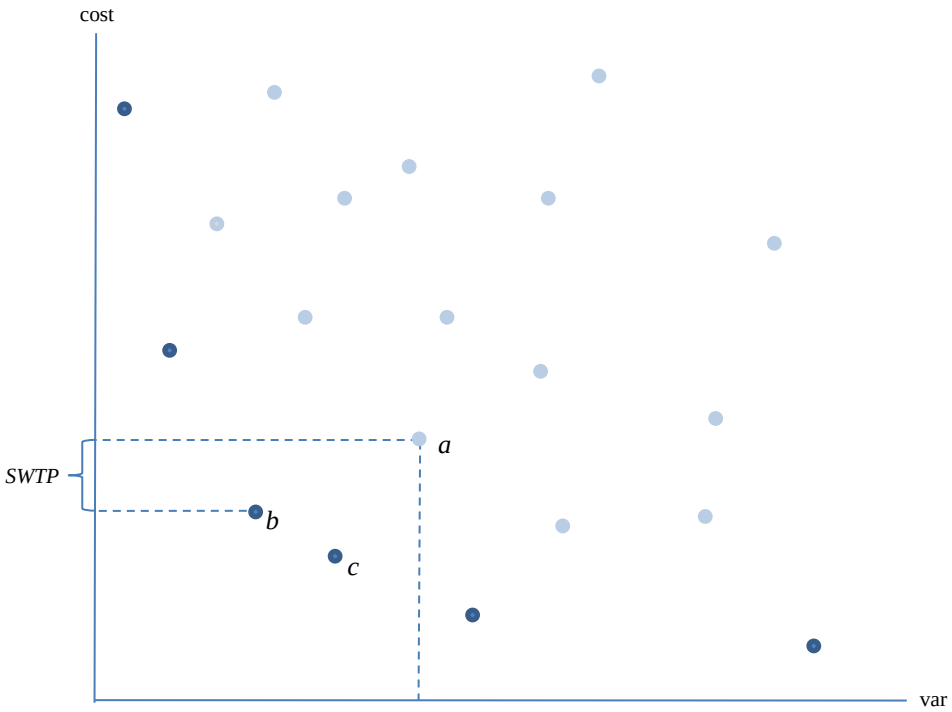

FIGURE A2—ILLUSTRATION OF THE SUFFICIENT WILLINGNESS TO PAY FOR BRAND

Figure A2 illustrates how we calculate the sufficient willingness to pay (*SWTP*) for the bundle of unobserved PDP attributes that vary from brand to brand. To begin, consider a plan, *a*, that lies on the efficiency frontier in cost-variance-brand space, where cost means the total cost (premiums plus ex post OOP drug costs) to the individual. Figure A2 is projected in cost-variance space. The dots represent other available plans. Plans on the efficiency frontier in cost-variance space have dark shading; plans off the frontier have light shading. The area inside the rectangle defined by the dashed lines that intersect at point *a* defines the portion of the efficiency frontier where other plans dominate *a* in cost-variance space. In the figure there are two such plans, *b* and *c*. We define *SWTP* as the amount of income the consumer gives up by choosing to purchase plan *a* instead of the most expensive plan on the portion of the cost-variance frontier that dominates plan *a*. Hence, $SWTP = cost_a - cost_b$.

SWTP can be interpreted as an arbitrarily close approximation to the willingness to pay for latent attributes of the consumer's preferred brand for a consumer with preferences satisfying basic axioms of consumer preference theory. To see why, suppose that plan *a* is sold by brand *A* whereas plans *b* and *c* are sold by brand *B*, and the two brands differ in a vector of latent quality

attributes, q. Consider a consumer who prefers plan *b* to plan *c* and is indifferent between plans *b* and *a* such that

$$\begin{aligned} & U(y - cost_b, var_b, q_B) \\ (A11) \qquad & = U(y - cost_a, var_a, q_A) \\ & = U(y - cost_b - SWTP, var_a, q_A), \end{aligned}$$

where the last line follows from the definition of *SWTP*. The consumer's exact willingness to pay (WTP) to switch from q_B to q_A, evaluated at the best available point on the efficiency frontier in cost-variance space, is implicitly defined by the following equation

$$(A12) \qquad U(y - cost_b, var_b, q_B) = U(y - cost_b - WTP, var_b, q_A).$$

Combining (A11) and (A12) yields the following expression

$$(A13) \qquad U(y - cost_b - SWTP, var_a, q_A) = U(y - cost_b - WTP, var_b, q_A).$$

Assuming the consumer's preferences satisfy global risk aversion and strong monotonicity it must be the case that $WTP > SWTP$. This follows from (A13) because quality is held constant at q_A. That is, in order to hold utility constant when the variance decreases from var_a to var_b, the risk averse consumer's income must be reduced. Thus, $WTP = SWTP + \varepsilon$, where ε is a positive constant that reflects the willingness to pay to reduce the variance from var_a to var_b at q_A. Finally, notice that ε can be made arbitrarily close to zero (e.g. one tenth of one cent) without violating completeness, transitivity, strong monotonicity or risk aversion. It follows that $SWTP$ provides an arbitrarily close approximation to the willingness to pay for latent attributes of the consumer's preferred brand, conditional on cost and variance, that is sufficient to rationalize the consumer's observed choice. Also notice that SWTP equals 0 for any plan on the efficiency frontier in cost-variance space, whereas no value for SWTP can rationalize the choice of a plan that lies off the efficiency frontier in cost-variance-brand space.

This logic generalizes to any number of plans on the portion of the efficiency frontier that dominates plan *a* in cost-variance space. Regardless of the thickness or sparseness of plans in attribute space, we can always set ε to be less than *e*, where *e* is an arbitrarily small positive constant. Likewise, this logic can be generalized to any assignment of plans to brands by restricting the consumer to have identical tastes for the vector of latent attributes associated with brands *B, C, D,* and so on.

Table A5 reports results from our replication of the first three columns of Table 1 in AG. The columns of the two tables are directly comparable, and both rely on AG's definition of the brand dummy variables. Models with AG's brand-state dummies (AG's column (4)) do not converge.

TABLE A5— REPLICATION OF AG TABLE 1 USING CMS DATA

	(1)	(2)	(3)
Premium (hundreds)	-0.352***	-0.430***	-0.559***
	(0.00110)	(0.00143)	(0.00249)
OOP costs (hundreds)	-0.161***	-0.102***	-0.102***
	(0.000478)	(0.000562)	(0.000578)
Variance (millions)	-0.000136***	-0.000124**	-4.86e-05
	(5.28e-05)	(5.17e-05)	(6.54e-05)
Deductible (hundreds)		-0.101***	-0.0201***
		(0.00169)	(0.00281)
full gap coverage		0.818***	1.897***
		(0.00887)	(0.0146)
generic gap coverage		0.216***	0.529***
		(0.00685)	(0.00907)
Cost sharing		-1.205***	-0.313***
		(0.0156)	(0.0248)
Number of top 100 drugs on formulary		0.184***	0.190***
		(0.00107)	(0.00153)
CMS quality index	4.217***	3.322***	
	(0.00996)	(0.0112)	
Brand dummies	No	No	Yes
Number of consumers	464,543	464,543	464,543
Number of plans	1,348	1,348	1,348
Number of states	48	48	48
Number of brands	73	73	73
Pseudo R^2	0.17	0.20	0.32

Table A6 reports results from estimating the model in column (3) of Table 4 for each year from 2006 through 2010.

TABLE A6— SENSITIVITY OF MAIN RESULTS FROM AG'S FULL MODEL TO THE STUDY YEAR

	2006	2007	2008	2009	2010
Premium (hundreds)	-0.402***	-0.144***	-0.355***	-0.505***	-0.565***
	(0.00246)	(0.00180)	(0.00161)	(0.00183)	(0.00201)
OOP costs (hundreds)	-0.108***	-0.108***	-0.214***	-0.250***	-0.194***
	(0.000573)	(0.000580)	(0.000836)	(0.000849)	(0.000734)
Variance (millions)	-6.48e-05	1.51e-06***	0.000162***	-0.124***	1.46e-05
	(6.42e-05)	(3.39e-07)	(4.70e-05)	(0.0114)	(6.02e-05)
Deductible (hundreds)	0.0510***	-0.341***	-0.233***	-0.709***	-0.713***
	(0.00279)	(0.00175)	(0.00243)	(0.00351)	(0.00247)
Full gap coverage	1.162***	0.326***	-0.136	1.503***	-1.269***
	(0.0146)	(0.0225)	(8,047)	(0.120)	(0.0422)
Generic gap coverage	0.356***	-1.065***	-0.184***	0.281***	0.328***
	(0.00893)	(0.00654)	(0.00749)	(0.00860)	(0.00934)
Cost sharing	0.683***	5.198***	1.067***	-4.636***	-5.149***
	(0.0244)	(0.0208)	(0.0378)	(0.0432)	(0.0361)
Number of top 100 drugs on formulary	0.175***	0.275***	0.181***	0.150***	0.334***
	(0.00144)	(0.00264)	(0.00245)	(0.00268)	(0.00191)
Brand dummies	Yes	Yes	Yes	Yes	Yes
Number of people	464,543	566,962	602,992	614,714	629,225

Table A7 reports results from estimating the model in column (1) of AG's Table 3 for each year from 2006 through 2010. As shown, the premium coefficient is slightly below the OOP coefficient for 2008, 2009 and 2010, and the variance coefficient has a negative sign for both 2009 and 2010.

TABLE A7— SENSITIVITY OF AG'S BASE RESULTS TO THE STUDY YEAR

	2006	2007	2008	2009	2010
Premium (hundreds)	-0.352***	-0.441***	-0.342***	-0.287***	-0.236***
	(0.00110)	(0.00111)	(0.000832)	(0.000793)	(0.000719)
OOP costs (hundreds)	-0.161***	-0.176***	-0.382***	-0.297***	-0.296***
	(0.000477)	(0.000516)	(0.000710)	(0.000680)	(0.000599)
Variance (millions)	-0.000136***	4.76e-07*	0.000252***	-1.269***	-0.000307***
	(5.28e-05)	(2.74e-07)	(3.79e-05)	(0.0128)	(5.09e-05)
CMS quality index	4.208***	5.064***	1.040***	1.332***	-0.0115***
	(0.00994)	(0.00925)	(0.00425)	(0.00318)	(0.00260)
Brand dummies	no	no	no	no	no
Number of people	464,543	566,962	602,992	614,714	629,225

Table A8 reports the correlation coefficients between placebo plan characteristics and real plan characteristics calculated across all consumer-plan observations.

TABLE A8— CORRELATIONS BETWEEN PLACEBO AND REAL PLAN CHARACTERISTICS

	premium	OOP costs	variance	deductible	full gap coverage	generic gap coverage	cost sharing	top 100 count
premium	1.00							
OOP costs	-0.10	1.00						
variance	0.00	0.01	1.00					
deductible	-0.30	0.13	0.00	1.00				
full gap coverage	0.33	-0.05	0.00	-0.13	1.00			
generic gap coverage	0.30	-0.08	0.00	-0.30	-0.06	1.00		
cost sharing	-0.33	0.18	0.00	-0.04	0.04	-0.08	1.00	
top 100 count	0.21	-0.10	0.00	-0.09	0.08	0.09	-0.43	1.00
count 8	-0.01	-0.02	0.00	-0.08	-0.03	-0.02	-0.06	0.05
count 9	0.06	-0.03	0.00	-0.07	0.08	0.06	-0.03	0.07
count D	0.06	0.00	0.00	-0.03	0.09	-0.01	0.01	-0.05
count d	-0.01	0.00	0.00	0.04	-0.05	0.11	-0.05	0.06
count e	0.01	0.03	0.00	0.08	-0.06	0.07	0.02	0.02
count k	0.07	-0.02	0.00	-0.07	-0.11	0.14	-0.13	0.07
count l	-0.06	0.01	0.00	0.04	-0.01	-0.03	0.06	-0.03
count o	0.03	0.00	0.00	0.01	-0.03	0.11	0.01	0.04
count r	0.06	-0.01	0.00	-0.03	0.05	-0.01	0.02	-0.03
count x	-0.15	0.04	0.00	0.10	0.07	-0.29	0.15	-0.14

TABLE A9— RESULTS FROM MODELS WITH PLACEBO PLAN CHARACTERISTICS

Variable	Coefficient
Count of 8's	-0.0243***
	(0.00568)
Count of 9's	-0.0523***
	(0.00604)
Count of D's	-0.0154**
	(0.00653)
Count of d's	0.158***
	(0.00701)
Count of e's	0.0929***
	(0.00737)
Count of k's	-0.215***
	(0.00450)
Count of l's	0.0301***
	(0.00631)
Count of o's	-0.0522***
	(0.00782)
Count of r's	-0.228***
	(0.00716)
Premium (hundreds)	-0.429***
	(0.00260)
OOP costs (hundreds)	-0.108***
	(0.000571)
Variance (millions)	-5.11e-05
	(6.38e-05)
Deductible (hundreds)	-0.0250***
	(0.00315)
Full gap coverage	1.314***
	(0.0151)
Generic gap coverage	0.383***
	(0.00913)
Cost sharing	1.019***
	(0.0254)
Number of top 100 drugs on formulary	0.172***
	(0.00141)
Brand dummies	Yes
Number of people	464,543

Table A10 shows results provided to us by AG regarding the placebo test. It also compares the implied WTP for actual plan financial attributes from AG's 2011 article, our replication of them, their new results and our replication of them. The results on the financial attributes show that their new results diverge from their old ones by at least $90 for 4 of the 5 attributes. In contrast, all of ours are within $65. The lower half of the Table reports the results from their placebo attributes and our replication of their placebo model. For several reasons these results are not directly comparable to the results we report, yet they yield similar qualitative insights: first, they replaced our count variables for each alphanumeric with indicator variables for any positive count of the alphanumeric. Although this makes it impossible to isolate the marginal effects comparable to the financial attributes, to facilitate comparison we replicate their approach here. Second, they stated that they normalized these placebo attributes to zero, relative to whether a "9" is present, but they did not implement any similar normalization for the financial attributes. Third, two separate values are reported for the presence of "k", and no values are reported for the presence of an "e". Nonetheless, as with our results the test implies that these imaginary attributes influence people's PDP choices in economically meaningful ways. For example, AG's results imply that people would be willing to pay $117 more for a plan with a "d", "o" and "l" in the encrypted plan ID than for a plan with three "9s", whereas they would pay $124 for a plan with three "9s" to avoid a plan with an "8", "D" and "x". Both of these, as well as a number of other combinations, exceed the magnitude estimated for of all of the real plan attributes in AG 2011 other than full gap coverage.

TABLE A10— COMPARING ESTIMATED WILLINGNESS TO PAY FOR REAL AND PLACEBO PLAN CHARACTERISTICS FROM AG 2011 BASELINE MODEL OUR REPLICATION OF AG, AND NEW RESULTS PROVIDED BY AG

	AG 2011	Our replication		AG placebo specification		Our replication	
	WTP ($)	WTP ($)	Difference from AG 2011 ($)	WTP ($)	Difference from AG 2011	WTP ($)	Difference from AG 2011
Decreasing the deductible from $250 to $0	80	15	-65	293	213	20	-60
Covering one additional "top 100" drug	50	40	-10	9	-41	39	-11
Adding generic gap coverage	50	89	39	142	92	87	37
Increasing cost sharing from 25% to 65%	80	95	15	541	461	92	12
Adding full gap coverage	300	306	6	434	134	297	-3
Encrypted plan ID includes at least one							
"d"				60		-97	
"o"				40		-32	
"k"--result 1				29		11	
"k"--result 2				-27		--	
"l"				17		-40	
"r"				16		14	
"9"				Reference		-30	
"e"				Not provided		-60	
"8"				-7		-37	
"D"				-34		-38	
"x"				-83		-77	

TABLE A11— RESULTS FROM FIVE LARGEST REGIONS DEFINING BRAND DUMMIES BASED ON CONTRACT ID

	region 25	region 17	region 11	region 22	region 4
Estimated parameter ratios					
premium / OOP	1.3	7.3	9.7	5.7	78.9
	(0.0)	(0.2)	(0.3)	(0.2)	(5.4)
variance / premium	0.0	0.0	0.0	-0.3	0.0
	(0.0)	(0.0)	(0.0)	(0.1)	(0.0)
deductible / premium	-3.0	0.5	0.3	-0.2	0.0
	(0.1)	(0.0)	(0.0)	(0.0)	(0.0)
full gap / premium	-2.9	-3.3	-4.3	-3.2	-4.4
	(0.1)	(0.1)	(0.0)	(0.1)	(0.0)
generic gap / premium	0.6	1.8	-0.9	-2.3	-1.8
	(0.5)	(0.1)	(0.0)	(0.1)	(0.0)
cost share / premium	-2.9	2.4	-2.8	5.0	1.8
	(0.8)	(0.1)	(0.2)	(0.2)	(0.1)
top 100 / premium	-0.3	-0.3	-0.2	-0.3	-0.1
	(0.0)	(0.0)	(0.0)	(0.0)	(0.0)
Welfare loss (% of costs)					
$\varepsilon \equiv 0$	101	19	25	25	19
ε is unrestricted	19	9	17	8	20
PDP Menu					
# plans	41	42	43	47	44
# brands	17	17	19	21	19
# plans w/ gap coverage	7	6	8	6	6
# plans w/ no deductible	23	25	25	27	25
Consumers					
mean age	76	78	76	76	78
% with dementia	6.2	8.2	7.8	8.6	9.4
% off cost-var frontier	74	79	76	72	82
% off cost-var-brand frontier	36	27	26	25	16
mean potential savings	621	469	543	517	606
number	46,997	37,939	30,138	29,387	24,162

Table A12 replicates the results in Table 5 after limiting the sample to white females under 80 who have not been diagnosed with Alzheimer's, dementia, or depression. See the discussion of Table 5 for additional details.

TABLE A12: RESULTS FROM MODELS IN TABLE 5 BUT WITH THE SAMPLE RESTRICTED TO WHITE FEMALES AGE<80 WITHOUT ALZHEIMER'S DISEASE OR DEMENTIA OR DEPRESSION

	United States	region 25	region 17	region 11	region 22	region 4
Decision utility parameters						
premium / OOP	3.3	1.0	5.6	4.2	1.7	-5.0
	(0.0)	(0.1)	(0.2)	(0.3)	(0.2)	(2.1)
variance / premium	0.0	0.0	0.0	-0.1	-0.2	-0.8
	(0.0)	(0.0)	(0.0)	(0.0)	(0.2)	(0.6)
deductible / premium	-0.1	-3.6	0.1	0.3	-0.7	4.9
	(0.0)	(0.3)	(0.0)	(0.1)	(0.2)	(1.8)
full gap / premium	-2.5	-2.6	-3.0	-3.3	1.6	-8.3
	(0.0)	(0.2)	(0.1)	(0.2)	(0.9)	(1.6)
generic gap / premium	-0.8	2.0	1.9	-0.1	-3.6	3.2
	(0.0)	(1.0)	(0.2)	(0.1)	(0.6)	(2.0)
cost share / premium	-2.8	-4.2	2.7	-9.1	5.7	-30.5
	(0.1)	(1.7)	(0.3)	(0.6)	(1.2)	(11.9)
top 100 / premium	-0.4	-0.3	-0.3	-0.4	-0.6	-0.1
	(0.0)	(0.0)	(0.0)	(0.0)	(0.1)	(0.1)
Welfare loss (% of costs)						
$\varepsilon \equiv 0$	41	94	18	31	63	--
ε is unrestricted	7	27	8	11	4	--
Number of consumers	155,115	17,196	11,448	9,869	9,174	6,916

Table A13 reports summary statistics of the distribution of region-level results, restricted to the 24 regions with statistically significant positive estimates for the marginal utility of income. The last row reports the premium-to-OOP ratio that is predicted from an extended version of AG's DU model from equation (4) that allows the premium-to-OOP ratio to vary with the number of plans in the choice set, the number of brands, the number of plans with gap coverage, the number of plans with zero deductible, the consumer's age, and an indicator for whether the consumer is diagnosed with dementia including Alzheimer's disease. Coefficient estimates are reported in Table A16.

TABLE A13—SUMMARY STATISTICS OF THE DISTRIBUTION OF REGION-SPECIFIC ESTIMATED PARAMETER RATIOS, PDP MENU ATTRIBUTES, CONSUMER ATTRIBUTES AND NONPARAMETRIC OUTCOMES, 2006

	Mean	Standard deviation	Minimum	25th Percentile	Median	75th Percentile	Maximum
Estimated parameter ratios							
premium / OOP	4.8	2.8	1.1	2.7	4.2	6.6	12.3
variance / premium	-0.2	0.3	-0.8	-0.4	-0.2	0.0	0.6
deductible / premium	-0.4	1.8	-7.5	-0.1	0.1	0.2	1.1
full gap / premium	-3.0	1.7	-4.4	-4.2	-3.3	-2.5	0.6
generic gap / premium	-1.6	2.7	-9.5	-1.7	-1.1	-0.4	1.6
cost share / premium	-4.5	13.0	-25.8	-10.7	-6.0	-2.0	48.1
top 100 / premium	-0.4	0.2	-0.9	-0.6	-0.4	-0.3	-0.2
Welfare loss (% of costs)							
$\varepsilon \equiv 0$	42.1	24.1	17.7	25.1	33.6	54.5	109.9
ε is unrestricted	14.9	13.1	3.3	8.8	11.3	15.9	68.4
PDP Menu							
# plans	42	9	38	41	42	44	47
# brands	19	4	17	18	19	20	23
# plans w/ gap coverage	7	1	6	6	7	7	9
Consumers							
number	16,638	11,036	3,710	7,035	14,712	23,659	46,997
mean age	76	15	75	75	76	76	78
% with Alzheimer's	7	2	6	7	8	8	9
% off cost-var frontier	75	15	67	72	75	78	82
% off cost-var-brand frontier	19	6	8	16	19	23	34
mean potential savings	506	103	355	485	508	535	621
premium / oop ratio predicted by interaction model	4.1	0.6	3.3	3.8	4.0	4.2	5.9

TABLE A14—REGION-SPECIFIC ESTIMATED PARAMETER RATIOS, PDP MENU ATTRIBUTES, CONSUMER ATTRIBUTES AND NONPARAMETRIC OUTCOMES, 2006

	region 1	region 2	region 3	region 4	region 5	region 6	region 7	region 8
Estimated parameter ratios								
premium / OOP	3.2	12.3	-0.7	3.1	3.9	0.3	0.3	6.7
	(0.3)	(0.5)	(0.4)	(1.1)	(0.6)	(0.3)	(0.3)	(0.3)
variance / premium	-0.2	-0.2	5.7	0.6	-0.4	-0.3	-6.4	0.0
	(0.1)	(0.1)	(3.8)	(0.5)	(0.2)	(0.5)	(7.2)	(0.0)
deductible / premium		0.3	1.7	-7.5	0.3	-10.9	6.3	0.2
		(0.0)	(0.7)	(2.9)	(0.1)	(11.5)	(6.3)	(0.0)
full gap / premium		-4.3	-18.4	0.5	-3.0	23.1	29.8	-4.3
		(0.1)	(8.9)	(1.9)	(0.3)	(28.4)	(37.4)	(0.1)
generic gap / premium	-2.9	-1.6	1.1	-8.8	0.2	-10.6	15.0	-1.5
	(0.1)	(0.0)	(2.3)	(2.9)	(0.4)	(9.1)	(18.0)	(0.1)
cost share / premium	-25.8	-5.6	22.2	48.1	-9.7	-9.5	-107.7	-3.9
	(1.7)	(0.2)	(16.8)	(18.0)	(1.8)	(9.4)	(112.9)	(0.2)
top 100 / premium	-0.5	-0.2	0.3	-0.5	-0.6	-1.5	-4.1	-0.3
	(0.0)	(0.0)	(0.4)	(0.2)	(0.1)	(1.4)	(4.1)	(0.0)
Welfare loss (% of costs)								
$\varepsilon \equiv 0$	29	31	-193	110	34	421	403	24
ε is unrestricted	11	22	-12	68	7	23	42	13
PDP Menu								
# plans	41	44	46	44	47	52	41	38
# brands	19	22	22	20	22	23	19	17
# plans w/ gap coverage	6	7	6	6	6	8	6	7
# plans w/ zero deductible	24	28	25	25	26	30	23	22
Consumers								
number	5,729	18,248	10,661	24,162	10,570	19,417	11,148	19,447
mean age	76	77	76	78	76	76	75	75
% with Alzheimer's	7.0	7.7	7.6	9.4	7.8	8.0	7.1	6.6
% off cost-var frontier	67	76	71	82	77	74	73	68
% off cost-var-brand frontier	10	19	9	8	14	19	17	16
mean potential savings	355	491	463	606	493	475	511	533
premium / oop ratio predicted by interaction model	3.9	5.7	3.3	3.6	3.3	3.5	3.7	4.2

Note: the last row reports the premium-to-oop ratio predicted from a generalized version of AG's model that allows the ratio to vary with the proxy measures for menu complexity and cognitive ability. For more details see the explanation of Tables A13 and A16.

TABLE A14 (CONTINUED)—REGION-SPECIFIC ESTIMATED PARAMETER RATIOS, PDP MENU ATTRIBUTES, CONSUMER ATTRIBUTES AND NONPARAMETRIC OUTCOMES, 2006

	region 9	region 10	region 11	region 12	region 13	region 14	region 15	region 16
Estimated parameter ratios								
premium / OOP	0.3	7.8	3.3	6.2	5.5	2.1	3.6	2.6
	(0.3)	(0.4)	(0.2)	(0.3)	(0.4)	(0.2)	(0.2)	(0.2)
variance / premium	-11.1	-0.2	0.0	-0.4	-0.2	-0.4	-0.7	0.0
	(12.0)	(0.1)	(0.0)	(0.1)	(0.1)	(0.2)	(0.1)	(0.2)
deductible / premium	-17.3	-0.1	0.1	-0.1	0.6	0.2	-0.1	0.0
	(18.9)	(0.0)	(0.0)	(0.0)	(0.0)	(0.1)	(0.0)	(0.1)
full gap / premium	13.5	-4.4	-3.3	-4.4	-4.2	-1.2	-4.2	-2.5
	(20.1)	(0.1)	(0.1)	(0.1)	(0.1)	(0.5)	(0.1)	(0.3)
generic gap / premium	21.8	-1.1	-0.1	-1.4	-1.0	0.5	-1.2	-0.6
	(25.5)	(0.1)	(0.1)	(0.1)	(0.2)	(0.3)	(0.1)	(0.1)
cost share / premium	40.0	-2.0	-10.9	-3.5	-12.7	-14.7	-11.0	-7.6
	(44.9)	(0.2)	(0.6)	(0.2)	(0.6)	(1.5)	(0.6)	(0.6)
top 100 / premium	-1.4	-0.2	-0.5	-0.4	-0.4	-0.9	-0.5	-0.4
	(1.3)	(0.0)	(0.0)	(0.0)	(0.0)	(0.1)	(0.0)	(0.0)
Welfare loss (% of costs)								
$\varepsilon \equiv 0$	398	24	38	27	34	66	40	42
ε is unrestricted	61	12	9	16	21	9	11	7
PDP Menu								
# plans	45	42	43	41	40	43	42	45
# brands	21	19	20	19	19	20	19	19
# plans w/ gap coverage	6	7	8	6	6	7	7	9
# plans w/ zero deductible	24	24	25	23	23	25	25	29
Consumers								
number	7,650	17,268	30,138	16,928	10,389	15,932	23,832	9,340
mean age	76	75	76	75	76	76	76	75
% with Alzheimer's	7.4	8.0	7.8	7.9	7.5	7.7	7.0	6.1
% off cost-var frontier	79	79	76	71	74	72	76	78
% off cost-var-brand frontier	20	20	15	23	22	16	24	21
mean potential savings	558	495	543	522	499	495	540	427
premium / oop ratio predicted by interaction model	3.3	3.9	4.0	3.7	3.9	3.9	4.0	4.6

Note: the last row reports the premium-to-oop ratio predicted from a generalized version of AG's model that allows the ratio to vary with the proxy measures for menu complexity and cognitive ability. For more details see the explanation of Tables A13 and A16.

TABLE A14 (CONTINUED)—REGION-SPECIFIC ESTIMATED PARAMETER RATIOS, PDP MENU ATTRIBUTES, CONSUMER ATTRIBUTES AND NONPARAMETRIC OUTCOMES, 2006

	region 17	region 18	region 19	region 20	region 21	region 22	region 23	region 24
Estimated parameter ratios								
premium / OOP	7.0	4.4	-0.2	4.7	2.2	1.9	8.1	9.9
	(0.2)	(0.3)	(0.3)	(0.4)	(0.6)	(0.1)	(0.6)	(0.6)
variance / premium	0.0	-0.6	0.0	-0.3	0.0	-0.8	-0.2	-0.4
	(0.0)	(0.1)	(0.0)	(0.1)	(0.3)	(0.2)	(0.1)	(0.1)
deductible / premium	0.0	0.2	15.0	0.0	-1.6	-0.5	0.2	0.1
	(0.0)	(0.0)	(25.6)	(0.1)	(0.7)	(0.1)	(0.0)	(0.0)
full gap / premium	-3.5	-3.4	-46.6	-3.5	-3.3	0.6	-3.7	-4.0
	(0.1)	(0.1)	(74.3)	(0.2)	(0.7)	(0.5)	(0.1)	(0.1)
generic gap / premium	1.6	-1.1		-1.8	-9.5	-2.8	-0.3	-0.8
	(0.1)	(0.1)		(0.2)	(2.9)	(0.3)	(0.1)	(0.1)
cost share / premium	3.8	-6.8	32.5	-1.3	3.2	5.0	-2.5	-2.2
	(0.2)	(0.4)	(58.0)	(0.5)	(2.4)	(0.6)	(0.3)	(0.2)
top 100 / premium	-0.4	-0.4	3.5	-0.3	-0.6	-0.7	-0.4	-0.2
	(0.0)	(0.0)	(6.4)	(0.0)	(0.1)	(0.0)	(0.0)	(0.0)
Welfare loss (% of costs)								
$\varepsilon \equiv 0$	18	31	-295	24	40	62	20	23
ε is unrestricted	9	11	-50	10	8	3	10	16
PDP Menu								
# plans	42	41	40	38	39	47	42	40
# brands	18	18	17	17	18	22	18	17
# plans w/ gap coverage	6	6	6	6	6	6	7	7
# plans w/ zero deductible	25	25	24	23	24	27	25	25
Consumers								
number	37,939	13,492	7,317	6,785	4,265	29,387	6,880	7,499
mean age	78	76	75	75	75	76	76	77
% with Alzheimer's	8.2	7.9	8.5	6.9	7.8	8.6	7.6	7.5
% off cost-var frontier	79	73	79	74	75	72	79	78
% off cost-var-brand frontier	15	21	30	26	19	18	24	24
mean potential savings	469	491	591	520	547	517	520	536
premium / oop ratio predicted by interaction model	4.0	4.1	4.1	4.3	4.2	3.5	4.1	4.5

Note: the last row reports the premium-to-oop ratio predicted from a generalized version of AG's model that allows the ratio to vary with the proxy measures for menu complexity and cognitive ability. For more details see the explanation of Tables A13 and A16.

TABLE A14 (CONTINUED)—REGION-SPECIFIC ESTIMATED PARAMETER RATIOS, PDP MENU ATTRIBUTES, CONSUMER ATTRIBUTES AND NONPARAMETRIC OUTCOMES, 2006

	region 25	region 26	region 27	region 28	region 29	region 30	region 31	region 32
Estimated parameter ratios								
premium / OOP	1.1	-0.2	4.5	1.5	1.8	3.9	-2.2	5.6
	(0.0)	(0.9)	(0.6)	(0.7)	(2.0)	(0.4)	(0.7)	(0.4)
variance / premium	0.0	0.9	-0.8	0.1	-3.1	0.0	2.7	0.0
	(0.0)	(7.1)	(0.2)	(0.3)	(3.6)	(0.0)	(1.0)	(0.0)
deductible / premium	-3.6	6.5	0.2	-0.7	0.8	1.1	1.5	0.5
	(0.2)	(22.6)	(0.1)	(0.6)	(0.5)	(0.1)	(0.4)	(0.0)
full gap / premium	-2.9	-45.5	-3.1	-0.3	1.8	-2.3	-9.1	-3.1
	(0.1)	(147.7)	(0.3)	(2.3)	(7.3)	(0.3)	(1.5)	(0.1)
generic gap / premium	0.8		-0.7	-2.9	0.3	-0.8		-1.1
	(0.6)		(0.2)	(1.3)	(1.4)	(0.3)		(0.1)
cost share / premium	-3.2	-29.3	-6.4	-10.1	-25.8	-22.1	0.3	-6.4
	(0.9)	(114.5)	(0.8)	(5.0)	(25.8)	(1.9)	(2.1)	(0.4)
top 100 / premium	-0.4	62.6	-0.3	-0.7	-0.7	-0.6	0.3	-0.4
	(0.0)	(225.2)	(0.0)	(0.3)	(0.7)	(0.0)	(0.2)	(0.0)
Welfare loss (% of costs)								
$\varepsilon \equiv 0$	92	-324	33	79	109	59	-86	31
ε is unrestricted	24	-118	10	4	9	32	-29	15
PDP Menu								
# plans	41	43	43	43	44	45	44	47
# brands	23	19	19	20	20	22	20	20
# plans w/ gap coverage	7	6	7	6	7	6	6	7
# plans w/ zero deductible	23	26	26	25	25	25	23	28
Consumers								
number	46,997	1,587	3,710	4,926	1,703	12,314	5,594	23,141
mean age	76	75	76	75	75	76	76	76
% with Alzheimer's	6.2	6.9	7.4	5.7	6.4	6.6	6.3	6.6
% off cost-var frontier	74	74	73	69	78	74	76	78
% off cost-var-brand frontier	34	13	18	13	17	18	20	17
mean potential savings	621	414	468	444	510	483	532	521
premium / oop ratio predicted by interaction model	5.9	3.9	4.1	3.7	3.7	3.4	3.4	3.8

Note: the last row reports the premium-to-oop ratio predicted from a generalized version of AG's model that allows the ratio to vary with the proxy measures for menu complexity and cognitive ability. For more details see the explanation of Tables A13 and A16.

FIGURE A3—RATIO OF PREMIUM-TO-OOP COEFFICIENTS IN 2006, BY CMS REGION USING AG'S DEFINITION OF BRAND DUMMIES BASED ON CONTRACT ID

Note: The figure reports the premium-to-OOP coefficient ratio obtained by estimating region-specific models with contract id dummies. The econometric specification is the same as the national model in column 2 of Table 4. In regions with light-shaded numbers, we fail to reject the null hypothesis that the marginal utility of income is negative at the 5% level. All estimates are statistically indistinguishable from 1 at the 5% level.

Table A15 provides the coefficients and standard errors from meta-regressions of the conditional relationship between the premium-to-OOP ratio and proxy measures for menu complexity and cognitive ability. The models are limited to the 24 regions with statistically significant positive estimates for the marginal utility of income. The main text provides additional details.

TABLE A15: RESULTS FROM MODELS OF THE REGION-LEVEL ESTIMATES FOR AG'S PARAMETRIC MEASURES OF CHOICE QUALITY ON PROXY MEASURES FOR MENU COMPLEXITY AND COGNITIVE ABILITY

	(1)	(2)	(3)	(4)	(5)
				% welfare loss	
	premium-to-OOP coef ratio			$\varepsilon \equiv 0$	$\varepsilon \neq 0$
Number of plans	-0.508		-0.559	1.100	0.707
	(0.553)		(0.577)	(4.420)	(2.386)
Number of brands	-0.234		-0.214	7.613*	1.082
	(0.521)		(0.553)	(4.241)	(2.289)
Number of plans w/ gap coverage	-0.284		0.0865	0.837	0.703
	(0.896)		(0.943)	(7.227)	(3.901)
Number of plans w/ zero deductible	0.858		0.802	-4.213	-2.574
	(0.710)		(0.725)	(5.555)	(2.998)
mean age		0.706	0.811	8.567	10.43**
		(0.960)	(1.004)	(7.697)	(4.154)
% with Alzheimer's		0.277	0.445	-3.410	0.129
		(0.861)	(0.947)	(7.261)	(3.919)
Constant	11.33	-50.83	-52.80	-678.1	-769.2**
	(10.30)	(69.39)	(71.95)	(551.5)	(297.7)
Observations	24	24	24	24	24
R^2	0.151	0.061	0.239	0.404	0.422
Adjusted R^2	-0.028	-0.028	-0.029	0.193	0.218
P-value of model Wald Chi-Square	0.514	0.515	0.523	0.136	0.111

Note: *** p<0.01, ** p<0.05, * p<0.1

To check robustness of the results from the meta-regression in equation (9) we estimate AG's DU model (4) after adding interactions between the OOP ratio and the proxy measures for menu complexity and cognitive ability. This logit model accounts for variation in menu complexity across CMS regions and for variation in cognitive ability within and across CMS regions. Each of the interaction coefficients is statistically significant at the 1% level. To evaluate their economic magnitudes we use the estimates to predict how the premium-to-oop ratio would change as we move from the lowest value of each variable observed in our data to the highest value, while evaluating all other variables at their means. The resulting ranges are reported in the last two columns of Table A16. For example, the results in the first row of the table imply that *increasing* the number of plans in a consumer's choice set from 38 plans to 52 plans would *decrease* the premium-to-oop ratio from 4.6 to 2.9, contrary to the hypothesis of choice overload.

TABLE A16: ESTIMATED EFFECTS OF PROXY MEASURES FOR MENU COMPLEXITY AND COGNITIVE ABILITY ON THE PREMIUM-TO-OOP RATIO

	Summary statistics			Econometric estimates		Premium-to-OOP ratio	
	mean	min	max	interaction with OOP	standard error	predicted at the Min	predicted at the Max
Number of plans in choice set	43.15	38	52	-0.0035	(0.0004)	4.6	2.9
Number of brands in choice set	20.05	17	23	-0.0014	(0.0003)	4.0	3.7
Number of plans with gap coverage	6.67	6	9	0.0033	(0.0007)	3.7	4.1
Number of plans with zero deductible	25.08	22	30	0.0043	(0.0005)	3.4	4.7
Age	76.04	66	108	0.0002	(0.0001)	3.7	4.0
Dementia including Alzeheimer's	0.08	0	1	-0.0102	(0.0014)	3.8	3.5

Note: The estimated coefficients on premium and OOP are -0.406 and -0.066 respectively. Both have p-values of zero out to four decimal places.

Table A17 provides results from validation tests for the cases where the set of brands in an estimation region spans the set of brands in the prediction region. Two pairs of regions meet this criterion in 2006. As a result, we estimate the AG's two competing models for region 14 and then use the resulting coefficients

to predict outcomes in region 15, and we use estimates for region 30 to predict outcomes in region 28. Both region pairs are similar in their consumer populations and PDP menu complexity. AG's EUM model yields closer out-of-sample predictions than their DU model in every case but one. The shading indicates which prediction is closer to the data.

TABLE A17: RESULTS FROM BETWEEN-REGION VALIDATION TESTS FOR THE ONLY TWO PAIRS OF REGIONS IN 2006 FOR WHICH ONE REGION'S BRANDS ARE NESTED WITHIN THE OTHER'S

	region 14 → 15			region 30 → 28		
	data	AG's DU	AG's EUM	data	AG's DU	AG's EUM
	In-sample data and predictions					
	region 14			region 30		
Percent of consumers choosing:						
gap coverage	11	11	13	6	6	9
dominated plan	16	18	17	18	19	17
min cost plan within brand	47	40	41	41	35	40
Median consumer expenditures ($)						
premium + OOP	1,261	1,262	1,267	1,074	1,093	1,095
overspending on dominated plans	0	65	58	0	63	58
Market concentration						
Hirfindahl-Hirschman index	25	25	25	25	25	25
market share of top brand	44	44	44	39	39	39
	Out-of-sample data and predictions					
	region 15			region 28		
Percent of consumers choosing:						
gap coverage	9	6	10	18	11	13
dominated plan	12	25	19	24	18	17
min cost plan within brand	50	32	40	42	40	41
Median consumer expenditures ($)						
premium + OOP	1,096	1,205	1,178	1,418	1,352	1,355
overspending on dominated plans	0	102	70	0	67	57
Market concentration						
Hirfindahl-Hirschman index	44	30	31	0 21	27	26
market share of top brand	62	42	46	0 31	45	44

Table A18 provides results from the national validation test shown in Table 6 except using the root mean square error in predictions across regions in place of the mean absolute error.

TABLE A18— NONRANDOM HOLDOUT SAMPLE TESTS OF MODEL VALIDATION, 2006

		In-sample fit		Out-of-sample fit	
		AG's DU	AG's EUM	AG's DU	AG's EUM
	data	RMSE		RMSE	
		Using CMS Star Ratings for Quality			
Percent of consumers choosing:					
gap coverage	13	0	7	9	7
dominated plan	20	5	6	8	7
min cost plan within brand	52	8	9	11	9
Median consumer expenditures ($)					
premium + OOP	1,255	16	46	113	88
overspending on dominated plans	0	72	55	70	51
Market concentration					
Hirfindahl-Hirschman index	25	11	15	11	15
market share of top brand	37	12	18	14	18
		Using Brand Indicators for Quality			
Percent of consumers choosing:					
gap coverage	13	0	4	9	8
dominated plan	20	2	4	9	9
min cost plan within brand	52	6	9	14	13
Median consumer expenditures ($)					
premium + OOP	1,256	16	21	102	101
overspending on dominated plans	0	82	71	88	70
Market concentration					
Hirfindahl-Hirschman index	25	0	0	14	13
market share of top brand	37	0	0	18	17

Note: RMSE refers to the root mean square error between the regional-level model predictions and data, weighted across regions by the number of people in the sample in the region. The results are based on every possible pairwise combination of regions in 2006 except that they exclude regions 33 and 34 (HI and AK), and the lower half also excludes region 26 (NM). Thus the values in the top half are based on the results from all 992 of the possible regional out-of-sample predictions while those in the lower half are based on 930 of them.

Table A19 provides results from the national validation test suggested to us by Abaluck and Gruber. Specifically, we estimate the models using the 2006 data from 31 regions and use it to predict a single out-of-sample region, repeated using each of the 32 regions as the holdout region (excluding Alaska and Hawaii). This is very similar to an in-sample validation test as the set of plans and plan attributes in the single out-of-sample region is typically very close to being nested within the in-sample set (see Keane and Wolpin 2007). As before the measures of market concentration are defined at the region level as that is the policy-relevant market definition. Hence while the models with brand indicators match the market concentration perfectly across the 31 in-sample regions in each of the 32 separate tests (yielding a mean absolute deviation of 0), they do not perfectly predict the region-level market concentration for any single given in-region sample.

TABLE A19— RESULTS FROM THE NATIONAL MODEL VALIDATION TESTS SUGGESTED BY ABALUCK AND GRUBER

		In-sample fit		Out-of-sample fit	
		AG's DU	AG's EUM	AG's DU	AG's EUM
	data	|model error|		|model error|	
		Using CMS Star Ratings for Quality			
Percent of consumers choosing:					
gap coverage	13	0	2	6	6
dominated plan	20	2	2	6	6
min cost plan within brand	52	3	4	6	5
Median consumer expenditures ($)					
premium + OOP	1,255	4	15	61	60
overspending on dominated plans	0	63	51	63	51
Market concentration					
Hirfindahl-Hirschman index	25	11	14	11	14
market share of top brand	37	9	17	11	17
		Using Brand Indicators for Quality			
Percent of consumers choosing:					
gap coverage	13	0	2	6	6
dominated plan	20	1	1	5	5
min cost plan within brand	52	7	8	8	9
Median consumer expenditures ($)					
premium + OOP	1,255	10	14	61	57
overspending on dominated plans	0	64	61	71	63
Market concentration					
Hirfindahl-Hirschman index	25	4	4	5	4
market share of top brand	37	6	6	8	7

Note: | Model error | refers to the mean absolute deviation between the model predictions and data.