

Selection on moral hazard in health insurance*

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Abstract. Existing empirical work on asymmetric information in insurance markets tends to focus either on selection or on moral hazard, but not on how they interact. In this paper we explore the possibility that individuals may select insurance coverage in part based on their anticipated behavioral response to the insurance contract. Such “selection on moral hazard” can have important implications for attempts to ameliorate the consequences either of selection or of moral hazard. To explore these issues, we develop a model of plan choice and medical utilization, and estimate it using individual-level panel data from a single firm, containing information about health insurance options, choices, and subsequent claims. To identify the behavioral response to health coverage and the heterogeneity in it, we take advantage of a change in the health insurance options offered to some, but not all, of the firm’s employees. We find substantial selection on moral hazard in our setting, with individuals who exhibit greater behavioral response to coverage also selecting greater coverage. One implication of our estimates is that abstracting from selection on moral hazard could lead one to substantially over-estimate the spending reduction associated with introducing a high deductible health insurance option.

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1 Introduction

Economic analysis of market failure in insurance markets tends to analyze selection and moral hazard as distinct phenomena. In this paper, we explore the potential for *selection on moral hazard* in insurance markets. By this we mean the possibility that moral hazard effects are heterogeneous across individuals, and that individuals' selection of insurance coverage is affected by their anticipated behavioral response to coverage – their “moral hazard type.” We examine these issues empirically in the context of employer-provided health insurance in the United States.

Selection on moral hazard has implications for the standard analysis of both selection and moral hazard. For example, a standard – and ubiquitous – approach to mitigating selection in insurance markets is risk adjustment, i.e. pricing on observable characteristics that predict one's insurance claims. However, the potential for selection on moral hazard suggests that monitoring techniques that are usually thought of as reducing moral hazard – such as cost sharing that varies across categories of claims with differential scope for moral hazard – may also have important benefits in combatting adverse selection. In contrast, a standard approach to mitigating moral hazard is to offer plans with higher consumer cost sharing. But if individuals' anticipated behavioral response to coverage affects their propensity to select such plans, the magnitude of the behavioral response could be much lower (or much higher) from what would be achieved if plan choice were unrelated to the behavioral response. As we discuss in more detail below, not only the existence of selection on moral hazard but also the sign of any relationship between anticipated behavioral response and demand for higher coverage is *ex ante* ambiguous. Ultimately, these are empirical questions. To our knowledge, however, there is no empirical work on selection on moral hazard in insurance markets.

Health insurance provides a particularly interesting setting in which to explore these issues. Both selection and moral hazard have been well-documented in the employer-provided health insurance market in the U.S. Moreover, given the extensive government involvement in health insurance, as well as the concern about the size and rapid growth of the health care sector, there is considerable academic and public policy interest in how to mitigate both selection and moral hazard in this market.

Recognition of the possibility of selection on moral hazard, however, highlights potentially important limitations of analyzing these problems in isolation. For example, the sizable empirical literature on the likely spending reductions that could be achieved through higher consumer cost sharing has intentionally focused on isolating and exploring exogenous changes in cost sharing – such as those induced by the famous Rand experiment (Manning et al., 1987; Newhouse et al., 1993). Yet, the very same feature that solves the causal inference problem – namely randomization (or attempts to approximate it in the subsequent quasi-experimental literature on this topic) – removes the endogenous choice element. It thus abstracts, by design, from any selection on moral hazard, which could have important implications for the spending reductions achieved through offering plans with higher consumer cost sharing, especially since substantial plan choice is now the norm not only in private health insurance but also increasingly in public health insurance programs, such as Medicare Part D.

We explore these issues using data on the U.S. workers at Alcoa Inc, a large multinational producer of aluminum and related products. We observe individual-level data on the health insurance options, choices, and subsequent medical utilization of employees (and their dependents); we also observe relatively rich demographic information. Crucially for identifying and estimating moral hazard, we observe variation in the health insurance options offered to different groups of workers. In an effort to control health spending, Alcoa began introducing a new set of health insurance options in 2004, designed to encourage employees to move into plans with substantially higher consumer cost sharing. We calculate that, if there were no change in behavior, the move from the original options to the new options would have increased the average share of spending paid out of pocket from 13 to 28 percent. We exploit the fact that, for unionized employees, the introduction of the new health insurance options was phased in gradually, as the new health insurance options could only be introduced when existing union contracts expired.

We begin by providing descriptive and motivating evidence on moral hazard in our setting. Difference-in-differences estimates suggest that the new options are associated with an average reduction in medical spending of about \$600 (11 percent) per employee. We find evidence consistent with heterogeneity in this moral hazard effect, such as larger moral hazard effects for older relative to younger employees. We also present suggestive evidence of selection on moral hazard, with those who select more generous coverage appearing to have a greater behavioral response to coverage.

We then develop a utility-maximizing model of individual health insurance plan choices and claims. The model draws heavily on a relatively standard two-period framework for modeling health insurance demand and subsequent medical care utilization (as in, e.g., Cardon and Hendel, 2001). In the first period, a risk-averse expected-utility-maximizing individual makes optimal coverage choices based on his risk aversion, health expectations, and anticipated behavioral response to the contract choice. In the second period, health is realized and individuals make optimal medical expenditure decisions based on their realized health as well as on their chosen coverage. It is this last effect which generates what we term moral hazard, with a larger responsiveness corresponding to a higher “moral hazard type.” We allow for unobserved heterogeneity along three dimensions: health expectations, risk aversion, and moral hazard, and for flexible correlation across these three.

An individual’s optimal health insurance choice involves a trade-off of higher up-front premiums in exchange for lower ex-post out-of-pocket spending. All else equal, willingness to pay for coverage is increasing in the individual’s health expectation and his risk aversion; these are standard results. In addition, all else equal, willingness to pay for coverage is increasing in the individual’s moral hazard type: individuals with a greater behavioral response to coverage benefit more from more coverage, since they will consume more care as a result. This is the “selection on moral hazard” comparative static that is the focus of our paper. Empirically, however, the sign (let alone the magnitude) of any selection on moral hazard is ambiguous and depends on the heterogeneity in moral hazard as well as the correlation between moral hazard type and the other primitives that affect health insurance choice, expected health and risk aversion.

We use this model, together with the data on individual plan options, plan choice, and subsequent medical spending, to recover the joint distribution of individuals’ (unobserved) health type,

risk aversion, and moral hazard type. The econometric model and its identification share many properties with some of our earlier work on insurance (Cohen and Einav, 2007; Einav, Finkelstein, and Schripf, 2010). The inclusion of moral hazard and heterogeneity in it is new. The panel structure of the data and the staggered timing of the introduction of the new options are key in allowing us to identify this new element. The model is estimated using Markov Chain Monte Carlo Gibbs sampler, and its fit appears reasonable.

We estimate substantial heterogeneity in moral hazard, which is a necessary condition for selection on moral hazard to be important. For example, we find that the standard deviation across individuals of the spending reduction that would be achieved by moving them from the most comprehensive to the least comprehensive of the new options – essentially moving them from a no deductible plan to a high (\$3,000 for family coverage) deductible plan – is more than twice the average.

Moreover, we find substantial selection on moral hazard in our data. We estimate that the demand for the high deductible plan is declining in moral hazard type – so that the more behaviorally responsive individuals are less likely to choose the high deductible plan – with a quantitatively large gradient. For example, we find that for determining plan choice, selection on moral hazard is considerably more important than selection on risk aversion.

We examine some of the implications of the selection on moral hazard we detect for spending and welfare. For example, in terms of spending, our results suggest that if we were to introduce the high deductible plan in a setting where previously there was only the no deductible plan, and price it so that 10 percent of the population chooses the high deductible plan, spending would fall by approximately \$115 per person. By contrast, were we to ignore selection on moral hazard and assume that the 10 percent who chose the high deductible plan were randomly drawn from the moral hazard distribution, we would have estimated a spending reduction more than twice as large, at about \$270 per person. In terms of welfare, we estimate, for example, that about 10 percent of the welfare gain that can be achieved in our setting by perfect risk adjustment that eliminates adverse selection could be achieved if better monitoring technologies eliminated selection on moral hazard. We also estimate that about one-quarter of the welfare cost of moral hazard in our setting comes from selection on moral hazard, rather than the “traditional” inefficiency coming through excessive health care consumption. While our quantitative estimates are likely highly specific to our setting and our modeling choices, they nonetheless provide an interesting example of the potential for selection on moral hazard to play a non-trivial role in the analysis of both selection and moral hazard.

Our paper is related to several distinct literatures. As previously noted, our modeling approach is closely related to that of Cardon and Hendel (2001), which is also the approach taken by Bajari et al. (2006), Handel (2009), and Carlin and Town (2010) in modeling health insurance plan choice. Like our approach, all of these other papers have allowed for selection based on expected health type. We differ from these other papers by our focus on identifying and estimating moral hazard – and in particular heterogeneous moral hazard – and in examining the relationship between moral hazard type and plan choice. From a methodological perspective, we also differ from most discrete

choice models in that we do not allow for a choice-specific, i.i.d. error term, which does not seem appealing given the vertically rankable nature of our choices.

Our examination of selection on moral hazard is motivated in part by the growing empirical literature demonstrating that selection in insurance markets often occurs on dimensions other than risk type. This literature has tended to abstract from moral hazard, and focused on selection on preferences, such as risk aversion (Finkelstein and McGarry, 2006; Cohen and Einav, 2007), cognition (Fang et al., 2008), or desire for wealth after death (Einav, Finkelstein, and Schrimpf, 2010). Our exploration of selection on moral hazard highlights another potential dimension of selection and one that, we believe, has particularly interesting implications for contract design in contexts where moral hazard is important. For many questions the extent to which selection occurs on the basis of expected health type or risk aversion does not matter (see, e.g., Einav, Finkelstein, and Cullen, 2010). However, as we illustrate in this paper, for questions regarding the design of contracts to reduce selection and the implications of contract design for spending, the extent to which selection is based on moral hazard can be important. Yet we are not aware of any empirical work attempting to identify and analyze selection on moral hazard in insurance markets.¹

Finally, our analysis of the spending reduction associated with changes in cost sharing is related to a sizable experimental and quasi-experimental literature in health economics analyzing the impact of higher consumer cost sharing on spending. The difference-in-differences exercises with which we begin our analysis is very much in the spirit of this literature, which searches for identifying variation in consumer health plans to isolate the causal impact of consumer cost sharing on health spending. Our central difference-in-differences estimate translates into an implied arc elasticity of medical spending with respect to the average out-of-pocket cost share of about -0.14. This is broadly similar to the findings of the existing experimental and quasi-experimental literature which tends to produce arc elasticities in the range of -0.1 to -0.4, with the “central” Rand elasticity estimate of -0.2 (see Chandra, Gruber, and McKnight (2010) for a recent review). However, our subsequent exploration of heterogeneity in this average moral hazard effect and selection on it suggests the need for caution in using such estimates, which do not account for endogenous plan selection, for forecasting the likely spending effects of introducing the option of plans with higher consumer cost sharing. It also suggests that one can embed the basic identification approach of the difference-in-differences framework in a model that allows for and investigates such endogenous selection.

The rest of the paper proceeds as follows. Section 2 describes the data and presents descriptive

¹The basic conceptual point, however, is not unique to us. Karlan and Zinman (2009) observe that selection in a credit market may be on unobserved risk and/or on anticipated effort, although they do not empirically distinguish between the two. From a theory standpoint, Chassagnon and Chiappori (1997) characterize competitive equilibrium in an insurance market with both selection and moral hazard. And from an econometric point, Heckman, Urzua, and Vytlacil (2006) examine the properties of IV estimators when individuals select into treatment in part based on their anticipated response to the treatment, a phenomenon they refer to as “essential heterogeneity”; these ideas are then applied in the context of the returns to education in Carneiro, Heckman, and Vytlacil (2010).

evidence of moral hazard, heterogeneity in moral hazard, and selection on moral hazard in our data. Section 3 sketches a two-period model of an individual’s health insurance plan choice and spending decisions. Building on this behavioral model, Section 4 presents the econometric specification and describes its identification and estimation. Section 5 presents our results, and Section 6 illustrates some of their implications for spending and welfare. The last section concludes.

2 Data and Descriptive Evidence

2.1 Setting and Data

We study health insurance choices and medical care utilization of the U.S.-based workers (and their dependents) at Alcoa, Inc., a large multinational producer of aluminum and related products. Our main analysis is based on data from 2003 and 2004, although for some of the analyses we extend the sample through 2006.

In 2004, in an effort to control health care spending by encouraging employees to move into plans with substantially higher consumer cost sharing, Alcoa introduced a new set of health insurance PPO options. The new options were introduced gradually to different employees based on their union affiliation, since new benefits could only be introduced when an existing union contract expired. The staggered timing in the transition from one set of insurance options to another provides a plausibly exogenous source of variation that can help us identify the impact of health insurance on medical care utilization, which is what we mean throughout by the term “moral hazard.”

Our data contain the menu of health insurance options available to each employee, the employee’s coverage choices, and detailed, claim-level information on his (and any covered dependents’) medical care utilization and expenditures for the year.² The data also contain relatively rich demographic information (compared to typical claims data), including the employee’s union affiliation, employment type (hourly or salary), age, race, gender, annual earnings, job tenure at the company, and the number and ages of other insured family members.

Sample definition and demographics Alcoa has about 45,000 active employees per year. We exclude about 15 percent of the sample whose data are not suited to our analytical framework.³ Given the source of variation used to identify moral hazard, we concentrate on the approximately

²Health insurance choices are made in November, during the open enrollment period, and apply for the subsequent calendar year. They can be changed during the year only if the employee has a qualifying event, which is not common.

³The biggest reduction in sample size comes from excluding workers who are not at the company for the entire year (for whom we do not observe complete annual medical expenditures). In addition, we exclude employees who are outside the traditional benefit structure of the company (for example because they were working for a recently acquired company with a different (grandfathered) benefit structure); for such employees we do not have detailed information on their insurance options and choices. We also exclude a small number of employees because of missing data or data discrepancies.

one third of Alcoa workers who are unionized.⁴ We further exclude the approximately two thirds of unionized workers that are covered by the Master Steel Workers' agreement. These workers faced only one PPO option which was left unchanged over our sample period. Finally, we exclude the approximately 10 percent of unionized employees who choose HMOs or who opt out of Alcoa-provided insurance, thus limiting our sample to employees enrolled in one of Alcoa's PPO plans.⁵

Our baseline sample therefore consists of the approximately 4,000 unionized workers each year not covered by the Master agreement. These workers belong to one of 28 different unions. Table 1 (top row) provides some descriptive statistics on the demographic characteristics of our baseline sample in 2003. Our sample is 72 percent white, 84 percent male, with an average age of 41, average annual income of about \$31,000, and an average tenure of about 10 years at the company. Approximately one quarter of the sample has single (employee only) coverage, while the rest also cover additional dependents. The remaining rows of Table 1 show summary statistics for four different groups of employees based on when they were switched to the new benefit options (i.e. four different treatment groups); we discuss this comparison when we present our difference-in-differences strategy and results below.

As noted, our main analysis is based on the 2003 and 2004 data (7,574 employee-years and 4,481 unique employees). We exclude the 2005 and 2006 data from our primary analysis because it introduces two challenges for estimation of our plan choice model. First, the relative price of comprehensive coverage on the new options was raised substantially in 2005 and raised further in 2006, yet remarkably few employees already in the new option set changed their plans. This is consistent with substantial evidence on the persistence of health insurance plan choices, and the existence of switching costs (or other forms of behavioral inertia) in health insurance markets (Handel, 2009; Carlin and Town, 2010). Rather than modeling these switching costs – and their potential correlations with our primitives of interest – we prefer instead to restrict the data to a time period where they are less central to understanding plan choices. Of course, plan choice for individuals under the old options may also reflect inertial factors (indeed, plan switching is extremely rare (less than 1 percent) for employees whose options did not change in 2004), but the pricing under the old options is not changing during our sample period, making any such inertia less central for trying to understand current choices. Second, the pricing in 2006 is such that it is hard to rationalize some of the plan choices in which there is considerable mass, without extending the model to include some combination of switching costs, additional plan features, and/or biased expectations; again, we prefer to avoid these issues in the context of our primary question of interest.

⁴Approximately 70 percent of Alcoa workers are hourly employees, and approximately half of these are unionized. Salaried workers are not unionized.

⁵As is typical in claims data bases, we lack informaton for employees who choose an HMO or who opt out of employer coverage on both the details of their insurance coverage and their medical care utilization. Of course, this raises potential sample selection concerns. Reassuringly, as we show in Appendix A, the change in PPO health insurance options does not appear to be associated with a statistically or economically significant change in the fraction of employees who choose one of these excluded options.

The main drawback to limiting the data to 2003 and 2004 is that less than one-fifth of our sample were offered the new benefits starting in 2004, while another half of the sample was transitioned to the new benefits in 2005 and 2006 (Table 1, column (1)). Therefore, for some of the descriptive evidence we report in this section (which does not require an explicit model of plan choice) we use data from 2003-2006. This sample produces qualitatively similar descriptive results to the 2003-2004 sample, but the larger sample size allows for greater precision (and hence probing) in our descriptive exercises.

Medical spending We have detailed, claim-level information on medical expenditures and utilization. Our primary use of these data is to construct annual total medical spending for each employee (and his covered dependents). In Appendix A, we also use these data in a less aggregated way to break out spending by category (i.e., doctor’s office, outpatient, inpatient, and other).

Figure 1 graphs the distribution of medical spending for our sample. We show the distribution separately for the approximately three-quarters of our sample with non-single coverage and the remainder with single employee coverage; not surprisingly, average spending is substantially higher in the former group. Across all employees, the average annual spending (on themselves and their covered dependents) is about \$5,200.⁶ As is typical, medical expenditures are extremely skewed. For example, for non-single coverage, average spending (\$6,100) is about 2.5 times greater than the median spending (\$1,800), about 4 percent of our baseline sample has no spending, while the top 10 percent spends over \$13,000.

Health insurance options and choices An attractive feature of our setting is that the PPO plans in both the original and new regimes differ (within and across regimes) only in their consumer cost sharing requirements. They are identical on all non-cost sharing features, such as the network definition. Table 2 summarizes the original and new plan options and the fraction of employees who choose each option in our baseline sample. Employees may choose from up to four coverage tiers: single (employee only) coverage, or one of three non-single coverage tiers: employee plus spouse, employee plus children, or family. In our analysis we take coverage tier as given, assuming that it is primarily driven by family structure.⁷

There were three PPO options under the old benefits and five entirely different PPO options under the new benefits.⁸ The primary change from the old to the new benefits was to offer plans

⁶A little over one quarter of total spending is in doctor offices, about one third is for inpatient hospitalizations, and about one third is for outpatient services. About half of the remaining four percent of spending is accounted for by emergency room visits.

⁷Employee premiums vary across the four coverage tiers according to fixed ratios. Cost sharing provisions differ only between single and non-single coverage. Specifically, for a given PPO, deductibles and out-of-pocket maxima are twice as great for any non-single coverage tier as they are for single coverage. As shown in Table 1, about one quarter of the sample chooses single coverage. Within non-single coverage, slightly over half choose family coverage, 30 percent choose employee plus spouse, and about 16 percent choose employee plus children (not shown).

⁸Since the five options were all new, there was no option of being defaulted into one’s existing coverage. Default

with higher deductibles and to increase the lowest out-of-pocket maximum.⁹

As shown in the table, under the new options there was a shift to plans with higher consumer cost sharing. Under the old options virtually all employees faced no deductible. Looking at employees with non-single coverage in Panel B (patterns for single coverage employees are similar), about two fifths faced a \$2,000 out-of-pocket maximum while three-fifths faced a \$5,000 out-of-pocket maximum. By contrast, under the new options, about a third of the employees faced a deductible, and all of them faced a high out-of-pocket maximum of at least \$5,000 for non-single coverage.¹⁰

As one way to summarize the differences in consumer cost sharing under the different plans, we used the plan rules to simulate the average share of medical spending that would be paid out of pocket (counterfactually for most individuals) under different plans for all 2003 employees and their realized medical claims.¹¹ Less generous plans correspond to those with higher consumer cost sharing. The results are summarized in the third row of each panel of Table 2. Combining the information on average enrollment shares of the different plans with our calculation of the average cost sharing in the different plans, we estimate that, holding spending behavior constant, the change from the original options to the new options on average would have more than doubled the share of spending paid out of pocket from about 13 to 28 percent.¹²

The plan descriptions in Table 2, and the subsequent parameterization of our model in Section 4, abstract from some additional details. First, while we model all plans as having a 10 percent coverage was option 4, but given that the majority of employees did not choose it, we are not particularly concerned that defaults played an important role in 2004.

⁹At a point in time, prices within a coverage tier vary slightly across employees (in the range of several hundred dollars) under either the old or new options, depending on the employee's affiliation (see Einav, Finkelstein, and Cullen (2010) for more detail). Premiums were constant over time under the old options; as mentioned, under the new options, premiums were increased substantially (and cross-employee differences were removed) in 2005 and 2006 (not shown).

¹⁰A \$5,000 (\$2,500) out-of-pocket maximum for non-single (single) coverage is rarely binding. With no deductible and a 10 percent consumer cost sharing, the employee must have \$50,000 (\$25,000) in total annual medical expenditures to hit the out-of-pocket maximum. Using the realized claims, we calculate that only about one percent of the employees would hit the out-of-pocket maximum in a given year. By contrast, under the old options the lowest out-of-pocket maximum was \$2,000 (\$1,000) for non-single (single) coverage, corresponding to total annual spending of \$20,000 (\$10,000). Using the same realized claims distribution, we calculate that about 5.5 percent of employees would hit this out-of-pocket maximum.

¹¹By constructing (counterfactually) the share of a given (constant) set of medical expenditures that would be covered by different plans, we are able to construct a measure of the relative comprehensiveness of different plans that is purged of the confounding factors of selection and moral hazard that influence the actual out-of-pocket share of medical expenditures covered by each plan.

¹²These numbers are based on the average out of pocket shares by plan calculated in Table 2 and the plan shares for the 2003 - 2006 sample (not shown). Using the 2003-2004 sample's plan shares (shown in Table 2) we estimate that the move to the new options would on average raise the average out of pocket share from 12 to 25 percent.

in-network consumer coinsurance after the plan deductible is reached for all care, under the old options doctor visits and ER visits had in fact co-pays rather than coinsurance.¹³ Second, we have summarized (and model) the in-network features only. All of the plans have higher (less generous) consumer cost sharing for care consumed out of network rather than in network. We choose to model only the in-network rules (where more than 95% of spending occurs) in order to avoid having to model the decision to go in or out of network. Third, while in general the new options were designed to have higher consumer cost sharing, a wider set of preventive care services (including regular physicals, screenings, and well baby care) were covered with no consumer cost sharing under the new options.¹⁴ Finally, the least comprehensive of the new options (option 1) includes a health reimbursement account (HRA) into which the employer makes tax-free contributions that the employee can draw on to pay for out-of-pocket medical expenses, or roll over for subsequent years.

2.2 Descriptive evidence of moral hazard

Before turning to our model, we present some basic descriptive evidence of moral hazard in our setting. These findings motivate our subsequent modeling of potential selection on moral hazard. The analysis also provides a feel for the basic identification strategy for moral hazard.

Asymmetric information: the “positive correlation” property We start with the (easier) empirical task of documenting the existence of some form of asymmetric information in our data. Table 3 reports realized medical spending as a function of insurance coverage in our baseline sample. The analysis – which is in the spirit of Chiappori and Salanie’s (2000) “positive correlation test” – shows that under either the old or new options individuals who choose more comprehensive coverage have systematically higher (contemporaneous) spending. This is consistent with the presence of adverse selection and/or moral hazard in our data.

Moral hazard: difference-in-differences estimates To identify moral hazard in the data separately from adverse selection, we take advantage of the variation in the option set faced by different groups of employees. Table 4 presents this basic difference-in-differences evidence of moral hazard for our baseline sample. Specifically, we show various moments of the spending distribution in 2003 and in 2004 for the control group (employees who are covered by the old options in both years) and the treatment group (employees who are switched to the new options in 2004). The results show a

¹³Specifically they had doctor and ER co-pays of \$15 and \$75 respectively, or \$10 and \$50 depending on the plan. In practice, given the average costs of a doctor visit (~\$115) and an ER visit (~\$730) in our data, the switch from the co-pay to coinsurance did not make much difference for predicted out-of-pocket spending.

¹⁴Busch et al. (2006) and Cabral (2009) describe the treatment of preventive care in more detail, and analyze the impact of the change in benefit options on the use of preventive care. We estimate that the specific preventive care items affected by the change in benefit options account for less than 2 percent of total annual medical spending (which is the focus of our analysis).

strikingly consistent pattern across all the various moments of the spending distribution: spending falls for the treatment group, and tends to increase slightly for the control group.

The results in Table 4 also suggest slight differences in 2003 spending for the treatment group relative to the control group, although these cross-sectional differences are, for the most part, small relative to the changes over time within the treatment group. More generally, the bottom four rows of Table 1 indicate differences in demographics as well as initial spending across all four of the treatment groups. In Appendix A we therefore explore in depth the sensitivity of our difference-in-differences estimates to controlling for observable differences across employees, and also investigate the validity of the underlying identifying assumption behind the difference-in-differences estimates, namely that absent the changes in health insurance benefits these different groups would have experienced similar trends in health spending. We find these results generally quite reassuring.

Table 5 summarizes our central difference-in-differences estimates (which we then explore in more detail in Appendix A). Columns (1)-(3) show the results for our baseline 2003-2004 sample. The first column shows the difference-in-differences estimate when the dependent variable is measured in dollars. Such a specification assumes that the moral hazard effects of insurance occurs in levels. This is consistent with the model we write down in the next section. However, both because it is possible that the moral hazard effect is in fact proportional to spending, and because one may be concerned about the results being driven by a few outliers with extremely high spending, in columns (2) and (3) we investigate specifications that give rise to a proportional moral hazard effect. Given the large fraction of employees with zero spending, we cannot estimate the model in simple logs. Instead, in column (2) we report estimates from a specification in which spending, m , is measured by $\log(1 + m)$,¹⁵ and column (3) reports a quasi-maximum likelihood Poisson model.¹⁶ The results suggest that the move to the new options is associated with an economically significant decline in spending.

An important concern about the results in columns (1)-(3) is that they are not very precise. This is reflected in the large standard errors of the estimate, and in the relatively large differences in the quantitative implications of the different specifications. This lack of precision is driven by the fact that only about one-fifth of the employees in our sample are switched to the new benefits in 2004 (Table 1, column (1)). Therefore, in columns (4)-(6) we report analogous estimates from the 2003-2006 sample, during which more than half of the employees switched to the new benefits. As expected, the standard error of our estimates decreases substantially, and the quantitative implications of the results become much more stable across specifications. The estimated spending reduction is now statistically significant at the 5 percent level, with the point estimates suggesting a reduction of spending of about \$600 (column (4)) or 11-17% (columns (5) and (6)). In Appendix

¹⁵Given that almost all individuals spend at least several hundred dollars (Figure 1), the results are not sensitive to the choice of 1 relative to some other small numbers. For the same reason, the estimated coefficients can be approximately interpreted as elasticities.

¹⁶The QMLE-Poisson model requires only that the conditional mean be correctly specified for the estimates to be consistent. See, e.g., Wooldridge (2002, Chapter 19) for more discussion.

As we show that the reduction in spending appears to arise entirely through reduced doctor and outpatient spending, with no evidence of a discernible effect on inpatient spending.¹⁷

We can compute a back-of-the-envelope elasticity of health spending with respect to the out-of-pocket cost sharing by combining these estimates of the spending reduction with the estimates in Table 2 of the average cost sharing of different plans (holding behavior constant). Given the distribution of employees across the different plans, the numbers in Table 2 suggest that the change from the old options to the new options should increase the average share of out-of-pocket spending from 12.6 percent to 28.4 percent in the 2003-2006 sample. Combining the point estimate of a \$591 reduction in spending (Table 5, column (4)) with our calculation of the increase in cost sharing, our estimates imply an arc elasticity of medical spending with respect to out-of-pocket cost sharing of about -0.14.¹⁸ This is broadly similar to the widely used Rand experiment arc-elasticity of medical spending of -0.2 (Manning et al., 1987; Keeler and Rolph, 1988). Subsequent studies that have used quasi-experimental variation in health insurance plans have tended to estimate elasticities of medical spending in the range of -0.1 to -0.4.¹⁹

Heterogeneity in moral hazard: difference-in-differences estimates A necessary (but not sufficient) condition for selection on moral hazard is that there is heterogeneity in individuals' responsiveness to consumer cost sharing. To our knowledge, the experimental and quasi-experimental literature in health economics analyzing the impact of higher consumer cost sharing on spending has focused on average effects and largely ignored potential heterogeneity. This may in part reflect the fact that, because health realizations are, by their nature, partially random, testing for heterogeneity in moral hazard is not trivial. It is particularly challenging without an explicit model of the nature of moral hazard which can, for example, provide guidance as to whether the effect of consumer cost sharing is additive or multiplicative.²⁰

In the context of a model with an additive separable moral hazard effect (such as the one we

¹⁷The reduction in outpatient spending appears to occur entirely on the intensive margin, while the reduction in doctor spending may occur entirely through a reduction in doctor visits.

¹⁸We compute an arc elasticity, in which the proportional change in spending (and in consumer cost sharing) is calculated relative to the average observed across the old and new options, so that our results are more directly comparable with the existing literature. The arc elasticity is calculated as $\frac{(q_2 - q_1)/(q_1 + q_2)/2}{(p_2 - p_1)/(p_1 + p_2)/2}$ where p denotes the average consumer cost sharing rate. For the 2003-2006 sample, the proportional change in spending and cost sharing is 11% and 77%, respectively.

¹⁹See Chandra, Gruber, and McKnight (2010), who provide a recent review of some of this literature as well as one of the estimated elasticities.

²⁰Without such a model, a nonparametric test for whether there is heterogeneity in moral hazard effects is possible to construct when there is no choice in health insurance and an exogenous change in health insurance coverage. In this case, a nonparametric test can be developed by relying on the panel nature of the data and comparing the joint distribution (before and after the introduction of a new benefit) of the quantiles of medical spending for the treatment group relative to the control group; the change in individual's spending rank (i.e. the joint distribution of the quantiles of spending) in the control group provides an estimate of the variation in ranking across individuals in

develop below), homogeneous moral hazard would imply a constant (additive) change in spending for all individuals. The results in Table 4 showing the difference-in-differences estimates at different quantiles of the distribution indicate that the change in spending associated with the change in insurance options is higher at higher quantiles. Due to censoring at zero this is mechanically true (and therefore not particularly informative) at the lower spending quantiles, but even comparing quantiles above the median shows a marked pattern of larger effects at larger quantiles.²¹ Of course, since individuals may move quantiles with the change in options, this is not evidence of heterogeneity per se, but it is nonetheless suggestive.

Table 6 presents additional suggestive evidence of heterogeneous (level or proportional) moral hazard effects by reporting the difference-in-differences estimates separately for observably different groups of workers. Specifically, we show the estimated reduction in spending associated with the change from the old to the new options separately for workers above and below the median age (panel (A)), male vs. female workers (panel (B)), and above and below the median income (panel (C)) (we defer a discussion of the fourth panel until the next subsection). Of course, differences across demographic groups in the estimated reduced form effect of the change in health insurance options on spending may reflect either heterogeneous treatment effects (the object of interest) or heterogeneous treatments (i.e., greater changes in cost sharing for some groups than others, given their endogenous plan choices). To get a sense of the variation in treatment across groups, in columns (5) and (6) we report the average out of pocket share for each demographic group under the old and new options; column (7) reports the increase in the average out of pocket share associated with the change in options, which provides a measure of the treatment.

The estimates in Table 6 – while generally not precise – are suggestive of heterogeneous moral hazard. The top two rows show that the reduction in spending associated with the new options is an order of magnitude higher for older workers than for younger workers, despite a somewhat larger treatment for the younger workers (column (7)). Panel (B) indicates similar point estimates for male and female workers; although males experience a larger treatment. Similarly, panel (C) indicates similar point estimates for higher and lower income workers, but a somewhat larger treatment for higher income workers. While many of the estimates are quite imprecise, the results are suggestive of larger behavioral responses to consumer cost sharing for older workers than younger works, and perhaps for female workers relative to male workers and for lower income workers relative to higher income workers.

Selection on moral hazard: difference-in-differences estimates As discussed in the introduction, the pure comparative static of selection on moral hazard (holding all other factors that determine plan choice constant) is that individuals with a greater behavioral response to coverage

their spending to expect simply from the random nature of health realizations. However, when an endogenous plan choice is present (as in our setting), a nonparametric test for heterogeneity in moral hazard is more challenging.

²¹Kowalski (2010) finds similar patterns in her quantile treatment estimates using a different identification strategy in a different firm.

(i.e., a larger moral hazard effect) will choose greater coverage. We therefore examine whether the estimated moral hazard effect (estimated by examining the change in spending with the change from the original to the new options) is different between those who chose more vs. less coverage under the original options. Specifically, the last panel of Table 6 presents the estimated treatment effect of the move from the original to the new options separately for individuals who chose more coverage under the original options in 2003 compared to those who chose less coverage under the original options in 2003.²² Consistent with selection on moral hazard, we estimate a reduction in spending associated with the move from the old options to the new options that is more than twice as large for those who originally had more coverage than those who originally had less coverage, even though the reduction in cost sharing associated with the change in options (i.e., the treatment) is substantially larger for those who had less coverage. We do not have enough precision, however, to reject the null that estimated spending reductions are the same across the two groups. Overall, we view the findings as suggestive descriptive evidence of selection on moral hazard of the expected sign. The rest of the paper now investigates this phenomenon more formally.

3 A model of coverage choice and utilization

We now present a stylized model of individual coverage choice and health care utilization which we will then use as the main ingredient in our econometric specification and counterfactual exercises. The model is designed to allow us to isolate and examine separately three different potential determinants of coverage choice: health expectations, risk aversion, and “moral hazard type.”

We consider a two period model. In the first period, a risk-averse expected-utility maximizing individual makes an optimal health insurance coverage choice, using his available information to form his expectation regarding his subsequent health realization. In the second period, the individual observes his realized health and makes an optimal health care utilization decision, which depends on the realized health as well as on his coverage. It is this last effect which leads to what we call moral hazard. This general modeling framework is similar to the one used in existing empirical models of demand for health insurance and medical spending (Cardon and Hendel, 2001; Bajari et al., 2009; Handel, 2009; Carlin and Town, 2010).

We begin with notation. This is a model of individual behavior, so we omit i subscripts to simplify notation; in the next section, where we take the model to the data, we describe how individuals may vary. At the time of his utilization choice (period 2), an individual is characterized by two objects: his health realization λ , and his “moral hazard type” ω . The health realization

²²Specifically, we compare individuals who picked option 3 (“more coverage”) under the original options to those who picked option 2 (“less coverage”) under the original options. To do this analysis we need to limit the sample to the approximately 85 percent of the sample who was already employed at the firm by 2003 and in one of these two options. The estimated change in spending associated with the move from the old to the new options for this subsample is -859 (standard error 245), compared to -592 (standard error 264) in the full 2003-2006 sample (Table 5, column (4)).

λ captures the uncertain aspect of demand for healthcare, with individuals with higher λ being sicker and demanding greater healthcare consumption. The moral hazard type ω determines how responsive health care utilization decisions are to insurance coverage. In other words, ω affects the individual's price elasticity of demand for healthcare with respect to its (out of pocket) price, with individuals with higher ω being more price elastic and therefore increasing their utilization more sharply in response to greater insurance coverage.

At the time of coverage choice (period 1), an individual is characterized by three objects: $F_\lambda(\cdot)$, ω , and ψ . The first, $F_\lambda(\cdot)$, represents the individual's expectation about his subsequent health risk λ . It is precisely the (natural) assumption that individuals don't know λ with certainty at the time of coverage choice, which leads them to demand insurance. The second object that enters the individual's coverage choice is his moral hazard type ω , which determines his period 2 price elasticity of demand for health care. Because individuals are forward looking, they anticipate that their price sensitivity will subsequently affect their utilization choices, and this in turn affects their utility from different coverages. It is this channel that creates the potential for selection on moral hazard, which is the main focus of our paper. Finally, the third object is ψ , which captures the individual's coefficient of absolute risk aversion. Importantly, unlike ω and $F_\lambda(\cdot)$, which enter the coverage choice but also affect (deterministically and stochastically, respectively) utilization decisions, risk preferences affect coverage choice but play no direct role in utilization decisions.

Utilization choice In the second period, insurance coverage, denoted by j , is taken as given. We assume that the individual's health care utilization decision is made in order to maximize a tradeoff between health and money, with higher ω individuals putting greater weight on health. Specifically, we assume that the individual's second period utility is separable in health and money and can be written as $u(m; \lambda, \omega) = h(m - \lambda; \omega) + y(m)$, where $m \geq 0$ is the monetized utilization choice, λ is the monetized health realization, and $y(m)$ is the residual income. Naturally, $y(m)$ is decreasing in m at a rate that depends on coverage. In contrast, we assume that $h(m - \lambda; \omega)$ is concave in its first argument, so that it is increasing for low levels of utilization (when treatment presumably improves health) and is decreasing eventually (when there is no further health benefit from treatment and time costs dominate). Thus, we assume that the marginal benefit from incremental utilization is decreasing. Using this formulation, we think of λ , the underlying health realization, as shifting the level of optimal utilization m^* . Finally, we assume that $h(m - \lambda; \omega)$ is increasing in its second argument, but this is purely a normalization which (as we will see below) allows us to interpret individuals with higher ω as those who are more price elastic.

We parametrize further so that the second-period utility function is given by

$$u(m; \lambda, \omega, j) = \underbrace{\left[(m - \lambda) - \frac{1}{2\omega}(m - \lambda)^2 \right]}_{h(m - \lambda; \omega)} + \underbrace{[y - c_j(m) - p_j]}_{y(m)}. \quad (1)$$

That is, we assume that $h(m - \lambda; \omega)$ is quadratic in its first argument, with ω affecting its curvature. We also explicitly write the residual income as the initial income y minus the premium p_j associated

with coverage j and minus the out-of-pocket expenditure $c_j(m)$ associated with utilization m under coverage j . Because y and p_j are taken as given (at the time of utilization choice), it will be convenient to define

$$\tilde{u}(m; \lambda, \omega, j) = \left[(m - \lambda) - \frac{1}{2\omega}(m - \lambda)^2 \right] - c_j(m), \quad (2)$$

so that $u(m; \lambda, \omega, j) = \tilde{u}(m; \lambda, \omega, j) + y - p_j$.

Given this parameterization, the optimal utilization is given by

$$m^*(\lambda, \omega, j) = \arg \max_{m \geq 0} u(m; \lambda, \omega, j). \quad (3)$$

It will also be convenient to denote $u^*(\lambda, \omega, j) \equiv u(m^*(\lambda, \omega, j); \lambda, \omega, j)$ and $\tilde{u}^*(\lambda, \omega, j) \equiv \tilde{u}(m^*(\lambda, \omega, j); \lambda, \omega, j)$.

To facilitate intuition, we consider here optimal utilization for the case of a linear (i.e., constant coinsurance) coverage contract, so that $c_j(m) = c \cdot m$ where $c \in [0, 1]$. Full insurance is therefore given by $c = 0$ and no insurance is given by $c = 1$. The first order condition implied by the optimization problem in equation (3) is therefore given by $1 - \frac{1}{\omega}(m - \lambda) - c = 0$, or

$$m^*(\lambda, \omega, c) = \max [0, \lambda + \omega(1 - c)]. \quad (4)$$

Thus, abstracting from the potential truncation of utilization at zero, the individual will spend $m^* = \lambda$ with no insurance (i.e. $c = 1$) and $m^* = \lambda + \omega$ with full insurance (i.e. $c = 0$). Note that the utilization response to the change in coverage from full to no insurance is ω ; utilization responds more to changes in coverage for individuals of greater moral hazard type (i.e., higher ω). One way to think about this model of moral hazard, therefore, is that λ represents the non-discretionary health care shocks that individuals will always pay to treat, regardless of insurance. There is also discretionary health care utilization (such as various forms of preventive care, for example) which, without insurance will not be undertaken. With insurance, some amount of this discretionary care will be consumed, with individuals who place a higher weight on health relative to money (i.e., individuals with a higher ω) consuming more of this discretionary care when they are insured.²³

Coverage choice In the first period, the individual faces a fairly standard insurance coverage choice. As mentioned, we assume that the individual is an expected-utility maximizer, with a coefficient of absolute risk aversion of ψ . We further assume that the individual's von Neumann Morgenstern (vNM) utility function is of the constant absolute risk aversion (CARA) form, $w(x) = -\exp(-\psi x)$. In a typical insurance setting $w(x)$ is defined solely over financial outcomes. However, because moral hazard is present, individuals trade off income and health and therefore $w(x)$ is defined over the realized second-period utility $u^*(\lambda, \omega, j)$. We note that income enters $u^*(\lambda, \omega, j)$

²³We have written the model as if it is the individual who makes all the utilization decisions. In practice, many of the decisions are also affected by physicians. To the extent that physicians also respond to the individual's coverage (and they are likely to), our interpretation of moral hazard should be thought of as some combination of both the individual's and the physician's responses.

additively with a coefficient of one, so $u^*(\lambda, \omega, j)$ is monetized and can still be thought of in dollars, as in the regular case.

Consider now a set of coverage options J , with each option $j \in J$ defined by its premium p_j and coverage function $c_j(m)$. Following the above assumptions, the individual will then evaluate his expected utility from each option,

$$v_j(F_\lambda(\cdot), \omega, \psi) = - \int \exp(-\psi u^*(\lambda, \omega, j)) dF_\lambda(\lambda), \quad (5)$$

with his optimal coverage choice given by

$$j^*(F_\lambda(\cdot), \omega, \psi) = \arg \max_{j \in J} v_j(F_\lambda(\cdot), \omega, \psi). \quad (6)$$

An important modeling assumption to highlight is that, unlike the vast majority of applications that involve discrete choices, we do not add a choice-specific i.i.d. error term to the expected utility from each choice. Given the ordered nature of choices in our setting and the purely financial differences among plans, adding an additional i.i.d. error term does not seem appealing.

Measuring welfare and efficient contracts Our standard measure of consumer welfare in this context will be the notion of certainty equivalent. That is, for an individual defined by $(F_\lambda(\cdot), \omega, \psi)$, we denote the certainty equivalent to a contract j as the scalar e_j that solves $-\exp(-\psi e_j) = v_j(F_\lambda(\cdot), \omega, \psi)$, or

$$e_j(F_\lambda(\cdot), \omega, \psi) \equiv -\frac{1}{\psi} \ln \left[\int \exp(-\psi u^*(\lambda, \omega, j)) dF_\lambda(\lambda) \right]. \quad (7)$$

Our assumption of CARA utility over (additively separable) income and health implies no income effects. To see the implications of no income effects, we can substitute $u^*(\lambda, \omega, j) = \tilde{u}^*(\lambda, \omega, j) + y - p_j$ into equation (7) and reorganize to obtain

$$\begin{aligned} e_j(F_\lambda(\cdot), \omega, \psi) &\equiv \tilde{e}_j(F_\lambda(\cdot), \omega, \psi) + y - p_j \equiv \\ &\equiv -\frac{1}{\psi} \ln \left[\int \exp(-\psi \tilde{u}^*(\lambda, \omega, j)) dF_\lambda(\lambda) \right] + y - p_j, \end{aligned} \quad (8)$$

so that $\tilde{e}_j(F_\lambda(\cdot), \omega, \psi)$ captures the welfare from coverage, and residual income enters additively. Using this notation, differences in $\tilde{e}(\cdot)$ across contracts with different coverages capture the willingness to pay for coverage. For example, an individual defined by $(F_\lambda(\cdot), \omega, \psi)$ is willing to pay at most $\tilde{e}_k(F_\lambda(\cdot), \omega, \psi) - \tilde{e}_j(F_\lambda(\cdot), \omega, \psi)$ in order to increase his coverage from j to k .

Equation (8) can also be used to characterize the comparative statics of willingness to pay for more coverage with respect to the model's primitives. In general, willingness to pay for more coverage is increasing in risk aversion ψ and in risk $F_\lambda(\cdot)$ (in a first order stochastic dominance sense).²⁴ Given our specific parametrization, willingness to pay for more coverage is also increasing

²⁴These comparative statics do not always hold. The model has unappealing properties when a significant portion of the distribution of λ is over the negative range, in which case the individual is exposed to a somewhat artificial

in moral hazard type ω .²⁵

We assume that insurance providers are risk neutral, so that the provider's welfare is given by his expected profits, or

$$\pi_j(F_\lambda(\cdot), \omega) \equiv p_j - \int [m^*(\lambda, \omega, j) - c_j(m^*(\lambda, \omega, j))] dF_\lambda(\lambda), \quad (9)$$

where the integrand captures the share of the utilization covered by the provider under contract j . Total surplus s_j is then given by

$$s_j(F_\lambda(\cdot), \omega, \psi) = e_j(F_\lambda(\cdot), \omega, \psi) + \pi_j(F_\lambda(\cdot), \omega) = \tilde{e}_j(F_\lambda(\cdot), \omega, \psi) + y - \int [m^*(\lambda, \omega, j) - c_j(m^*(\lambda, \omega, j))] dF_\lambda(\lambda). \quad (10)$$

That is, total surplus is simply certainty equivalent minus expected cost.

Finally, it may be useful to characterize the nature of the efficient contract in this setting. Because of our CARA assumptions, premiums are a transfer which do not affect total surplus. Therefore, the efficient contract can be characterized by the efficient coverage function $c^*(\cdot)$ that maximizes total surplus (as given by equation (10)) over the set of possible coverage functions. Such optimal contracts would trade off two offsetting forces. On the one hand, an individual is risk averse while the provider is risk natural, so optimal risk sharing implies full coverage, under which the individual is not exposed to risk. On the other hand, the presence of moral hazard makes an insured individual's privately optimal utilization choice socially inefficient; any positive insurance coverage makes the individual face a healthcare price which is lower than the social cost of healthcare, leading to excessive utilization. Efficient contracts will therefore resolve this tradeoff by some form of partial coverage (Arrow, 1971; Holmstrom, 1979). For example, it is easy to see that no insurance ($c^*(m) = m$) is efficient if individuals are risk neutral or face no risk ($F_\lambda(\cdot)$ is degenerate), and that full insurance ($c^*(m) = 0$) is efficient when moral hazard is not present ($\omega = 0$). In all other situations, the efficient contract is some form of partial insurance.

uninsurable (background) risk (since spending is truncated at zero). We are not particularly concerned about this feature, however, as our estimated parameters do not give rise to it, and because we have experimented with a (non-elegant) modification to the model that does not have this feature, and the overall results were similar.

²⁵In a more general model, ω is associated with two effects. One is the increased utilization, which increases willingness to pay. The second effect is the increased flexibility to adjust utilization as a function of the realized uncertainty (λ), which in turn reduces risk exposure and reduces willingness to pay for insurance. Our specific parameterization was designed to have spending under no insurance unaffected by ω ; this eliminates this latter effect, and therefore makes the comparative statics unambiguous.

4 Econometric model: specification, estimation, and identification

4.1 Specification

We now turn to specify a more complete econometric model that is based on the simple model of individual coverage choice and utilization developed in the preceding section. This will allow us to jointly estimate coverage choices and utilization, relate the estimated parameters of the model to underlying economic objects of interest, and quantify how spending and welfare may be affected under various counterfactuals. The additional modeling assumptions in this section are of two different natures. First, we will need to specify more parametrically some of the objects introduced earlier (e.g., individuals' beliefs $F_\lambda(\cdot)$). Second, we need to specify how and what form of heterogeneity we allow across individuals, and for a given individual over time.

Our unit of observation is an employee i , in a given year t . We abstract from the specifics of the timing and nature of claims, and, as we have done so far, simply code utilization m_{it} as the total medical spending (in dollars) for the entire year. The individual faces the choice set of either the original plan options or the new plan options (as described in Table 2), depending on the year and the employee's union affiliation, which dictates whether and when he was switched to the new benefits options.

Using the model of Section 3, recall that individuals are defined by three objects: their beliefs about their subsequent health status $F_\lambda(\cdot)$, their moral hazard parameter ω , and their risk aversion ψ . We assume that ω_i and ψ_i may vary across employees, but are constant for a given employee over time. It is the potential heterogeneity in ω_i which is the focus of the paper. We also assume that $F_\lambda(\cdot)$ is a lognormal distribution with parameters $\mu_{\lambda,it}$, $\sigma_{\lambda,i}$, and $\kappa_{\lambda,i}$. Thus, beliefs about health also vary across employees, and we also allow $\mu_{\lambda,it}$ to be time varying to reflect the possibility that information about one's health evolves with time.

At the time of coverage choice individuals believe that

$$\log(\lambda_{it} - \kappa_{\lambda,i}) \sim N(\mu_{\lambda,it}, \sigma_{\lambda,i}^2), \quad (11)$$

and these beliefs are correct. Assuming a lognormal distribution for λ is natural, as the distribution of annual health expenditures is highly skewed with a fat tail. The additional parameter $\kappa_{\lambda,i}$ is used in order to capture the significant fraction of individuals who have no spending over an entire year. When $\kappa_{\lambda,i}$ is negative, the support of the implied distribution of λ_{it} is expanded, allowing for λ_{it} to obtain negative values, which in turn implies (when ω_i is not too large) zero spending. The parameter $\sigma_{\lambda,i}$ indicates the precision of the individual's information about his subsequent health.

It is the heterogeneity in $\mu_{\lambda,it}$, $\sigma_{\lambda,i}$, and $\kappa_{\lambda,i}$ that gives rise to the traditional form of adverse selection on the basis of expected health, i.e. on the basis of expected λ (denoted $\bar{\lambda}$) which is given by

$$\bar{\lambda}(\mu_\lambda, \sigma_\lambda, \kappa_\lambda) = \exp\left(\mu_\lambda + \frac{1}{2}\sigma_\lambda^2\right) + \kappa_\lambda. \quad (12)$$

That is, higher $\mu_{\lambda,it}$, $\sigma_{\lambda,i}$, or $\kappa_{\lambda,i}$ are all associated with higher expected λ , which all else equal leads to greater expected medical spending and greater cost by the insurance provider.²⁶ All else equal, individuals with higher $\mu_{\lambda,it}$, $\sigma_{\lambda,i}$, or $\kappa_{\lambda,i}$ also prefer to choose greater coverage, thus giving rise to adverse selection.

Let x_{it} denote a vector of observables which are taken as given, and let \bar{x}_i denote their within-individual average. In order to link the latent variables to observables, we make several parametric assumptions. First, we assume that $\log \omega_i$, $\log \psi_i$, and $\overline{\mu_{\lambda,i}}$ (which denotes the average (over time) of $\mu_{\lambda,it}$ for a given individual i) are drawn from a jointly normal distribution, such that²⁷

$$\begin{pmatrix} \overline{\mu_{\lambda,i}} \\ \log \omega_i \\ \log \psi_i \end{pmatrix} \sim N \left(\begin{pmatrix} \bar{x}_i \beta_\lambda \\ \bar{x}_i \beta_\omega \\ \bar{x}_i \beta_\psi \end{pmatrix}, \begin{pmatrix} \sigma_\mu^2 & \sigma_{\mu,\omega} & \sigma_{\mu,\psi} \\ \sigma_{\mu,\omega} & \sigma_\omega^2 & \sigma_{\omega,\psi} \\ \sigma_{\mu,\psi} & \sigma_{\omega,\psi} & \sigma_\psi^2 \end{pmatrix} \right). \quad (13)$$

We then assume a random effects structure on μ_{it} , so that μ_{it} varies over time, but is correlated within an employee, such that

$$\mu_{\lambda,it} = \overline{\mu_{\lambda,i}} + (x_{it} - \bar{x}_i) \beta_\lambda + \epsilon_{\lambda,it}, \quad (14)$$

where $\epsilon_{\lambda,it}$ is an i.i.d. normally distributed error term, with variance σ_ϵ^2 . The variance of $\mu_{\lambda,it}$ is then $\sigma_\mu^2 = \sigma_{\bar{\mu}}^2 + \sigma_\epsilon^2$. Finally, we assume that

$$\sigma_{\lambda,i}^{-2} \sim \Gamma(\gamma_1, \gamma_2) \mathbf{1}\{\sigma_{\lambda,i}^2 \leq \bar{\sigma}^2\} \quad (15)$$

and that

$$\kappa_{\lambda,i} \sim N(\bar{x}_i \beta_\kappa, \sigma_\kappa^2). \quad (16)$$

That is, $\sigma_{\lambda,i}^2$ is drawn from a right truncated inverse gamma distribution,²⁸ and $\kappa_{\lambda,i}$ is drawn from a normal distribution, and both are drawn independently from the other latent variables.

Thus, overall we estimate four vectors of mean shifters ($\beta_\lambda, \beta_\omega, \beta_\psi, \beta_\kappa$), eight variance and covariance parameters ($\sigma_\mu, \sigma_\epsilon, \sigma_\omega, \sigma_\psi, \sigma_\kappa, \sigma_{\mu,\omega}, \sigma_{\mu,\psi}, \sigma_{\omega,\psi}$), and two additional parameters (γ_1, γ_2) that determine the distribution of $\sigma_{\lambda,i}^{-2}$. Of course, an important decision is what observables \bar{x}_i shift which primitive, and whether we would like any observables to be excluded from one or more of the (four) equations. To pay particular attention to the underlying variation emphasized in Section 2, in all the specifications we experiment with, we include in \bar{x}_i treatment group fixed effects for each of the four treatment groups (see Table 1), as well as a year fixed effect on $\mu_{\lambda,it}$, the only time varying latent variable. We also include coverage tier fixed effects since both the choice sets and spending varies substantially by coverage tier (see Table 2 and Figure 1, respectively).

²⁶Note that expected medical spending of an individuals is closely related but not identical to $\bar{\lambda}$, since both moral hazard and the restriction that spending be non negative create a wedge between expected medical spending and expected health (see, e.g., equation (4)).

²⁷For notational simplicity we consider \bar{x}_i to be the superset of covariates, and implicitly assume some coefficient restrictions if we allow for different mean shifters for different latent variables.

²⁸We truncate the distribution of $\sigma_{\lambda,i}^{-2}$ because the untruncated distribution causes the unconditional distribution of λ_{it} to have no moments.

4.2 Estimation

We estimate the model using Markov Chain Monte Carlo (MCMC) Gibbs sampling. The multi-dimensional unobserved heterogeneity naturally lends itself to such methods, as the iterative sampling allows us to avoid evaluating multi-dimensional integrals numerically, which is computationally cumbersome. The key observation is that the model we developed is sufficiently flexible so that we can augment the latent variables into the model and formulate a hierarchical statistical model. To see this, let $\theta_1 = \{\beta_\lambda, \beta_\omega, \beta_\psi, \beta_\kappa; \sigma_\mu, \sigma_\varepsilon, \sigma_\omega, \sigma_\psi, \sigma_\kappa, \sigma_{\mu,\omega}, \sigma_{\mu,\psi}, \sigma_{\omega,\psi}; \gamma_1, \gamma_2\}$ be the set of parameters we are interested in, and let $\theta_2 = \{\lambda_{it}, \mu_{\lambda,it}, \sigma_{\lambda,i}, \kappa_{\lambda,i}, \omega_i, \psi_i\}_{i=1, t=2003}^{i=N, t=2004}$ be the set of employee-year latent variables. The model is set up so that, even conditional on θ_1 , we can always rationalize the observed data – namely, plan choice and medical utilization – by appropriately finding a set of latent variables for each individual, θ_2 .

Thus, the iterative procedure is straightforward. We can first sample from the distribution of θ_1 conditional on θ_2 . Because, conditional on θ_2 , there is no additional information in the data about θ_1 this part of the sampling is simple and quite standard. Then, we can sample from the distribution of θ_2 conditional on θ_1 and the information available in the data. This latter step is of course more customized toward our specific model, but does not introduce any conceptual difficulties. The full sampling procedure, the specific prior distributions we impose, and the resultant posteriors are described in detail in Appendix B. We verified using Monte Carlo simulations that the procedure seems to work quite effectively, and is pretty robust to initial values. For our baseline results, the estimation seems to converge after about 5,000 iterations of the Gibbs sampler, so we drop the first 10,000 draws and use the last 10,000 draws of each variable to report our results. The results we report are based on the posterior mean and posterior standard deviation from these 10,000 draws.

One important difficulty that our model introduces is related to our decision to not allow for an additive separable plan-specific error term. It is extremely common in applications of discrete choice (such as ours) to add such error terms, and often to assume that they are distributed i.i.d. across plans and individuals. Such error terms serve two important roles. First, they allow the researcher to rationalize any choice observed in the data through a large enough error term. Second, their independence makes the objective function of any M-estimator smooth, which is computationally attractive for numerical optimization. In the context of our application, however, we view such error terms as economically unappealing. The options from which individuals in our sample choose are financially rankable and are identical in their non-financial features. This makes one wonder what such error terms would capture that is outside of our model. The clear ranking of the options also makes the i.i.d. nature of the error terms not very appealing. Instead, we introduce a fair amount of heterogeneity along the other dimensions of our model. Some of this heterogeneity (e.g., the heterogeneity in $\sigma_{\lambda,i}$ and $\kappa_{\lambda,i}$) is richer than the minimum required to capture the key economic forces we would like to capture, but this richness is what allows us to rationalize all observed choices in the data. This still leads to a model which is not very attractive for numerical optimization, which is one important reason why we use Gibbs sampling.

4.3 Identification

We briefly discuss the identification of the model. Conditional on the individual-behavior model described in Section 3, the object of interest that we seek to identify is the joint distribution of $F_\lambda(\cdot)$, ω , and ψ . We have data on individuals' health insurance options, choices, and medical spending. Throughout the paper we make the strong assumption that individuals beliefs (about their subsequent λ) are correct.²⁹ The model and its identification share many properties with some of our earlier work on insurance (Cohen and Einav, 2007; Einav, Finkelstein, and Schrimpf, 2010). The key novel element is that we now allow for moral hazard, and heterogeneity in it. The panel structure of the data and the staggered timing of the introduction of the new options are key in allowing us to identify this new element. We organize our discussion of identification in two steps. We first consider nonparametric identification of our model with ideal data, and then discuss the ways in which our actual data is different from the ideal, thus requiring us to make additional parametric assumptions that aid in identification.

Identification with ideal data The two features of our data set that are instrumental for identification are the panel structure of the data and the exogenous change in the health insurance options available to employees. In the ideal setting, we consider a case in which we observe individuals for a sufficiently long period before and a sufficiently long period after the change in coverage. Moreover, we assume that the choice set from which employees can choose coverage is continuous (for example, one can imagine a continuous coinsurance rate, and an increasing and differentiable mapping from coinsurance rate to premium).

In such a setting, our model is non-parametrically identified. To see this, note that such data provide us with two medical expenditure distributions, $G_i^{before}(m)$ and $G_i^{after}(m)$, for each individual i . Using the realized utility model (during the second period of the model), these two distributions allow us to recover for each individual $F_{i,\lambda}(\cdot)$ and ω_i . To see this, recall that abstracting from the truncation of medical spending at zero, our model implies that medical expenditure m_{it} is equal to $\lambda_{it} + \omega_i(1 - c_t)$. If $F_{i,\lambda}(\cdot)$ is stable over time,³⁰ one can regress (for each employee i separately) m_{it} on a dummy variable that is equal to 1 after the change. The estimated coefficient on the dummy variable would be then an estimate of $\omega_i(c_{after} - c_{before})$, providing an estimate of ω_i . The distribution of λ_{it} can then be recovered by observing that $\lambda_{it} = m_{it} - \omega_i(1 - c_t)$, which is known.

Conditional on $F_{i,\lambda}(\cdot)$ and ω_i , individual i 's choice from a continuous set of options provides a

²⁹While it is reasonable to question this assumption, absent direct data on beliefs some assumption about beliefs is essential for identification. Otherwise, it is not possible to distinguish beliefs from other preferences that only affect choices, such as risk aversion (see Einav, Finkelstein and Schrimpf (2010) for a more detailed discussion of this point). While we could instead assume some other (pre-specified) form of biased beliefs, correct beliefs seem like a natural starting point.

³⁰If $F_{i,\lambda}(\cdot)$ changes over time, one could parameterize, identify, and estimate the autocorrelation structure with a sufficiently long panel. We therefore treat $F_{i,\lambda}(\cdot)$ as stable over time throughout this section.

unique mapping from choices to his coefficient of absolute risk aversion since – conditional on $F_{i,\lambda}(\cdot)$ and ω_i – the coefficient of risk aversion is the only unknown primitive that may shift employees’ choices, and it does so monotonically. Thus, using information about $F_{i,\lambda}(\cdot)$ and ω_i and individual i ’s choice from the continuous option set,³¹ we can recover ψ_i . Since we recovered $F_{i,\lambda}(\cdot)$, ω_i , and ψ_i for each employee, we can now combine these estimates for our entire sample, and obtain the joint distributions of $F_\lambda(\cdot)$, ω , and ψ .

Identification with our specific data Our actual data depart from the ideal data described above in two main ways. First, although we have a panel structure, we only observe individuals for two periods in the baseline sample (that is limited to 2003 and 2004). Second, the choice set is highly discrete (including three to five options) rather than continuous. We thus make additional parametric assumptions to aid us in identification. This implies that our identification in the actual estimation cannot rely anymore on identifying the individual-specific parameters employee-by-employee. Rather, we observe a distribution of medical expenditures before the change and a distribution for medical expenditure after the change. We then identify the model by comparing the distribution after with the distribution before.

We can now think first about the identification of moral hazard. A comparison of spending distributions before and after a change in health insurance options may be contaminated by other confounders that change over time. Therefore, analogously to the difference-in-differences strategy of the reduced form (Section 2), we use the majority of the sample for which the options did not change during our sample period as a control group. We can therefore conceptually think of identification in our baseline sample as if we follow a *stable* population before and after a treatment, using the control population to adjust for any time-varying effects.

To gain intuition for our identification of moral hazard, consider a set of individuals who chose the same sequence of plans in 2003 and 2004. Of course, this is a selected subset of the population, a point that we will return to below. Without moral hazard, the distribution of medical expenditures for this group, before and after the change, would have remained the same. With moral hazard, spending under the new, say, lower coverage plan is lower. Loosely, and abstracting from truncation of spending at zero, the overall difference in the level of spending identifies the average moral hazard effect. Since our model implies that the moral hazard parameter affects spending additively, the extent of heterogeneity in moral hazard is identified by the difference in the distributions, quantile-by-quantile.

Once the distribution of moral hazard, ω_i , is known, the remaining identification challenge is very similar to our earlier work (Cohen and Einav, 2007; Einav, Finkelstein, and Schrimpf, 2010). Conditional on the distribution of ω_i , our data provide information about coverage choices and subsequent realizations. By assuming that $F_{i,\lambda}(\cdot)$ follows a lognormal distribution, we can map the data on choices and spending to the remaining primitives of risk aversion ψ_i and risk types

³¹This can be done using either the options set before the change or after. In fact, the ideal data leads to over identification, so could allow us to test or enrich the model.

$F_{i,\lambda}(\cdot)$. Intuition for this is perhaps most easily seen in two steps (although in practice it is more efficient to estimate all parameters simultaneously, as we do). The observed distribution of medical spending (net of the known moral hazard) provides information on the distribution of health risk $F_{i,\lambda}(\cdot)$; conditional on health risk and moral hazard, the choice of insurance identifies risk aversion (ψ_i). We assume a three-dimensional heterogeneity in $F_{i,\lambda}(\cdot)$ – in mean μ , variance σ , and offset κ . Loosely, the distribution of the mean is primarily identified by the first moment of the spending distribution, the distribution of the variance by the second moment, and the distribution of κ is primarily driven by the extent of zero spending across different choices

Two difficulties still remain. First, the choice set is discrete, so choices can only map to intervals of risk aversion. Second, while the distribution of ω_i across individuals is known, the specific value of ω_i is not known for each individual. Here, the parametric assumption regarding the joint normal distribution of $\log \omega_i$, $\overline{\mu_{\lambda,i}}$, and $\log \psi_i$ is useful, as it allows us to integrate over all possible values within each such choice interval. The final step is to repeat a similar argument for each observed sequence of choices, which together aggregate to the joint distribution of the population as a whole, which is the object we wish to identify.

5 Results

5.1 Parameter estimates

Table 7 presents the estimated parameters from estimating the model on the baseline sample of 7,572 employee-years. The top panel presents the estimated coefficients on the mean shifters of the four latent variables: $\mu_{\lambda,it}$ and $\kappa_{\lambda,i}$ that affect health risk, ω_i that affects moral hazard, and ψ_i that captures risk aversion. The middle panel report the estimated variance-covariance matrix and additional parameters. At the bottom we report some implied quantities of interest that are derived from the estimates.

Overall, the estimates imply (bottom panel of Table 7) an average realized health risk of about \$5,600 per employee-year. We estimate an average moral hazard parameter (ω) that is about 15 percent as large as health risk, or about \$820 dollar; by way of context, recall that ω is approximately the size of the spending effect as we move individuals from no insurance to full insurance.^{32,33}

The signs of the covariates seem generally sensible. Looking at the top panel, our estimates

³²Abstracting from truncation of spending at 0, with no insurance individuals spend λ while with full insurance they spend $\lambda + \omega$. See equation 4.

³³We estimate an average coefficient of absolute risk aversion of about 0.0016, but caution against trying to compare this to existing estimates. In our model, realized utility is a function of both health risk and financial risk, while in other papers that estimate risk aversion from insurance choices (e.g., Cohen and Einav, 2007; Handel, 2009) realized utility is only over financial risk. Thus, the estimated “level” of risk aversion is not directly comparable; indeed, one could add a separable health related component to utility that is affected only by λ to change the risk aversion estimates, without altering anything else in the model.

imply that employees with single coverage are associated with lower expected health realizations relative to employees who cover their spouse or their entire family, who (together with their covered dependents) are associated with greater (aggregate) medical shocks. Employees who cover only their children are in between. The effect of coverage tier on moral hazard is similar, with employees who cover their spouse and their entire families showing the largest moral hazard effect. This likely reflects the fact that in our model moral hazard type (ω) is measured in absolute (dollar) terms rather than relative to health risk, so individuals with greater health risk have more opportunities to exercise moral hazard. Finally, employees who also cover dependents are estimated to have lower levels of (absolute) risk aversion. The rest of the covariates in our baseline specification capture group and year dummy variables.

We estimate statistically significant and economically large heterogeneity in each one of the components: health risk, moral hazard, and risk aversion. The magnitude of the heterogeneity is most easily gauged by examining the bottom panel of Table 7. Our estimates indicate a standard deviation for realized health risk (λ) of about \$40,000, or a coefficient of variation of over 7. Moral hazard (ω) is also estimated to be highly heterogenous, with a standard deviation across employees of about \$2,400, or a coefficient of variation of about 3. Finally, we estimate a coefficient of variation for absolute risk aversion (ψ) that is about one.

The correlations (see middle panel) are all statistically significant, and their signs seem reasonable. The correlation between health risk and moral hazard is positive, presumably reflecting the same point discussed earlier, that sicker individuals face more opportunities to exercise discretion with respect to utilization behavior. The correlation between risk aversion and health risk (and moral hazard) is negative, perhaps reflecting the fact that individuals who are more risk averse are also those who take better care of their health. A similar pattern was documented by Finkelstein and McGarry (2006) in the context of long-term care insurance. Finally, as may be expected, we estimate a strong correlation in $\mu_{\lambda,it}$ over time, greater than 0.8 (not shown), suggesting that much of an individual's health risk is persistent over time, for example due to chronic conditions.

5.2 Model fit

In Table 8 we report the actual and predicted plan choice probabilities. We fit the choices of employees who are choosing from the original plan options remarkably well. The fit of the choices from the new options is also reasonable, but not as good as the fit for the original options. This is likely because there are many fewer employees in the baseline sample who are subject to the new options. Thus, to the extent that the same model attempts to rationalize the choices from both the old and new options, it is natural that more weight is given to trying to fit choices from the old menu, leading to slightly worse fit for those choosing from the new menu.³⁴

³⁴In light of this, we experimented with an alternative sample that adds the non-unionized hourly employees to the baseline sample; all of the non unionized hourly employees were switched to the new options in 2004. There are approximately 10,000 non union hourly employees per year (compared to the approximately 4,000 unionized hourly employees per year in our baseline sample) so this sample addition substantially increases the proportion and absolute

Figure 2 reports the actual and predicted distributions of medical expenditure. The top panel reports the fit for the individuals facing the old options, and the bottom panel reports the fit for the individuals facing the new options. Overall, the fit is quite reasonable. For example, predicted median spending is within about 10 percent of actual median spending under both the original and new options. The most noticeable discrepancy is that our estimates generates over dispersion. We therefore generate fatter right tails of spending, which – due to the lognormal distribution – lead us to over predict expected spending by about 25 percent.³⁵

In this context, it is important to recognize that our decision to not add i.i.d. error terms to the plan choice model makes it more difficult to fit the plan choices and the medical expenditure at the same time. With i.i.d. error terms, the estimation procedure moves the precision of the i.i.d. error terms in order to explain very different choices without altering parameters that govern the medical expenditures. In our model this cannot be done, so dispersed choices (e.g., a non-negligible fraction choosing options 1 and 5 in the new options, with very few individuals choosing options 2 and 3) must be rationalized using dispersed distribution of expected spending, which in turn leads to some of the over dispersion in spending that we predict.

Finally, we note that our model estimates about an 8 percent reduction in spending associated with moving from the old option set to the new option set. This is broadly similar to the difference-in-differences estimates we obtained for the same sample (Table 5, columns (1)-(3)). However, given how imprecise our difference-in-differences estimates are for the 2003-2004 sample, we caution against making too much of any comparison.

5.3 Moral hazard estimates

The parameter ω_i captures moral hazard in our model. Recall that, abstracting from the truncation of spending at zero, employee i would spend λ_{it} if he had no insurance, and with full insurance would spend $\lambda_{it} + \omega_i$. Thus, ω_i can be thought of as the scope for moral hazard. As discussed, the bottom panel of Table 7 reports that the estimated average of ω_i is about 820 dollars, or about 15% of the estimated health risk (the average of λ_{it}).

Table 9 reports an alternative way one could quantify moral hazard. In the top row of the table, we calculate each employee’s expected decline in medical expenditure as we move him from the highest to the lowest coverage in the new options. We will feature the move (or choice) between these two options in all of our subsequent counterfactual exercises. Recall that, as we have modeled these options, moving from the highest to the lowest coverage primarily entails moving someone

number of people in the new options in 2004. We find, not surprisingly, that the fit of the model for the new options improves, while the fit for the old options worsens slightly; other aspects of the model fit are roughly similar. As in our baseline model, we find evidence of heterogeneity in moral hazard and substantial selection on moral hazard, with higher moral hazard types disproportionately selecting higher coverage (results not shown).

³⁵To conserve on space, Figure 2 pools individuals across coverage tiers, but the fit within singles or non-singles looks similar to the results pooled by coverage tier, and the predicted differences in spending between singles and non-singles are similar to the observed ones.

from a plan with no deductible to a plan with a high deductible, specifically a \$3,000 deductible for non-single coverage, or \$1,500 for single coverage (Table 2). We estimate that the average spending effect from this move is \$270. The second row reports a similar exercise, but considers moving individuals from full insurance to no insurance. We estimate an average spending reduction of \$790; this is slightly lower than the average ω_i of \$822 reported earlier (see Table 7) precisely because of the truncation of spending at zero.

These economically meaningful estimates of moral hazard satisfy one necessary condition for selection on moral hazard – the focus of our paper – to be important. A second necessary condition is that moral hazard be heterogeneous. Indeed, we find important heterogeneity in our moral hazard estimates across individuals. For example, the estimated variance of $\log(\omega)$ is about 2, and highly statistically significant (Table 7), implying that an employee who is one standard deviation above the mean has about four times greater ω and an employee who is one standard deviation below the mean has about four times smaller ω . As shown in the last panel of Table 7, across individuals, the standard deviation of ω_i is more than \$2,400, and the coefficient of variation of ω is almost 3.

Again, Table 9 reports more empirically-motivated measures of heterogeneity in moral hazard. The top row shows that the spending decline as we move individuals from the no deductible plan to the high deductible plan has a standard deviation of \$571, compared to the mean of \$270. The median spending reduction is only \$37, while the 90th percentile exhibits an almost \$800 reduction. Similarly, as we move individuals from full insurance to no insurance, we estimate that the median reduction in spending is \$237, but the 90th percentile of the spending reduction distribution is almost \$1,800.

5.4 Selection on moral hazard compared to other factors

The fact that individuals are heterogeneous in their moral hazard response to coverage does not of course mean that they select on it in any quantitatively meaningful way. That is, it is conceivable that heterogeneity in other factors is more important in determining plan choice. As one way to gauge the quantitative importance of selection on moral hazard, we examine how the choice of coverage varies with the quantiles of the marginal distribution of moral hazard ω , and compare this to how the choice of coverage varies with the quantiles of the marginal distribution of risk aversion ψ , and of expected health risk $E(\lambda)$. Once again, we focus on the choice between the highest coverage and lowest coverage plan in the new options (see Table 2). Loosely, our exercise resembles the introduction of a high deductible health insurance plan into a setting where previously there was only a no deductible plan. We set the premiums so that, on average, 10 percent of our sample chooses the high deductible plan.

Figure 3 reports the results. It shows the fraction of individuals choosing the high deductible coverage, conditional on the individual being in each quantile of the marginal distribution of moral hazard ω , of risk aversion ψ , and of expected health risk $E(\lambda)$. We present two different sets of results. The top panel presents the pattern while taking as given the underlying correlation structure among these objects, as well as the variation of observables (x 's) across the quantiles of

each object. This panel can be thought of as giving the empirical answer to the question of how much selection there is, on net, on each of the latent primitives that we model. Given the flexible correlations we allow for, these patterns are a-priori of ambiguous sign. The bottom panel repeats the same exercise but fixes observables at their sample average and “shuts down” the effect of the correlation structure by drawing values for the other two latent factors independently of the value of the factor for which the graph is drawn. This panel can be thought of as giving the answer to the conceptual comparative static exercise of how much selection there is on one factor, holding all else equal (both on observables and unobservables). As discussed previously, demand for higher coverage generally increases in expected health risk, in risk aversion, and in moral hazard. Our purpose here is to assess the relative magnitudes. Taken together, the two panels help inform not only whether empirically there is selection on moral hazard and of what sign (top panel) but also the extent to which any such selection is primarily “direct” selection based on moral hazard rather than “indirect” selection arising from the correlation structure between moral hazard and other factors which may be driving plan choice.

The results in the top panel indicate that empirically there is selection on moral hazard of the expected sign, with higher moral hazard types (higher ω) less likely to choose the high deductible plan. In terms of the substantive importance of this selection, both panels reveal a similar qualitative pattern: selection on moral hazard is substantially larger than selection on risk aversion and of similar magnitude to selection on health risk. For example, the top panel indicates that moving from the 10th percentile to the 90th percentile of the moral hazard distribution is associated with about a 22 percentage point decline in the demand for the high deductible plan, while moving from the 10th to the 90th percentile of the expected health distribution is associated with about a 28 percentage point decline in the demand for the high deductible plan. While some of this reflects the correlation structure across observables and unobservables in our data, the “pure” comparative static shown in the bottom panel produces quite comparable magnitudes. This suggests that much of this selection on moral hazard is “direct” selection. In other words, in making plan choices, individuals select not only based on their expected level of spending that they would incur with no insurance, but also on their expected slope, or incremental spending due to insurance.

By contrast, we find selection on risk aversion considerably less important than selection on either moral hazard or expected health. In our data there is very little variation in demand for the high deductible plan across the centiles of the risk aversion distribution (reflecting various correlations), and even the “pure” comparative static suggests only about a 10 percentage point range between the 10th and 90th percentile.

6 Some implications for spending and welfare

In the final section of the paper we perform some counterfactual analyses designed to shed some light on the potential implications of the selection on moral hazard that we estimate. We focus on the potential implications of selection on moral hazard for two types of analyses: the impact on

spending of offering plans with higher consumer cost sharing, and the impact on welfare of policies aimed at reducing selection.

6.1 Implications for spending

We investigate the implications of the selection on moral hazard that we detect for attempts to combat moral hazard through higher consumer cost sharing. To this end, we perform counterfactual analyses of the spending reduction associated with introducing a lower coverage option. Given our finding that higher “moral hazard types” prefer greater coverage, introducing plans with greater consumer cost sharing will produce less of a spending reduction than would be estimated if selection on moral hazard were ignored, and it were assumed that those who select the lower coverage option are drawn at random from the “moral hazard type” distribution.

In the health care sector, the impact of consumer cost sharing on moral hazard is an issue of considerable policy as well as academic interest. The size and rapid growth of the health care sector, and the pressure this places on public sector budgets, has created great interest among both academics and policymakers in possible approaches to reducing health care spending. Encouraging individuals to enroll in plans with higher consumer cost sharing, such as the tax-advantaged Health Savings Accounts (HSAs) designed to increase enrollment in high deductible plans, is seen as one potentially promising approach to reducing health spending.

To examine the implications of selection on moral hazard for analysis of such efforts, Figure 4 engages in the same exercise as in Figure 3 of giving employees in our sample a choice between the no deductible and high deductible health insurance plans in the new options. In Figure 3 we fixed the price of each option and reported the fraction of each quantile who choose each plan. In Figure 4 we instead gradually increase the (relative) price of the higher coverage (no deductible) option, and ask how selected is the group of employees who endogenously select the lower coverage (high deductible) option at each given price. To show the extent of selection, the figure reports the average per employee decline in annual spending for those employees who move from the no deductible plan to the high deductible plan at each price.

The figure illustrates strong selection on moral hazard, especially when the share of the high deductible plan is small. For example, when the price of the no deductible coverage is low enough so that only 10 percent of the employees select the high deductible coverage, the average (per employee) spending decline for those who select the high deductible plan instead of the no deductible plan is just over \$100. By contrast, were all employees to choose the high deductible plan instead of the no deductible plan, we estimate the per employee spending decline would be \$270. As noted in the introduction, the common practice in the literature on health insurance and moral hazard is to look for experimental variation that randomly moves individuals across plans. Such variation would recover the unconditional average effect of coverage (which is \$270 in our context); this does not account for selection on moral hazard and will therefore substantially over-estimate the spending reduction associated with the introduction of the high deductible plan when only a small share of individuals select it.

This selection reflects the earlier observation that, all else equal, individuals that are associated with higher moral hazard (higher ω_i) have higher willingness to pay for insurance, and are therefore the ones that would be the last to switch to the lowest coverage, as we gradually increase the price of highest coverage. It is somewhat interesting that in our setting the selection on moral hazard becomes less important (i.e., the slope of the line in Figure 4 becomes less steep) at higher levels of prices for the no deductible plans (which leads to greater fractions choosing the high deductible plan). The same underlying forces are still in play, but are offset by the correlation structure with other primitives.

6.2 Implications for welfare

Our findings of selection on moral hazard also have implications for policies aimed at reducing selection. Analysis of how to mitigate selection often focuses on risk adjustment – whereby individual’s insurance premiums are adjusted on the basis of individual covariates (such as age, gender, and prior health conditions) that are predictive of expected medical spending. From this perspective, the potential for selection on moral hazard suggests that investments in better monitoring technologies – such as coinsurance that varies across diagnoses (e.g., heart attack vs. headache) or types of healthcare (e.g., prescription drugs vs. inpatient services) with different behavioral responsiveness to insurance – may also be effective for ameliorating adverse selection at the same time that they combat moral hazard.

Our final set of counterfactual analyses considers these issues of contract design by using our model to go further out of sample to analyze the impact of alternative contract designs on social welfare. Specifically, we consider two extreme cases: perfect screening and perfect monitoring. Under perfect screening, we assume that insurers can observe and price on all the determinants of expected healthcare use that the individual can observe at the time of his choice. Under perfect monitoring, we assume that insurance providers are able to perfectly observe the realization of λ – i.e. the individual’s ex post health – and to only reimburse spending that is associated with λ without reimbursing the moral hazard component of spending, ω . Of course, neither extreme case is realistic in practice. Nonetheless, policies that accomplish some screening or some monitoring are routinely used and our extreme counterfactuals provide one way to gauge the maximum potential of such contract designs, as well as the maximum potential contribution of eliminating selection on moral hazard to the total welfare gains from these designs.

Table 10 reports our results. Once again we restrict our attention to a choice between the no deductible and high deductible plans under the new options (Table 2, options 5 and 1 respectively). Throughout this section we make the simplifying assumption of perfect competition for the incremental coverage among providers of the no deductible plan, so that the incremental price of the no deductible plan breaks even for those who provide it: incremental price is equal to incremental cost.³⁶ We report the implications of various counterfactual contracts for the equilibrium (incre-

³⁶We normalize the price of the lower coverage option to zero. Given our assumptions of CARA utility and a realized utility that is additively separable in income, the price level does not affect plan choice or welfare.

mental) premium for the no deductible plan, the share choosing this plan, expected spending per employee, and total welfare (or surplus) per employee. Our primary focus is on the consequence of different contract designs for total welfare (i.e. the sum of consumer welfare and producer welfare) which in our context is the certainty equivalent minus expected costs (see equation (10)).

The first row presents the “status quo” benchmark contract with no (additional) screening or monitoring. As with the observed contracts in our data, individuals are offered a “uniform” price that only varies by coverage tier, and insurance companies reimburse medical spending, regardless of its origin, based on their contract rules. We estimate that the competitive, average incremental price for the no deductible plan is about \$2,700, and that at this competitive price about two-fifths of the employees select the no deductible plan. Note that the observed incremental prices of the no deductible plan in the data are lower (and the probability of choosing this plan correspondingly higher) than what we estimate to be the competitive price (see Table 2), reflecting the subsidies associated with employer-provided benefits. We normalize total welfare per employee in this “status quo” benchmark to be zero, so that we can more easily compare the welfare gains from alternative contract designs.

The second row presents our “perfect screening” counterfactual, which eliminates adverse selection. Specifically, we assume that insurers can observe and price on all the determinants of health care utilization that the individual knows at the time of his insurance choice – i.e., all of the components of $F(\lambda)$ as well as ω . We solve for the incremental price of the no deductible plan that breaks even for each employee individually, thereby eliminating the adverse selection that arises from uniform pricing. The results indicate that, as expected, the elimination of adverse selection leads to lower prices, increased coverage (i.e., greater fraction choosing the no deductible plan), and higher welfare. It also leads to higher expected spending as more individuals are covered by the no deductible plan and therefore spend more. We estimate the welfare gain per employee from eliminating adverse selection to be about \$458. By way of perspective, we calculate the total surplus from perfect screening relative to everyone being in the high deductible plan to be \$1,065, so that adverse selection appears to cut the surplus from offering the no deductible plan almost in half.

Of particular interest is the contribution of eliminating selection on moral hazard to the welfare gain from eliminating selection. Row 3 explores this by reporting the welfare gain from eliminating only selection on moral hazard (ω) but continuing to allow selection on health risk $F(\lambda)$. Specifically, we allow insurers to observe ω and price on it, but not on $F(\lambda)$. This is of course not a very sensible scenario, since presumably if insurers could observe ω they could also refuse to reimburse on it, and thus eliminate moral hazard entirely (not just selection on moral hazard). But it is a conceptually useful way to examine the welfare cost of different sources of selection. The results in Row 3 suggest that the welfare cost of selection on moral hazard is \$45, or about 10 percent of the \$458 total welfare cost of selection from Row 2.

The fourth row presents our “perfect monitoring” counterfactual, which eliminates moral hazard. Here we assume that insurance coverage only applies to “ λ -related” spending, which in the context of our model means that instead of reimbursing based on actual spending (i.e., reimbursing $m - c_j(m)$), the contracts reimburse $\max\{\lambda, 0\} - c_j(\max\{\lambda, 0\})$ regardless of what the actual

spending is. In such situations, optimizing individuals would spend $\max\{\lambda, 0\}$, which would be the socially efficient level of spending. Row 4 of Table 10 indicates that this elimination of moral hazard reduces spending by about \$550 per employee. It also indicates that relative to the status quo (no screening or monitoring), eliminating moral hazard through perfect monitoring increases welfare by \$211 per employee, or by about half as much as the welfare gain associated with eliminating adverse selection through perfect screening (row 2).³⁷

Again, we are particularly interested in the relative contributions to this welfare cost of moral hazard of selection on moral hazard compared to the “traditional” moral hazard inefficiency that comes through inefficient spending choices. To examine this, in Row 5 we again consider an artificial counterfactual. Specifically, we assume that individuals make their contract choices in the first period as if they are faced with the “perfect monitoring” contracts (Row 4), but then in the second period make their spending decision faced with the observed contracts that reimburse in the same manner as the actual contracts (i.e., reimburse based on m rather than based on λ). This allows us to isolate the welfare gain from eliminating solely selection on moral hazard, while preserving the distortion in second period consumption caused by moral hazard. The results suggest that eliminating selection on moral hazard can achieve a little over one-quarter of the \$211 welfare gain from elimination of moral hazard (Row 4).

Overall, these results suggest that, in our setting, selection on moral hazard contributes non-trivially to both the welfare cost of selection and the welfare cost of moral hazard. Of course, our estimates undoubtedly depend on our specific setting (contracts and population). Nonetheless, at a broad level, our findings suggest that in thinking about contract design, traditional approaches to combatting moral hazard may well aid in combatting selection, and vice versa.

7 Conclusions

This paper takes a first step toward marrying empirical analysis of selection with that of moral hazard. The active (and growing) empirical literature on insurance demand has focused almost exclusively on selection on risk type or risk preferences, and largely abstracted from moral hazard.³⁸ The large and venerable literature on moral hazard in insurance has largely focused on average moral hazard effects, abstracting from potential heterogeneity as well as potential selection on that heterogeneity. In this paper we introduced the (to our knowledge) previously overlooked potential for selection on moral hazard, or in other words, the possibility that individuals’ anticipated behavioral response to insurance contracts affects their contract choice.

We explored the existence, nature, and implications of selection on moral hazard empirically in the context of the employer-provided market for health insurance in the United States. We

³⁷We also explored the results for a “first best” (within these set of contracts) counterfactual, which assumes both perfect screening and perfect monitoring. We found that relative to the status quo, the first best raises total welfare by \$743 per employee.

³⁸See Einav, Finkelstein and Levin (2010) for a recent discussion of this literature.

estimate substantial heterogeneity in moral hazard and selection on it, with individuals who have a greater behavioral response to the contract (i.e., greater “moral hazard type”) demanding more coverage. We estimate that “moral hazard type” is roughly as important as health expectations in determining whether to buy a low deductible plan. In other words, selection based on the expected slope of spending (i.e., incremental spending due to insurance) appears about as quantitatively important in our setting as “traditional” selection based on the expected level of spending (i.e., health risk type). Such selection on moral hazard can have important implications for traditional analysis of either selection or moral hazard. For example, we estimate that if we ignored selection on moral hazard, we could estimate a spending reduction associated with introducing a high deductible plan that is substantially larger than what we estimate when we account for the fact that those who select the high deductible plan have a disproportionately low behavioral response to such cost sharing.

Needless to say, our quantitative estimates are highly specific to our particular population and our particular counterfactual analyses. Nonetheless, at a broad level, they illustrate the potential importance of selection on moral hazard for understanding the welfare consequences of both selection and moral hazard. They also illustrate some of the potential implications of selection on moral hazard for policies designed to ameliorate these welfare costs. They suggest, for example, that efforts to reduce health spending by introducing health insurance options with high consumer cost sharing – such as the high deductible plans available through Health Savings Accounts – may produce substantially smaller spending reductions than would have been expected based on the existing estimates of moral hazard effects in health insurance which has ignored selection on moral hazard. They also suggest that improvements in monitoring technology – traditionally thought of as a way to reduce moral hazard – may have the ancillary benefit of ameliorating some of the welfare costs of selection.

Given the importance of the topic, we hope that future work will explore selection on moral hazard in other contexts and in other ways. As noted, we know of very little work that even examines heterogeneity in moral hazard effects, let alone selection of insurance on this heterogeneity. Both the approaches taken in this paper and those suggested (but not explored) by Einav, Finkelstein and Cullen (2010, Section III.D) for estimating heterogeneity in moral hazard effects and its correlation with demand should be fruitful to apply in other settings. In addition, our analysis has focused exclusively on the spending and welfare implications of selection on moral hazard for a given set of contracts; it would be interesting to consider, both theoretically and empirically, the implications of selection on moral hazard for richer analyses of contract designs.

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Appendix A: Additional descriptive results on moral hazard

In this appendix we report in more detail on the results of our difference-in-differences analysis of the impact of the change in health insurance options on healthcare spending and utilization. Specifically, we estimate the impact of the change in coverage separately for different types of healthcare utilization, investigate the validity of our identifying assumption, and explore a number of other additional potential concerns with the analysis. All of the results shown are for the 2003-2006 sample.

Econometric framework The basic difference-in-differences specification (which we used in Tables 5 and 6) is:

$$y_{ijt} = \alpha_j + \delta_t + \beta \cdot Treat_{jt} + \mathbf{x}'_{ijt}\boldsymbol{\phi} + \varepsilon_{ijt}, \quad (17)$$

where y_{ijt} is the outcome variable of interest for employee i in treatment group j at time t . We classify each employee i into one of four possible treatment groups – “switched in 2004,” “switched in 2005,” “switched in 2006,” and “switched later” – based on his union affiliation which determines the year in which he is switched to the new set of health insurance options. The coefficients α_j represent a full set of treatment group fixed effects; these control for any fixed differences across treatment groups. The vector of δ_t ’s represents a full set of year fixed effects; these control (flexibly) for any common secular year-to-year changes across all treatment groups.³⁹ The vector x denotes a set of employee demographic covariates that are included in some of our

³⁹An annual measure is a natural unit of time since it is both the unit of time during which the set of health insurance incentives apply (i.e., cost sharing requirements reset at the beginning of the year) and the time over which the choice of health insurance contract is made. In some additional analysis below we also report results at the

specifications; there are no such covariates in our baseline specification. We adjust the standard errors to allow for an arbitrary variance-covariance matrix within each of the 28 different unions in our sample.⁴⁰

The main coefficient of interest is β , the coefficient on the variable $Treat_{jt}$. The variable $Treat_{jt}$ is an indicator variable that is equal to 1 if group j is offered the new health insurance options in year t , and 0 otherwise. For example, for the group “switched in 2004” $Treat_{jt}$ is 0 in 2003, and 1 in 2004 and subsequent years, while for the “switched later” group the variable $Treat_{jt}$ is 0 in all years.

Impact on types of medical spending and care utilization Appendix Table A1 examines the impact of the change in health insurance options on the various components of health care spending and health care utilization. We can break out health care spending into doctor visits (approximately 25 percent of the total), outpatient spending (approximately 35 percent of the total), inpatient spending (approximately 35 percent of the total), and other (which accounts for about 4 percent of spending, about half of which is due to emergency room visits). Column (1) shows our baseline results for 2003-2006 for total spending (i.e., Table 5, column (4)). It indicates that the change from the old health insurance options to the new health insurance options was associated with, on average, a \$591 (11 percent) reduction in annual medical spending.

Columns (2) through (5) show estimates separately for spending on doctor visits, spending on outpatient visits, spending on inpatient visits and other spending. We detect a statistically significant decline in annual doctor spending of \$220 (15 percent) and in annual outpatient spending of \$310 (16 percent). The point estimates for inpatient spending suggest a statistically insignificant decline in inpatient spending of \$117 (6 percent).

In addition to spending, we are able to measure utilization on the extensive margin. We define doctor visits as the total number of doctor visits by anyone in the household covered by the insurance (limited to a maximum of one per day). On average, an employee has 12 doctor visits for covered members in a given year. Outpatient visits are defined in an identical manner, where the average is 3 outpatient visits per year. We also code an indicator variable for whether there are any inpatient hospitalizations for anyone insured over the year; on average 14 percent of the employees have an inpatient hospitalization in a given year.

Columns (6) through (8) show the estimated effects on these measures of utilization. We estimate that the change in health insurance options is associated with a statistically and economically significant decline in the average number of annual doctor visits 1.9 (16 percent). Given the average cost of a doctor visit in our data of about \$115, it is possible that the decline in spending on doctor visits comes entirely on the extensive margin. There is no evidence of an economically or statistically significant impact of the change in health insurance options on outpatient visits or inpatient hospitalization. The estimated decline in outpatient spending therefore presumably reflects a decrease in the intensity of treatment (i.e., spending conditional on the visit).

quarterly level, which allows for a finer examination of pre- and post-period dynamics.

⁴⁰Ideally, we would allow for an arbitrary variance-covariance matrix within each of the four treatment groups, but we are concerned about small sample biases with such few clusters (Cameron, Gelbach, and Miller, 2010). Below we report alternative results aggregated to the treatment group level in which we estimate the model by Generalized Least Squares (GLS) and allow for both heteroskedasticity as well as treatment-group specific auto-correlation parameters. These tend to produce similar point estimates and smaller standard errors relative to our baseline specification.

Validity of identifying assumption The identifying assumption in interpreting the difference-in-differences β coefficient from equation (17) as the causal impact of the change in health insurance options on the outcome of interest is that absent the change in health insurance options, employees in the different treatment groups would have otherwise experienced similar changes in their healthcare utilization or spending. Employees who are switched at different times differ in some of their demographics as well as in their 2003 (pre period) spending (see Table 1). Such observable differences across the treatment groups is not a problem per se for our difference-in-differences analysis which uses group fixed effects and therefore controls for any time-invariant differences across the treatment group. It naturally, however, raises concerns about the validity of our identifying assumption.

We undertake two types of analysis designed to help shed light on the likely validity of the identifying assumption. First, as our most direct investigations, we examine whether outcomes were trending similarly across the different groups in the periods prior to the change in health insurance options. These results are quite reassuring; there is no evidence of any substantively or statistically significant declines in spending in the several quarters prior to the change in health insurance options. Second, as a more indirect investigation, we also examine the sensitivity of our baseline results to controlling for observable characteristics of the employees. Again, it is quite reassuring that the basic OLS estimate in the 2003-2006 sample is not particularly sensitive to controlling for observable worker characteristics.

Dynamics. To compare pre-period trends across the treatment groups we disaggregated the data from the annual to the quarterly level (so that t now denotes quarters rather than years) and estimate:

$$y_{ijt} = \alpha_j + \delta_t + \beta \cdot Treat_{jt} + \phi \cdot Treat_{jt,0} + \varepsilon_{ijt} \quad (18)$$

where $Treat_{jt,0}$ is an indicator variable for whether it is the quarter before group j is switched to the new health insurance options. The variable $Treat_{jt,0}$ acts as a pre-specification test; it will be informative of whether there are any differential trends in the outcome variables of interest across different treatment groups *before* the change in health insurance options. We estimate equation (18) at the quarterly rather than annual level primarily because at the annual level we would not be able to estimate pre period trends for the first treatment group (who is switched in 2004) which is roughly one-fifth of our sample, as there is only one year (2003) of pre data for this group. Another advantage of the quarterly specification is that it allows us to test for anticipation effects which presumably are most likely to occur immediately prior to the switch.⁴¹

Appendix Table A2 reports the results from estimating equation (18). In the interest of brevity, we report results for total spending only; results from components of spending (or utilization) are broadly similar (not shown). Column (1) reports the results from estimating equation (18) without the pre-period specification variable $Treat_{jt,0}$. It is therefore the exact analog of equation (17) but at the quarterly level rather than annual level. Correspondingly, therefore, the estimated coefficient on $Treat_{jt}$ is one-quarter the level of what we estimated in column (4) of Table 5. Column (2) of Table A2 shows the results when the pre-period variable $Treat_{jt,0}$ is included in the regression. The estimated main effect (the coefficient

⁴¹In specifications at the quarterly level the δ_t represent a full set of quarter-of-year fixed effects rather than year fixed effects.

on $Treat_{jt}$) is virtually unaffected by the inclusion of this additional variable, although the standard error increases noticeably. More importantly, the coefficient on the pre-period specification test variable $Treat_{jt,0}$ is the opposite sign, statistically insignificant, and less than one-third the magnitude of the main effect. This goes some way toward assuaging concerns that the estimated effect is just picking up differential trends across groups.

A potential concern with quarterly level data is that results may be much more sensitive to outliers. To investigate this concern, in columns (3) and (4) we repeat the analysis in columns (1) and (2) but censor the dependent variable at the 99th percentile. Comparing columns (1) and (3), we see very similar point estimates on the estimated treatment effect (-148 in the uncensored estimate in column (1) and -157 in the censored estimate in column (3)) but a substantially lower standard error (65.76 vs. 43.62); this comparison is consistent with little or no economic incentive effect at the 99th percentile and therefore the introduction of noise from including the estimates above this point.⁴² The pre-specification test on the censored data in column (4) shows a virtually identical main effect to the censored estimate in column (3), however now the pre period effect is not only statistically insignificant but substantively trivial (with a coefficient of -0.331 (standard error = 69) it is about two orders of magnitude smaller the main effect with a coefficient of -157). Finally, in column (5), as a further check on the validity of the identifying assumption, we re-estimate equation (18) with the addition of treatment-group specific linear trends; this allows each treatment group to be on a different (linear) trend over the 2003-2006 period and investigates whether the switch in health insurance options is associated with a change in spending for the treatment group relative to its average trend, relative to the changes in spending experienced at the same calendar time by other treatment groups relative to their own trends. The fact that the main estimate remains quite similar in magnitude is consistent with the evidence that these groups are not in fact on very different trends which are driving the estimated effect of the change in health insurance.

To more thoroughly examine the full range of pre-period dynamics, as well as to examine the dynamics in the timing of the post-period in any impact of the change in health insurance regime on the outcomes of interest, we also estimate a more flexible version of this quarterly specification that includes a full set of dummies for the number of quarters it has been since (or until) the switch. Specifically, we estimate

$$y_{ijt} = \alpha_j + \delta_t + \sum_{k=-11}^{12} \lambda_k Switch_{ijt,k} + \varepsilon_{ijt}, \quad (19)$$

where $Switch_{ijt,k}$ is an indicator variable for whether individual i is in a group j which at time t is k quarters away from the switch in health insurance options. The period $k = 1$ corresponds to the first quarter in which the group is under the new health insurance options, while $k = 0$ corresponds to the quarter right before the switch to the new health insurance options, etc. Thus, for example, for the “Switched in 2004” group,

⁴²The 99th percentile of the spending distribution is \$57,500 for non-single coverage and \$29,600 for single coverage. This level exceeds the out-of-pocket maximum on all plans with any non trivial mass except for the lowest coverage option (option 1) under the new plan options (see Table 2). Censoring the data at a spending level above the out of pocket maximum of the lowest coverage plan is conceptually valid since any spending above this amount cannot be affected by the cost-sharing features of the plan, except via income effects. To the extent that our censoring level is lower than the highest out of pocket maximum, censoring the dependent variable should bias downward our estimated effect of increased cost sharing. In practice, the results in Appendix Table A2 do not suggest any substantive downward bias.

$Switch_{ijt,1}$ is turned on (equal to 1) in the first quarter of 2004, while $Switch_{ijt,-3}$ is turned on the first quarter of 2003, and $Switch_{ijt,12}$ is turned on in the last quarter of 2006; for the “Switched later” group, all $Switch_{ijt,k}$ variables are set to 0. We examine periods from $k = -11$ (i.e., 12 quarters or 3 years before the switch) through $k = 12$ (i.e., 12 quarters or 3 years after the switch) although of course not all treatment groups can be used in identifying each of these periods (a point we return to below).

The coefficients of interest are the time pattern on the $\lambda'_k s$, the coefficients on the $Switch_{ijt,k}$ indicators. Column (6) of Table A2 shows the coefficients on the λ_k 's from estimating equation (19) on the outcome variable of total spending. We show (and focus our attention on) only the four quarters before and four quarters after the switch, since these are all identified off of the full sample; by contrast, coefficients further removed from $k = 0$ are identified off of only some of the groups; as a result, the time pattern at longer intervals potentially conflates the true time pattern with heterogeneous treatment effects across the groups identifying different coefficients.⁴³ We observe two interesting (and reassuring) features of the time pattern. First, we can see that the decline in spending after the switch to the new regime happens pretty much instantaneously. This is reassuring as the timing of the effect suggests that we are estimating the effect of the change in plans, rather than some confounding factor. Second, there is no systematic trend in spending in the quarters before the switch for select relative to other groups with other timing; while the pattern is admittedly quite noisy it is relatively flat. This is re-assuring in further supporting the likely validity of the identifying assumption that absent this change in plans, the different groups would have been on similar trends in spending.

Sensitivity to covariates. An alternative way to shed light on the likely validity of the identifying assumption is to explore the sensitivity of the results to the inclusion of covariates. Appendix Table A3 explores these issues. This analysis is all done at an annual level. Column (1) replicates the baseline results from Table 5, column (4). Column (2) of Table A3 shows the results with the addition of controls for coverage tier; this specification is shown to mimic the one we used in our baseline modeling approach below. Column (3) adds controls for a wider set of employee demographic characteristics: in addition to whether they have single coverage, we control for their age, gender, the number of dependents insured on the policy, whether they are white, the number of years they have been at Alcoa, and their annual salary. The results in columns (1) through (3) indicate the results are not sensitive – in either magnitude or precision – to controlling for employee demographics; the baseline estimate of a \$591 decline in spending associated with the move to the new PPO options changes to a \$523 or \$537 when the controls are added. As a stronger set of controls, we can include individual fixed effects for employees in the sample for more than one year. Column (4) shows the baseline results limited to the approximately half of employees who are in our data in all four years. The point estimate of the decline in spending associated with the move to the new PPO options is noticeably larger (\$966) in this subsample, presumably reflecting heterogeneity in treatment effects and/or the treatment (i.e., plan selection) itself. More interestingly for our purposes, column (5) shows that the point estimate is unaffected (\$966) by the inclusion of individual fixed effects in this subsample. Overall, we

⁴³For example, employees in the “Switched in 2006” group do not contribute to the identification of the parameter estimates beyond the third quarter under the new policy, while individuals in the “Switched in 2004” group do not contribute to the identification of the parameter estimates beyond the third quarter prior to the policy.

view the robustness of our results to various inclusions of covariates as reassuring with respect to the validity of the identifying assumption.

Additional sensitivity analyses Finally, Appendix Table A4 explores a variety of additional concerns and sensitivity analysis. One concern, noted earlier, is with sample selection. Specifically, we excluded from our analysis the 11 percent of employees who choose to opt out of insurance or choose the HMO option (available in all years and to all our employees) rather than one of the PPO options we study. To the extent that the new PPO options were more or less attractive to employees – in either their benefit design and/or their pricing – this raises concerns that our treatment variable (the offering of the new PPO options) could affect selection out of our sample and thus bias our estimates. To investigate this, we added back in the excluded individuals and re-estimated equation (17) for the binary dependent variable of whether the employee chose a non PPO option (i.e., is excluded from our baseline sample). The results indicate that the new options are associated with a statistically insignificant and economically small 2.1 percentage point decline in the probability of an employee choosing a non PPO option. We suspect this reflects the fact that the excluded options are sufficiently horizontally differentiated from the PPO options that they are largely determined by other factors (outside insurance options, taste for HMO plan, etc.) and thus not that sensitive on the margin to redesigns of the PPO options; consistent with this, in Einav, Finkelstein, and Cullen (2010) we find that variation in the relative prices of the five new PPO options also does not have an economically or statistically significant association with the decision to choose one of these non PPO options. This is also consistent with Handel (2009)’s finding – in the context of a different employer provided health insurance setting – that individuals in a PPO are unlikely to subsequently choose an HMO when the set of HMO and PPO options change.

Another concern noted above was the treatment of the standard errors. Our baseline specification adjusts for an arbitrary variance-covariance matrix within each of the 28 unions (whose contracts determine which of the four treatment groups the employee is in). To investigate the sensitivity of our estimates to this approach, we follow the estimation approach pursued by Chandra, Gruber and McKnight (2010) in a similar context. Specifically, we aggregate our employee-level data to the treatment group level and estimate the treatment group by quarter data using Generalized Least Squares (GLS), with a treatment-group specific auto correlation parameter and variance. Column (3) of Table A4 reports the results of this estimation; for comparison purposes, column (2) reproduces the results of the quarterly OLS estimation of the employee-level regression, with clustering at the union level (see Table A2, column (1)). We are reassured that these two specifications yield not only similar point estimates (-\$147.8 in column (2) and -\$164.4 in column (3)) but also very similar standard errors; indeed, the standard errors are slightly smaller in the GLS specification than in our baseline OLS specification.

Appendix B: Sampling algorithm

Throughout, we will let Y denote the data. $\Theta = (\theta_1, \theta_2)$ is the set of parameters. We will write $\Theta_{-\beta}$ for all the parameters except β . We will use the following notation for the variance of the latent variables:

$$V \begin{pmatrix} \omega \\ \psi \\ \mu_{\lambda,i,2003} \\ \mu_{\lambda,i,2004} \end{pmatrix} = \Sigma = \begin{pmatrix} \sigma_\omega^2 & \sigma_{\omega,\psi} & \sigma_{\mu,\omega} & \sigma_{\mu,\omega} \\ \sigma_{\omega,\psi} & \sigma_\psi^2 & \sigma_{\mu,\psi} & \sigma_{\mu,\psi} \\ \sigma_{\mu,\omega} & \sigma_{\mu,\psi} & \sigma_\mu^2 & \sigma_{\mu_{03},\mu_{04}} \\ \sigma_{\mu,\omega} & \sigma_{\mu,\psi} & \sigma_{\mu_{03},\mu_{04}} & \sigma_\mu^2 \end{pmatrix}. \quad (20)$$

Suppose now that we have some initial draws of the parameters. We sample each parameter conditional on the others and the data as follows.

- Draw $\beta = (\beta_\omega, \beta_\psi, \beta_\lambda, \beta_\kappa) | \Theta_{-\beta}, \omega_i, \psi_i, \lambda_{it}, \mu_{it}, \sigma_i, Y$. Given $\omega_i, \psi_i, \lambda_{it}, \mu_{it}, \sigma_i, \kappa_i$, the vector β does not enter the density of the data. Spending depends only on (λ_{it}, ω_i) and plan choices depend only on $(\mu_{\lambda, it}, \sigma_i, \kappa_i, \omega_i, \psi_i)$. Therefore, the distribution of $\beta | \Theta_{-\beta}, \omega_i, \psi_i, \lambda_{it}, \mu_{\lambda, it}, \sigma_i, \kappa_i, Y$ does not depend on Y . Leaving out the prior for now, the posterior of β is:

$$\begin{aligned} f(\beta | \Theta_{-\beta}, \omega_i, \psi_i, \lambda_{it}, \mu_{\lambda, it}, \sigma_i, \kappa_i) &\propto \prod_{i=1}^N f(\lambda_{it} | \mu_{\lambda, it}, \sigma_i, \omega_i, \psi_i, \theta_{-\beta}, \beta) f(\mu_{\lambda, it}, \sigma_i, \omega_i, \psi_i | \theta_{-\beta}, \beta) \\ &\propto \prod_{i=1}^N e^{-\frac{1}{2} \left(\frac{\log(\lambda_{it} - \kappa_i) - \mu_i}{\sigma_i} \right)^2} \exp \left[-\frac{1}{2} (u_i - x_i \beta)'^{-1} (u_i - x_i \beta) \right] f(\sigma_i | k, \theta) \\ &\propto \exp \left(-\frac{1}{2} (\beta - \hat{\beta})' (X'^{-1} \otimes I_N) X (\beta - \hat{\beta}) \right) \end{aligned} \quad (21)$$

where

$$\begin{aligned} u_i &= (\log \omega_i, \log \psi_i, \mu_{i,2003}, \mu_{i,2004}, \kappa_i) & \underbrace{U}_{5N \times 1} &= \begin{pmatrix} \log \omega \\ \log \psi \\ \mu \end{pmatrix} \\ \underbrace{\beta}_{k_\omega + k_\psi + k_\lambda + k_\kappa \times 1} &= [\beta_\omega, \beta_\psi, \beta_\lambda, \beta_\kappa] & \underbrace{X}_{5N \times k_\omega + k_\psi + k_\lambda + k_\kappa} &= \text{diag} \left(x^\omega, x^\psi, \begin{pmatrix} x_{2003}^\lambda \\ x_{2004}^\lambda \end{pmatrix}, x^\kappa \right) \end{aligned} \quad (22)$$

and

$$\hat{\beta} = (X'^{-1} \otimes I_N) X^{-1} (X'^{-1} \otimes I_N) U \quad (23)$$

Hence, with a diffuse prior, the posterior of β is simply

$$N(\hat{\beta}, (X'^{-1} \otimes I_N) X^{-1}) \quad (24)$$

With a $N(\beta_0, V_0)$ prior, the posterior of β would be

$$N(\bar{\beta}, (X'^{-1} \otimes I_N) X + V_0^{-1})^{-1} \quad (25)$$

with

$$\bar{\beta} = (X'^{-1} \otimes I_N) X + V_0^{-1})^{-1} \left(X'^{-1} \otimes I_N \right) U \hat{\beta} + V_0^{-1} \beta_0 \quad (26)$$

- Draw $\Sigma | \Theta_{-\Sigma}, Y$. In order to impose the restrictions on Σ above (for example, that $\text{cov}(\mu_{\lambda,2003}, \omega) = \text{cov}(\mu_{\lambda,2004}, \omega)$ and $\text{cov}(\mu_{\lambda,2003}, \psi) = \text{cov}(\mu_{\lambda,2004}, \psi)$), we sample Σ in various pieces. To do this, it is useful to define α as the coefficient from regressing $\mu_{\lambda, it} - x_{it}^\lambda \beta_\lambda$ on $\log \omega - x^\omega \beta_\omega$ and $\log \psi - x^\psi \beta_\psi$. That is,

$$\alpha = \begin{pmatrix} \alpha_\omega \\ \alpha_\psi \end{pmatrix} = \begin{pmatrix} \sigma_\omega^2 & \sigma_{\omega,\psi} \\ \sigma_{\omega,\psi} & \sigma_\psi^2 \end{pmatrix}^{-1} \begin{pmatrix} \sigma_{\omega,\mu} \\ \sigma_{\psi,\mu} \end{pmatrix} \quad (27)$$

Using this notation, we can write

$$\mu_{\lambda,it} - x_{it}^\lambda \beta_\lambda = \alpha_\omega (\log \omega_i - x_i^\omega \beta_\omega) + \alpha_\psi (\log \psi_i - x_i^\psi \beta_\psi) + \epsilon_{it} \quad (28)$$

Where ϵ_{it} is normally distributed and independent of $\log \omega - x^\omega \beta_\omega$ and $\log \psi - x^\psi \beta_\psi$. We parameterize the variance of $(\epsilon_{i,2003}, \epsilon_{i,2004})$ as

$$V \begin{pmatrix} \epsilon_{i,2003} \\ \epsilon_{i,2004} \end{pmatrix} = \frac{\sigma_\epsilon^2}{1 - \rho^2} \begin{pmatrix} 1 & \rho \\ \rho & 1 \end{pmatrix} \quad (29)$$

That is, we think of ϵ as coming from an AR(1) process. Note that for $T = 2$, as in our baseline model, specifying that ϵ follows an AR(1) process carries no restriction – we could just as well simply say that ϵ has some variance matrix. However, our sampling algorithm and code are written for generic T , and for $T \geq 3$, the AR(1) assumption is a meaningful restriction.

- Draw $\Sigma_{\omega,\psi} = \begin{pmatrix} \sigma_\omega^2 & \sigma_{\omega,\psi} \\ \sigma_{\omega,\psi} & \sigma_\psi^2 \end{pmatrix} | \Theta_{-\Sigma}, Y$. As above, the posterior of $\Sigma_{\omega,\psi}$ given the latent variables and the data does not depend on the data. Standard calculations show that if the prior for $\Sigma_{\omega,\psi}$ is $IW(A, m)$ then its posterior is $IW(n\hat{\Sigma}_{\omega,\psi} + A, n + m)$ where

$$\hat{\Sigma}_{\omega,\psi} = \frac{1}{n} \sum \begin{pmatrix} \log \omega_i - x_i^\omega \beta_\omega \\ \log \psi_i - x_i^\psi \beta_\psi \end{pmatrix} \begin{pmatrix} \log \omega_i - x_i^\omega \beta_\omega \\ \log \psi_i - x_i^\psi \beta_\psi \end{pmatrix}' \quad (30)$$

- Draw $\alpha | \Theta_{-\alpha}, \omega_i, \psi_i, \lambda_{it}, \mu_i, \sigma_i, Y$. As above, the posterior of α given the latent variables and the data does not depend on the data. Ignoring any prior for now, the posterior is

$$\begin{aligned} f(\alpha) | \Theta_{-\alpha}, \omega_i, \psi_i, \lambda_{it}, \mu_i, \sigma_i &\propto \prod_{i=1}^N \prod_{t=1}^T f(\lambda_{it} | \mu_i, \sigma_i, \omega_i, \psi_i, \Theta_{-\alpha}, \alpha_{\omega,\psi}) \times \\ &\times f(\mu_i | \omega_i, \psi_i, \Theta_{-\alpha_{\omega,\psi}}, \alpha) f(\sigma_i | k, \theta) f(\omega_i, \psi_i | \Theta) \\ &\propto \prod_{i=1}^N \prod_{t=1}^T \exp \left(-\frac{1}{2} \left(\frac{\tilde{y}_{it} - \tilde{x}_i \alpha}{\alpha_\epsilon / \sqrt{1 - \rho^2}} \right)^2 \right) \end{aligned} \quad (31)$$

where $\tilde{y}_{it} = (\mu_{it} - x_{it}^\lambda \beta_\lambda)$ and $\tilde{x}_i = \begin{pmatrix} \log \omega_i - x_i^\omega \beta_\omega \\ \log \psi_i - x_i^\psi \beta_\psi \end{pmatrix}$. The usual calculations would show that if the prior for α is $N(b_0, V_0)$, then the posterior is:

$$N \left(((1 - \rho^2) \alpha_\epsilon^{-2} X' X + V_0)^{-1} ((1 - \rho^2) \alpha_\epsilon^{-2} X' Y + V_0 b_0), ((1 - \rho^2) \alpha_\epsilon^{-2} X' X + V_0)^{-1} \right) \quad (32)$$

where X is $(\tilde{x}_1, \dots, \tilde{x}_N)'$ repeated twice, and Y is $(\tilde{y}_{1,2003}, \dots, \tilde{y}_{N,2003}, \tilde{y}_{1,2004}, \dots, \tilde{y}_{N,2004})'$.

- Draw $\sigma_\epsilon^2 | \Theta_{-\sigma_\epsilon^2}, \omega_i, \psi_i, \lambda_{it}, \mu_i, \sigma_i, Y$. The same reasoning as for α shows that with a $\Gamma(a_1, a_2)$ prior, the posterior of σ_ϵ^{-2} is $\Gamma \left(N + a_1, 1 / \left(\frac{1 - \rho^2}{2} \sum_{it} (\tilde{y}_{it} - \tilde{x}_i \alpha_{\omega,\psi})^2 + 1 / a_2 \right) \right)$.
- Draw $\rho | \Theta_{-\rho}, \mu_{it}, \omega_i, \psi_i, \lambda_{it}, \sigma_i, Y$. As above, the posterior of ρ given the latent variables and the data does not depend on the data. The distribution of ρ given the latent variables is proportional

to

$$\begin{aligned}
f(\rho|\mu_{it}\omega_i, \psi_i, \lambda_{it}, \sigma_i, \Theta_{-\rho}) &\propto \prod_{i,t} f(\mu_{it}|\rho, \omega_i, \psi_i, \Theta_{-\rho}) \\
&\propto \prod_{i=1}^N \sqrt{1-\rho^2} \exp\left(-\frac{1}{2}(1-\rho^2)\epsilon_{i1}^2\right) \prod_{t=2}^T \exp\left[-\frac{1}{2}(\epsilon_{it} - \rho\epsilon_{i,t-1})^2\right] \\
&\propto (1-\rho^2)^{N/2} \exp\left[-\frac{1}{2}(\rho - \hat{\rho})' \left(\sum_{i=1}^N \sum_{t=2}^T \epsilon_{i,t-1}^2\right) (\rho - \hat{\rho})\right]
\end{aligned} \tag{33}$$

where $\hat{\rho} = \frac{\sum_{i=1}^N \epsilon_{i1}^2 + 2\sum_{t=2}^T \sum_{i=1}^N \epsilon_{it}\epsilon_{i,t-1}}{2\sum_{i=1}^N \sum_{t=2}^T \epsilon_{i,t-1}^2}$, so ρ has the density of a normal truncated to $[-1, 1]$ and scaled by $(1-\rho^2)^{N/2}$.⁴⁴ We sample from it using a metropolis sampler with candidate density,

$$N(\rho_{\text{current}}, N^{-1/2}) \tag{34}$$

This leads to an acceptance rate between 0.3 and 0.5 for a wide range of sample sizes.

- Draw $\lambda_{it}, \omega_i | \Theta_{-\lambda, -\omega}, Y$. This means drawing λ, ω from the region that rationalizes the observed choices and spending. The likelihood of the latent variables given spending m and choice j is:

$$f(\lambda_i, \omega_i | \Theta_{-\lambda, -\omega}) \propto \prod_{t=1}^T e^{-\frac{1}{2}\left(\frac{\log \lambda_{it} - \mu_{it}}{\sigma_i}\right)^2} e^{-\frac{1}{2}\left(\frac{\log \omega_i - m_i^o}{s^o}\right)^2} \mathbf{1}(j^*(\omega, \psi, \mu, \kappa, \sigma) = j) \mathbf{1}(m^*(\lambda, \omega) = m) \tag{35}$$

where $m_i^o = x_i^\omega \beta_\omega + (\mu_i - x_i^\lambda \beta_\lambda) \delta_\mu + (\log \psi - x_i^\psi \beta_\psi) \delta_\psi$ and $s^o = \sqrt{\sigma_\omega^2 - S_{\omega, (\mu, \psi)} \Sigma_{\mu, \psi}^{-1} S_{(\mu, \psi), \omega}}$ with $\delta = \Sigma_{\mu, \psi}^{-1} S_{(\mu, \psi), \omega}$ and $S_{(\mu, \psi), \omega}$ the vector of covariances between ω and (μ, ψ) and $\Sigma_{\mu, \psi}$ the variance of (μ, ψ) . We can do accept-reject sampling to sample from the region where $j^*(\omega, \psi, \mu, \sigma, \kappa) = J$. However, the area where $m^*(\lambda, \omega) = m$ has measure zero, so accept-reject sampling will not work. Instead, we have to more carefully characterize spending (λ, ω) to sample from the appropriate area. Let d be the chosen plan's deductible, x the maximum out of pocket sending, and c the copayment rate. A person chooses m to maximize utility:

$$\max_m (m - \lambda) - \frac{1}{2\omega} (m - \lambda)^2 \begin{cases} m & m < d \\ d + c(m - d) & m \geq d \text{ \& } d + c(m - d) < x \\ x & d + c(m - d) \geq x \end{cases} \tag{36}$$

There are four possible solutions for m : 0, λ , $\lambda + (1-c)\omega$, and $\lambda + \omega$. We check whether each of these satisfy the constraints in (36) and compare the utilities of the ones that do.

We sample from the distribution of the latent variables subject to $m^*(\lambda, \omega) = m$ using a Metropolis-Hastings sampler. The density of ω_i given m_{it} is

$$f(\omega_i | \{m_{it}\}, m_i^o, s^o) \propto \prod_{t=1}^T \left(\begin{aligned} &\mathbf{1}\{m = 0\} P(m = 0 | \omega) \\ &+ \mathbf{1}\{0 < m < d\} P(m = m_{it} | \omega) \\ &+ \mathbf{1}\{d < m < x\} \frac{1}{m_{it} - (1-c)\omega} e^{-\frac{1}{2}\left(\frac{\log(m_{it} - (1-c)\omega_i) - \mu_{\lambda, it}}{\sigma_i}\right)^2} \\ &+ \mathbf{1}\{m > x\} + \mathbf{1}\{x < m\} \frac{1}{m_{it} - \omega} e^{-\frac{1}{2}\left(\frac{\log(m_{it} - \omega_i) - \mu_{\lambda, it}}{\sigma_i}\right)^2} \end{aligned} \right) \tag{37}$$

We sample from this density by:

⁴⁴We tried to sample from this density using rejection sampling. We drew $\rho^* \sim TN(\hat{\rho}, v_\rho, -1, 1)$ and accepted with probability $(1-\rho^2)^{N/2}$, unfortunately this leads to unacceptably low acceptance rates.

1. Sample

$$\omega \sim \tilde{f}(\omega|\dots) \propto e^{-\frac{-1}{2}\left(\frac{\log \omega_i - m_i^o}{s^o}\right)^2} \prod_{t=1}^T \left[\mathbf{1}\{d < m < x\} \frac{1}{m_{it} - (1-c)\omega} e^{-\frac{-1}{2}\left(\frac{\log(m_{it} - (1-c)\omega_i) - \mu_{\lambda, it}}{\sigma_{\lambda, i}}\right)^2} + \mathbf{1}\{x < m\} \frac{1}{m_{it} - \omega} e^{-\frac{-1}{2}\left(\frac{\log(m_{it} - \omega_i) - \mu_{\lambda, it}}{\sigma_{\lambda, i}}\right)^2} \right] \quad (38)$$

We sample from this density using the Metropolis-Hastings algorithm with a normal candidate density for $\log \omega$. For each draw of ω_i , we run five metropolis iterations.

2. If $m_{it} = 0$ for any t , draw $\log \lambda_{it} \sim N(\mu_{\lambda, it}, \sigma_{\lambda, i})$.
3. If $0 < m_{it} < d$, set $\lambda_{it} = m_{it}$
4. Accept ω_i if the observed m_{it} is the solution to (36) and $j_{it} = j^*(\omega_i, \psi_i, \mu_{\lambda, it}, \sigma_{\lambda, i}, \kappa_i)$ for all t , else repeat.

- For $t = 2003, 2004$, draw $\mu_{it} | \Theta_{-\mu}, Y$. The posterior is a normal distribution truncated to the region where the choices implied by the model match the choices in the data. We repeatedly draw from this normal distribution until the choices match. The joint distribution of $\log \psi_i, \log \omega_i, \{\mu_{is}\}, \log(\lambda_{it} - \kappa_i)$ is normal with mean $(x_i^\psi \beta_\psi, x_i^\omega \beta_\omega, \{x_{is}^\lambda \beta_\lambda\}, x_{it}^\lambda \beta_\lambda)$ and variance

$$V_i = \begin{pmatrix} & & & & \begin{pmatrix} \sigma_{\omega, \mu} \\ \sigma_{\psi, \mu} \\ \sigma_\mu^2 \\ \sigma_\mu^2 \end{pmatrix} \\ & \Sigma & & & \\ & & & & \\ \begin{pmatrix} \sigma_{\omega, \mu} & \sigma_{\psi, \mu} & \sigma_\mu^2 & \sigma_\mu^2 \end{pmatrix} & & & & \sigma_i^2 + \sigma_\mu^2 \end{pmatrix} \quad (39)$$

Note that we do not need to condition on $\log \lambda_{is}$ for $s \neq t$, because conditional on μ_{is}, μ_{it} and $\log \lambda_{is}$ are independent. Let $C_{\mu_t, (\omega, \psi, \mu_s, \lambda)}$ be the vector of covariances between μ_{it} and the other latent variables, $V_{-\mu_t; i}$ be V_i with the row and column for μ_{it} deleted, and

$$e_i = \begin{pmatrix} \log \omega_i \\ \log \psi_i \\ \mu_{is} \\ \lambda_{it} \end{pmatrix} - \begin{pmatrix} x_i^\omega \beta_\omega \\ x_i^\psi \beta_\psi \\ x_{is}^\lambda \beta_\lambda \\ x_{it}^\lambda \beta_\lambda \end{pmatrix} \quad (40)$$

The posterior mean of μ_{it} is then $e_i \delta_i$ with $\delta_i = C_{\mu_t, (\omega, \psi, \mu_s, \lambda)} V_{-\mu_t; i}^{-1}$ and the variance is $\sigma_\mu^2 - C_{\mu_t, (\omega, \psi, \mu_s, \lambda)} V_{-\mu_t; i}^{-1} C'_{\mu_t, (\omega, \psi, \mu_s, \lambda)}$.

- Draw $\psi_i | \Theta_{-\psi}, Y$. As with μ_{it} , the posterior will be a normal distribution truncated to the region where the choices implied by the model match the choices in the data. We repeatedly draw from this normal distribution until the choices match. Define e_i as when sampling μ_{it} , but leave out λ_{it} . Also, let $C'_{\psi, (\omega, \mu)}$ be the vector of covariances of ψ and (ω, μ) and $\Sigma_{-\psi}$ be Σ with the row and column for ψ removed. Then, the posterior distribution of ψ is

$$N\left(e_i \Sigma_{-\psi}^{-1} C_{\psi, (\omega, \mu)}, \sigma_\psi^2 - C_{\psi, (\omega, \mu)} \Sigma_{-\psi}^{-1} C'_{\psi, (\omega, \mu)}\right) \quad (41)$$

- Draw $\sigma_i | \Theta_{-\sigma_i}, Y$.

$$f(\sigma_i | \log \lambda_{it}, \mu_i, \theta, k) \propto \mathbf{1}\{\sigma^2 < \bar{\sigma}^2\} \frac{1}{\sigma^{2(k-1)}} e^{-\sigma^{-2}/\theta} \prod_t \frac{1}{\sigma} e^{-\frac{1}{2}\left(\frac{\log \lambda_{it} - \mu}{\sigma_i}\right)^2} \mathbf{1}\{\sigma^2 < \bar{\sigma}^2\} \frac{1}{\sigma^{2(k-1+T/2)}} e^{-\sigma^{-2}(1/\theta + \frac{1}{2} \sum_t (\log \lambda_{it} - \mu_i)^2)} \quad (42)$$

So the posterior of σ_i^{-2} is $\Gamma(k + T/2, \frac{2\theta}{2 + \theta \sum_t (\log \lambda_{it} - \mu_i)^2}) \mathbf{1}\{\sigma_i^2 < \bar{\sigma}^2\}$, a truncated Gamma distribution.

- Draw $\gamma_1 | \Theta_{-\gamma_1}, Y, \dots$

$$\begin{aligned}
f(\gamma_1 | \sigma_i, k, Y, \dots) &\propto \prod f(\sigma_i | \gamma_1, k) p(\gamma_1) \\
&\propto \prod \sigma_i^{-2(k-1)} \frac{e^{-\sigma_i^{-2}/\gamma_1}}{\gamma_1^k \Gamma(k)} \frac{1}{1 - F_\Gamma(\bar{\sigma}^{-2}; k, \gamma_1)} p(\gamma_1) \\
&\propto (1 - F_\Gamma(\bar{\sigma}^{-2}; k, \gamma_1))^{-N} (1/\gamma_1)^{Nk} e^{-(1/\gamma_1) \sum \sigma_i^{-2}} p(\gamma_1) \\
&\propto (1/\gamma_1)^{Nk+k_0-1} e^{-(1/\gamma_1) \frac{\gamma_{1,0} \sum \sigma_i^{-2} + 1}{\gamma_{1,0}}} \times (1 - F_\Gamma(\bar{\sigma}^{-2}; k, \gamma_1))^{-N}
\end{aligned} \tag{43}$$

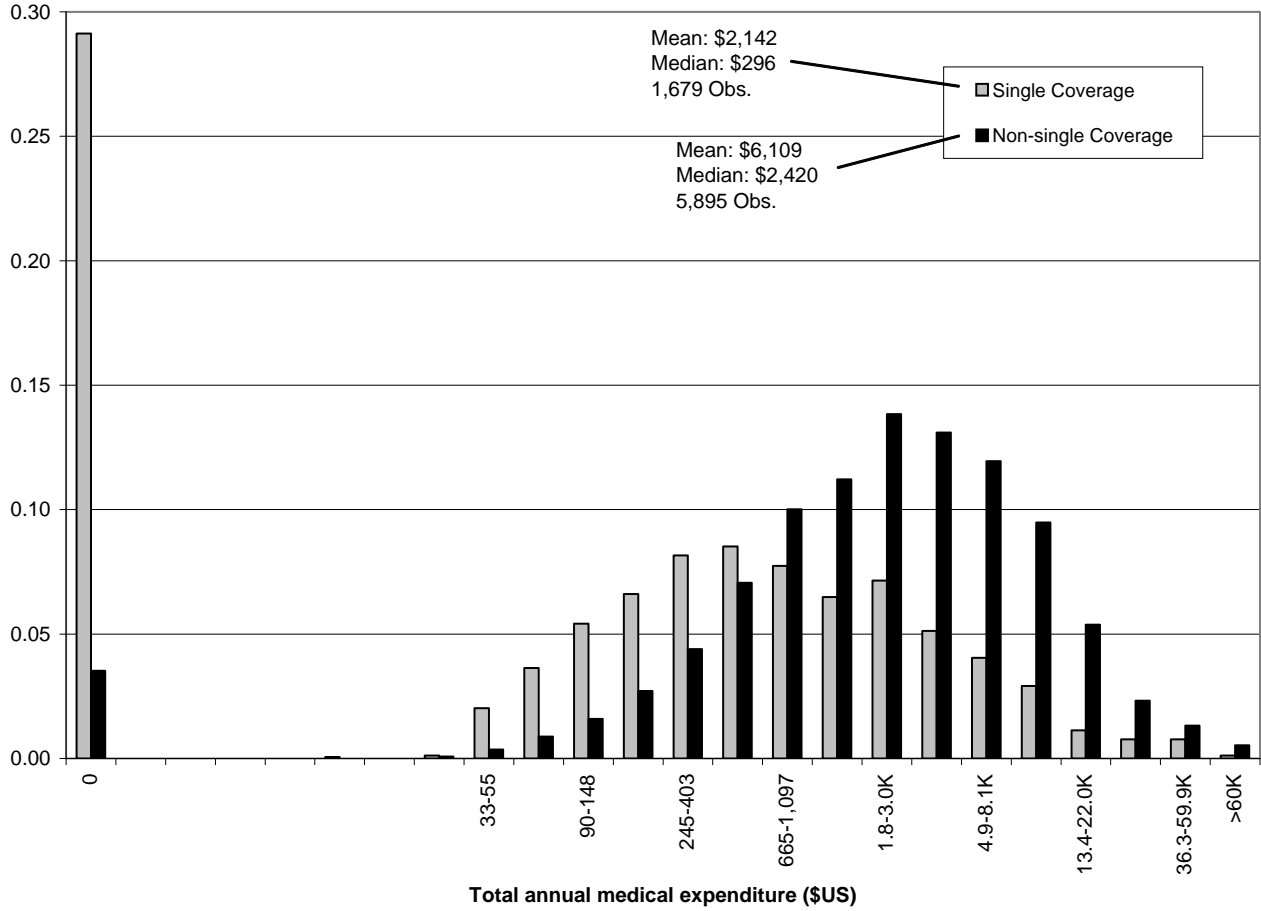
where the prior for $1/\gamma_1$ is $\Gamma(k_0, \gamma_{1,0})$. This is a gamma distribution times some weighting function. Therefore, we use a metropolis sampler with candidate density for $1/\gamma_1$ a $\Gamma(Nk + k_0, \frac{\gamma_{1,0}}{\gamma_{1,0} \sum \sigma_i^{-2} + 1})$. Given the current estimates, $1 - F_\Gamma(\bar{\sigma}^{-2}; k, \gamma_1)$ is very close to one, so this metropolis sampler accepts nearly all draws.

- Draw $\gamma_2 | \Theta_{-\gamma_2}, Y$.

$$\begin{aligned}
f(k | \sigma_i, \theta, \dots) &\propto \prod \sigma_i^{-2(k-1)} \frac{e^{-\sigma_i^{-2}/\theta}}{\theta^k \Gamma(k)} p(k) (1 - F_\Gamma(\bar{\sigma}^{-2}; k, \theta))^{-N} \\
&\propto \frac{e^{k \sum \log \sigma_i^{-2} + \log \theta^{-N}}}{\Gamma(k)^N} p(k) (1 - F_\Gamma(\bar{\sigma}^{-2}; k, \theta))^{-N}
\end{aligned} \tag{44}$$

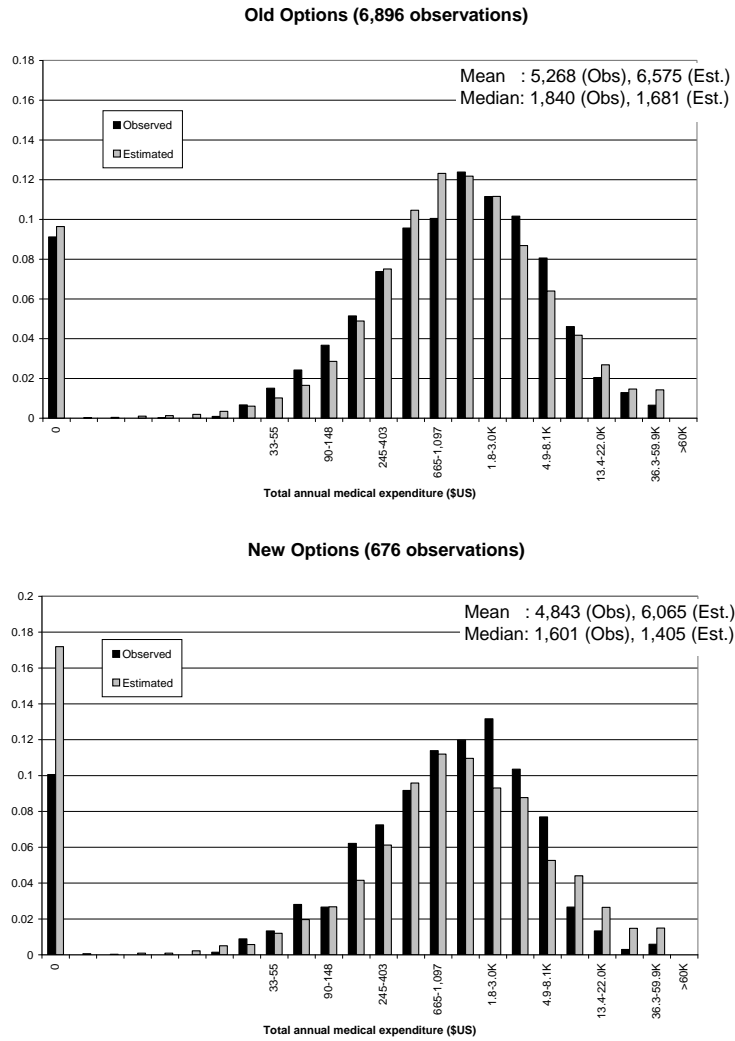
which is a nonstandard distribution. We use the adaptive rejection metropolis sampling (ARMS) method of Gilks, Best, and Tan (1995) to sample from it. This is a hybrid accept-reject and metropolis sampling scheme. It is designed to sample from log-concave and nearly log-concave densities efficiently. Without the $(1 - F_\Gamma(\bar{\sigma}^{-2}; k, \theta))^{-N}$ term, this density would be log-concave (it may be log-concave anyway), and ARMS can sample from it very efficiently.

Figure 1: The cross-sectional distribution of medical expenditure



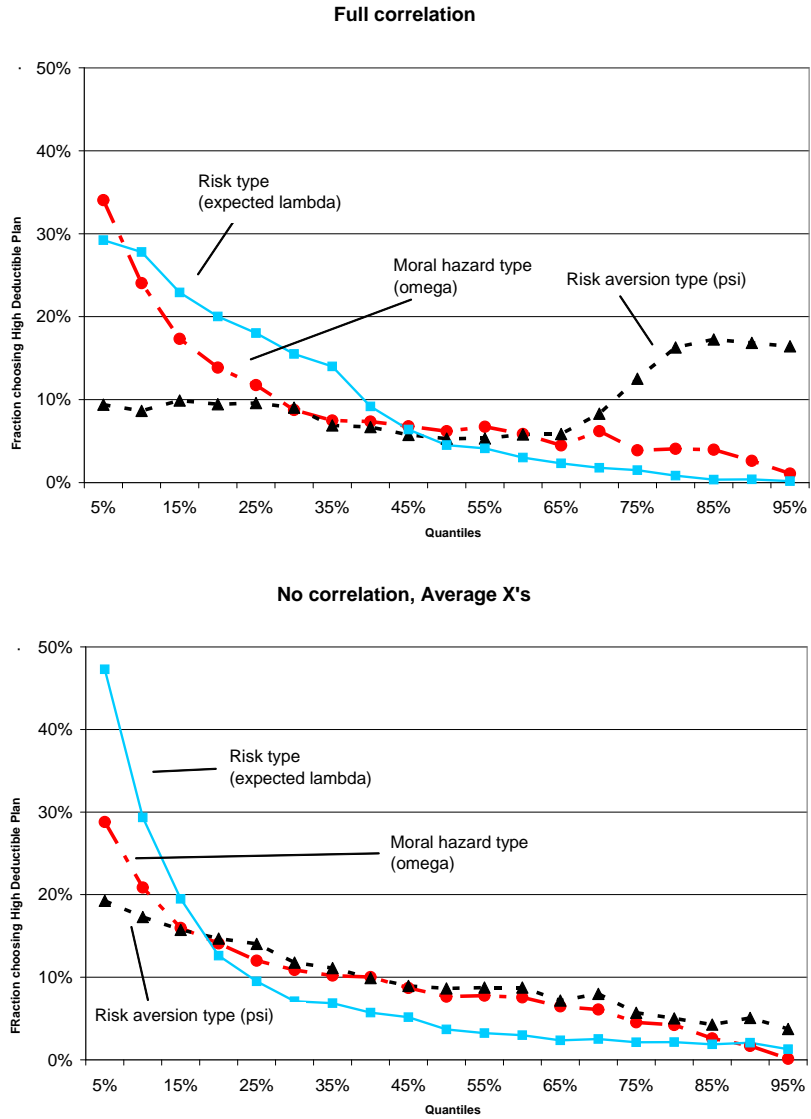
The figure presents the distribution of total annual medical expenditure for each employee (and any covered dependents) in our baseline sample. The graph uses a log scale, such that the second bin covers expenditure lower than $\exp(0.5)$, the next covers expenditures between $\exp(0.5)$ and $\exp(1)$, and so on; the x-axis labels show the corresponding dollar amounts of selected bins. An observation is an employee-year, pooling data from 2003 and 2004. The grey bars correspond to employees with a single coverage, while the black bars correspond to employees who also covered additional dependents (spouse, children, or both).

Figure 2: Model fit – medical spending distributions



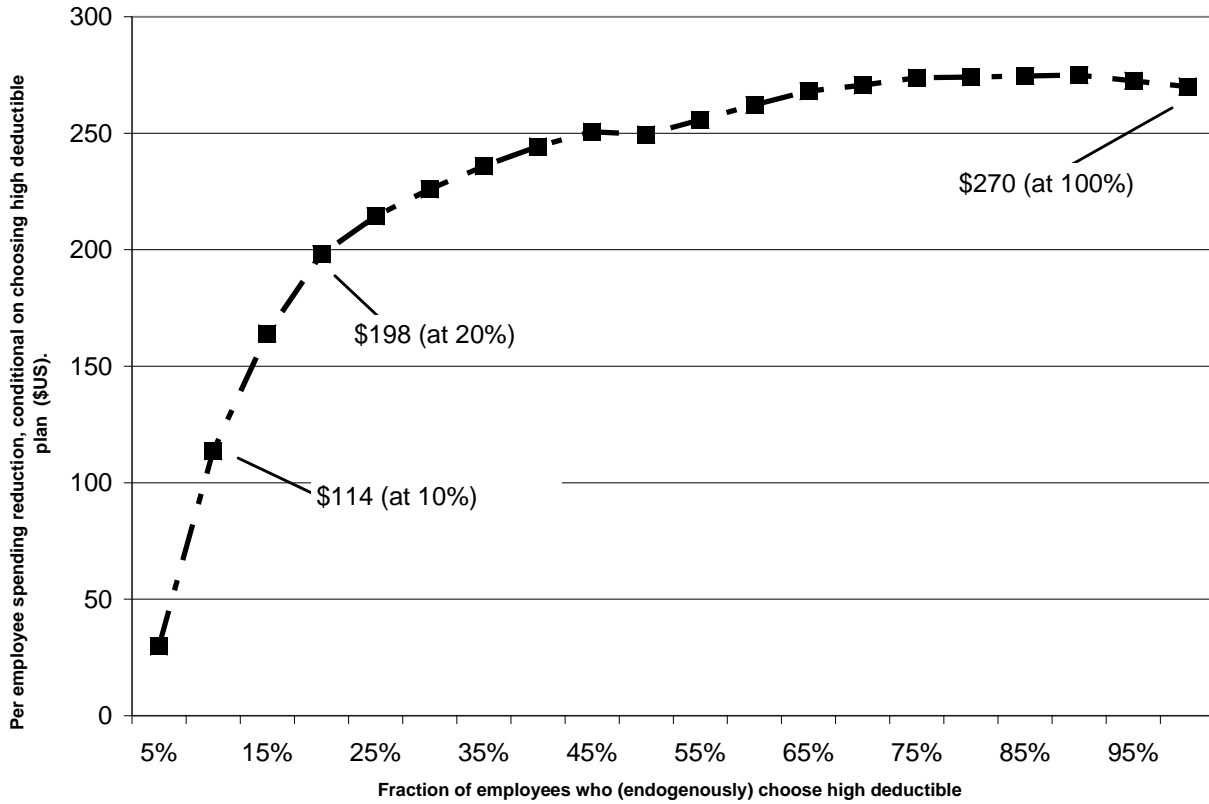
The figure presents the distribution of total annual medical expenditure, in the data and in model simulations based on the estimated parameters. The graph uses a log scale, such that the second bin covers expenditure lower than $\exp(0.5)$, the next covers expenditures between $\exp(0.5)$ and $\exp(1)$, and so on; the x-axis labels show the corresponding dollar amounts of selected bins. The top panel compares spending of individuals who faced the original options, and the bottom panel compares the spending distribution of individuals who faced the new options.

Figure 3: Selection on moral hazard relative to other sources of selection



The figure illustrates the relative importance of the three different sources of selection that we model. We consider an individual's choice between two available options: the no deductible and high deductible plans among the new set of options (see Table 2, options 5 and 1 respectively). We assume the observed (averaged within each coverage tier) premiums for these two options. Each point in the figure indicates the fraction of individuals choosing the high deductible (i.e. low coverage) option relative to the no deductible (high coverage) option. We consider three sources of selection: $E(\lambda)$ (risk), ω (moral hazard), and ψ (risk aversion). For each of them, we compute the fraction choosing the high deductible at different quantiles of the distribution. In the top panel, we take into account the estimated correlation between each component and the others, as well as the effect of various demographics (X 's), while in the bottom panel we repeat the same exercise but fix the X 's at their means and assume that the other components of the model are drawn independently (that is, assuming no correlation).

Figure 4: Spending implications of selection on moral hazard



The figure illustrates the potential spending implications arising from selection on moral hazard. To construct the table, we use an exercise similar to the one used for Figure 3. For each individual, we use the model estimates to compute his decline in expected annual expenditure as we move him from the highest coverage (no deductible) to the lowest coverage (high deductible) in the new benefits options (see Table 2, options 5 and 1 respectively). We then vary the relative price of the highest coverage, allowing employees to endogenously choose between the two options, and report the per-employee expected decrease in spending for the group of individuals who chooses the lowest coverage at each price. Without selection on moral hazard, the curve would have been flat. Selection on moral hazard implies that those with the lowest moral hazard effects of insurance are those who have the lowest willingness to pay for incremental coverage and are therefore the first (as the price of coverage increases) to switch from higher to lower coverage. Ceteris paribus, therefore, selection on moral hazard generates an upward sloping curve; this can be offset through the correlation between moral hazard and other components of demand (such as risk aversion or health risk).

Table 1: Summary statistics for 2003 sample

	Obs.	Average Age	Average Annual Income	Average Tenure with Alcoa	Fraction Male	Fraction White	Fraction Single Coverage	Avg number of insured family members (if non-single coverage)	Total annual medical spending (US\$)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Baseline sample	3,996	41.3	31,292	10.2	0.84	0.72	0.23	2.8	5,282
Switched in 2004	683	44.5	39,715	15.5	0.96	0.85	0.21	2.7	5,194
Switched in 2005	974	39.7	25,532	8.2	0.73	0.44	0.25	2.8	5,364
Switched in 2006	1,075	38.3	29,952	5.7	0.86	0.82	0.23	2.9	5,927
Switched after 2006	1,264	43.3	32,316	12.7	0.85	0.79	0.22	2.6	4,717

Top row presents statistics based on the 2003 data for our baseline sample, which covers all hourly union workers not covered by the Master Steelworker’s Agreement (except those that get dropped in the process of the data cleaning described in the text). The subsequent rows (“Switched in 2004,” “Switched in 2005,” and so on) partition our baseline sample based on the year in which employees were switched to the new set of health insurance options. Total annual medical spending in column (9) is for employees and any covered dependents.

Table 2: Old and new health plans

Panel A: Single coverage (N=1,679)

	Original Plan Options			New Plan Options				
	Option 1	Option 2	Option 3	Option 1 ^a	Option 2	Option 3	Option 4	Option 5
Plan features:								
Deductible	1,000	0	0	1,500	750	500	250	0
Out of Pocket Maximum	5,000	2,500	1,000	4,500	3,750	3,500	2,750	2,500
Average Share of Spending Paid Out of Pocket ^b	0.580	0.150	0.111	0.819	0.724	0.660	0.535	0.112
Employee Premium ^c	0	351	1,222	0	132	224	336	496
Fraction choosing each option^d	3.3%	63.5%	33.2%	14.1%	0.0%	2.2%	37.8%	45.9%

Panel B: Non-single coverage (N=5,895)

	Original Plan Options			New Plan Options				
	Option 1	Option 2	Option 3	Option 1 ^a	Option 2	Option 3	Option 4	Option 5
Plan features:								
Deductible	2,000	0	0	3,000	1,500	1,000	500	0
Out of Pocket Maximum	10,000	5,000	2,000	9,000	7,500	7,000	5,500	5,000
Average Share of Spending Paid Out of Pocket ^b	0.495	0.130	0.098	0.732	0.600	0.520	0.387	0.111
Employee Premium ^c	0	354	1,297	0	364	620	914	1,306
Fraction choosing each option^d	0.6%	56.1%	43.3%	3.9%	0.6%	1.8%	24.4%	69.3%

The table summarizes the key features of the original and new health insurance coverage options. The features shown apply to in-network spending. Not shown are coinsurance rates (applied to those who reached the deductible but have yet to reach the out-of-pocket maximum) which are 10% in all plans (old and new). There are some other small differences between the original and new options that are associated with out-of-network spending, preventive care, and certain treatments associated with co-pays rather than coinsurance in the original set of options. See text for further details.

^a The New Option 1 includes a health reimbursement arrangement (HRA). Every year the employer sets aside \$750 (for single; \$1,250 for non-single coverage) that the employee can use (tax free) to pay for a variety of expenses such as deductibles and coinsurance payments. Unused HRA funds roll over to future years and, eventually, can be used during retirement to finance health insurance, provided through the company or through COBRA. Our baseline model abstracts from the HRA component of New Option 1.

^b To compute the average share of spending out of pocket, we use the 2003 claims from all individuals in the baseline sample and apply each option's coverage details to this (common) sample. We then compute, for each option, the ratio of the resultant out-of-pocket expenses to the total claim mounts, and report the average (across employees in 2003) for each option. As a result, our computed average share of spending out of pocket abstracts from any differential behavioral effect of each contract.

^c Premiums are normalized so that the lowest coverage is free for all employees. This is true in both the original and new options, up to small variation of several hundred dollars across employees. We report the average premium for employees in the baseline sample, pooling 2003 and 2004. Premiums vary by coverage tier; there is also some additional variation (across employees within coverage tier) in the incremental premiums associated with greater coverage options. The variation is based on the business unit to which each employees belongs (see Einav, Finkelstein, and Cullen, 2010).

^d Statistics are based on all employee in the baseline sample, pooling 2003 and 2004.

Table 3: Spending patterns by coverage level

	Single Coverage			Non-Single Coverage		
	Count	Mean	Median	Count	Mean	Median
<i>Original Plan Options</i>						
Highest coverage	512	3,130	557	2,318	6,634	2,670
All other coverages	1,032	1,793	233	3,036	5,766	2,287
<i>New Plan Options</i>						
Highest coverage	62	1,650	447	375	6,858	2,630
All other coverages	73	560	52	166	3,367	1,447

The table shows (contemporaneous) spending by coverage choice. Under the original options, the highest coverage is option 3. Under the new options, the highest coverage is option 5. See Table 2 for coverage details.

Table 4: Basic difference-in-differences in baseline sample

	Obs.	Mean	Fraction with zero spending	10th	25th	50th	75th	90th
Control (Switched after 2004)								
2003 spending	3,313	5,300	0.09	52	426	1,775	5,178	11,984
2004 spending	2,902	5,248	0.09	55	516	1,888	5,589	12,253
Treated (Switched in 2004)								
2003 spending	683	5,194	0.08	79	579	1,956	5,048	12,644
2004 spending	676	4,843	0.10	0	447	1,601	4,615	9,468
Treated-Control Differences (levels)								
2003 spending		-106	-0.01	27	153	181	-130	660
2004 spending		-405	0.01	-55	-69	-287	-974	-2,785
2004-2003 Difference (levels)								
Control (switched after 2004)		-52	0.00	3	90	113	411	269
Treated (Switched in 2004)		-351	0.02	-79	-132	-355	-433	-3,176
Difference in differences (levels)		-299	0.02	-82	-222	-468	-844	-3,445
Difference (percentages)								
Control (switched after 2004)		-1.0%	0.0%	5.8%	21.1%	6.4%	7.9%	2.2%
Treated (Switched in 2004)		-6.8%	25.0%	-100.0%	-22.8%	-18.1%	-8.6%	-25.1%
Diff. in differences		-5.8%	25.0%	-105.8%	-43.9%	-24.5%	-16.5%	-27.4%

Table 5: Difference-in-differences estimates of impact of change in health insurance options on annual medical spending

	2003-2004 sample			2003-2006 sample		
	OLS in levels (1)	OLS in logs (2)	QMLE-Poisson (3)	OLS in levels (4)	OLS in logs (5)	QMLE-Poisson (6)
Estimated Treatment effect	-301.9 (752.6) [0.69]	-0.356 (0.19) [0.07]	-0.061 (0.15) [0.69]	-591.8 (264.2) [0.034]	-0.175 (0.12) [0.17]	-0.114 (0.048) [0.018]
Mean Dependent Variable	5,230	6.91	5,230	5,392	6.9	5,392
N	7,574	7,574	7,574	14,638	14,638	14,638

The table shows the difference-in-difference estimate of the spending reduction associated with moving from the old options to the new options. The unit of observation is an employee-year. Dependent variable is the total annual medical spending for each employee and any covered dependents (or log of 1 + total spending in column (2) and column (5)). The coefficient shown is the coefficient on an indicator variable that is equal to 1 if the employee's treatment group is offered the new health insurance options that year, and 0 otherwise. All regressions include year and treatment group fixed effects. We classify employees into one of four possible treatment groups - switched in 2004, switched in 2005, switched in 2006, or switched later - based on his union affiliation which determines the year in which he is switched to the new health insurance options. Estimation is either by OLS or QMLE Poisson as indicated in the column headings. Standard errors (in parentheses) are adjusted for an arbitrary variance-covariance matrix within each of the 28 unions; p-values are in [square brackets]. Columns (1)-(3) show estimates for the 2003-2004 sample; Columns (3)-(6) expand the sample to include 2003-2006.

Table 6: Suggestive evidence of heterogeneous moral hazard and of selection on moral hazard

		Obs.	Mean spending	Estimated change in spending associated with change in options (levels)		Avg Out-of- Pocket Share (Old Options)	Avg Out-of-Pocket Share (New Options)	Increase in Out-of-Pocket Share
		(1)	(2)	Coeff. (3)	Std. Err. (4)	(5)	(6)	(7)
(A)	Above median age (of 43)	6,972	6,263	-1,302	(799)	12.4	27.8	15.4
	Below or equal to median age (of 43)	7,666	4,600	-85.8	(483)	12.9	29.5	16.6
(B)	Male	12,373	5,442	-604	(293)	12.6	29.1	16.5
	Female	2,265	5,120	-579	(693)	12.9	25.8	12.9
(C)	Above median income (of \$31,000)	7,322	5,669	-364	(602)	12.2	29.1	16.9
	Below median income (of \$31,000)	7,316	5,116	-301	(397)	13	28.1	15.1
(D)	Less coverage in 2003	6,997	5,003	-621	(513)	13.4	32	18.6
	More coverage in 2003	5,229	6,296	-1,336	(596)	10.1	23.5	13.4

The table shows results for different groups of workers (shown in different rows) in the 2003-2006 sample. Column (1) reports the number of employee-years in the sample, and column (2) reports their mean annual medical spending over the sample period. Columns (3) and (4) report, respectively, the coefficient and standard error of the estimated change in spending associated with moving from the old to the new options. This is based on a difference-in-differences regression on the 2003-2006 sample; we report in columns (3) and (4) the coefficient and standard error on an indicator variable that is equal to 1 if the employee’s treatment group is offered the new health insurance options that year, and 0 otherwise. The dependent variable is always total annual medical spending for each employee and any covered dependents. All regressions include year and treatment group fixed effects. Standard errors (in parentheses) are adjusted for an arbitrary variance-covariance matrix within each of the 28 unions. Columns (5) and (6) show the average out of pocket share within each group under the old and new options respectively. These are calculated based on the share of employees within each group in each plan, and the plan specific out of pocket shares shown in Table 2 (which are computed on a common sample of workers across plans). Column (7) reports the increase in the average out of pocket share for each group associated with moving from the old options to the new options. In panel (D), the sample is limited to employees who are employed at the firm in 2003 and who choose either “more coverage” (option 3 from Table 2) or “less coverage” (option 2 from Table 2) in 2003.

Table 7: Parameter estimates

Mean Shifters				
	Mu_Lambda (Health risk)	Kappa_Lambda (Health risk)	Omega (Moral hazard)	Psi (Risk aversion)
Constant	5.88 (0.086)	264 (38)	5.29 (0.14)	-5.50 (0.06)
Coverage tier				
Single	(omitted)	(omitted)	(omitted)	(omitted)
Family	1.60 (0.078)	-261 (37)	0.75 (0.17)	-1.47 (0.05)
Emp+Spouse	1.60 (0.084)	-73 (58)	0.56 (0.32)	-1.46 (0.06)
Emp+Children	0.85 (0.091)	-258 (47)	-0.22 (0.28)	-1.13 (0.06)
Switch group				
Switch 2004	0.13 (0.075)	297 (51)	-0.40 (0.20)	-0.40 (0.05)
Switch 2005	0.08 (0.073)	161 (43)	0.15 (0.17)	-0.40 (0.05)
Switch 2006	0.14 (0.074)	92 (43)	-0.20 (0.19)	-0.10 (0.05)
Switch later	(omitted)	(omitted)	(omitted)	(omitted)
2004 Time dummy	-0.10 (0.020)		--	--
Variance-covariance matrix				
	Mu_Lambda (Health risk)	Omega (Moral hazard)	Psi (Risk aversion)	
Mu_Lambda	0.52 (0.06)	0.23 (0.08)	-0.14 (0.04)	
Omega	--	2.01 (0.15)	-0.16 (0.06)	
Psi	--	--	0.19 (0.02)	
Additional parameters				
Sigma_Mu_Bar			0.78 (0.04)	
Sigma_Kappa			0.44 (0.06)	
Gamma1 (Sigma_Lambda parameter)			0.0092 (0.0013)	
Gamma2 (Sigma_Lambda parameter)			49 (4.1)	
Implied quantities				
	Lambda (Health risk)	Omega (Moral hazard)	Psi (Risk aversion)	
Expected	5,620	822	0.00156	
Std. Dev.	40,200	2,426	0.00151	

The table presents our baseline parameter estimates based on our baseline sample of 7,572 employees. As described in the text, the estimates are based on a Gibbs sampler; the table reports the posterior mean and the posterior standard deviations in parentheses. Bottom panel reports some implied quantities of interest that are derived from the estimated parameters.

Table 8: Model fit – choice probabilities

Original options (N = 6,896)		
<i>Plan</i>	<i>Data</i>	<i>Model</i>
Option 1	1.3%	3.0%
Option 2	57%	57%
Option 3	42%	40%
New options (N = 676)		
<i>Plan</i>	<i>Data</i>	<i>Model</i>
Option 1	5.9%	7.0%
Option 2	0.4%	4.0%
Option 3	1.8%	1.0%
Option 4	27%	13%
Option 5	65%	75%

The table reports the actual and predicted choice probabilities of each plan. Plans are numbered from lowest to highest coverage. For plan details see Table 2.

Table 9: Spending implications of moral hazard estimates

	Mean	Std. Dev.	10th	25th	50th	75th	90th
Spending difference as we move from no to high deductible plan	270	571	0	0	37	273	787
Spending difference as we move from full to no insurance	790	2,427	0	64	237	703	1,777

The table reports the implied spending implications if we move different employees across plans. For each employee, we use the model estimates to compute his decline in expected annual expenditure as we change his insurance plan. In the top row, we move each employee from the highest coverage option under the new benefit options (option 5) to the lowest coverage option under the new benefit options (option 1); roughly speaking, this entails moving from a plan with no deductible to a plan with a high deductible; see Table 2 for more details. In the bottom row, we move each employee from full to no insurance. The table then summarizes the cross-sectional distribution of the spending effects. The estimates are primarily driven by the estimated distribution of ω , but they take into account the truncation of spending at zero by integrating over the conditional (on ω) distribution of λ .

Table 10: Spending and welfare effects of asymmetric information

	Average equilibrium (incremental) premium	No deductible plan share	Expected spending per employee	Total welfare per employee
(1) "Status quo": no screening or monitoring	2,737	0.41	5,725	normalized to 0
(2) "Perfect screening": premiums depend on $F(\lambda)$ and ω	1,465	0.93	5,846	458
(3) "Imperfect screening": premiums depend on ω (but not on $F(\lambda)$)	2,649	0.43	5,706	45
(4) "Perfect monitoring": contracts reimburse only " λ -related" spending	2,566	0.46	5,183	211
(5) "Imperfect monitoring": perfect monitoring assumed for choice (but not for utilization)	2,566	0.46	5,737	57

The table reports the spending and welfare effects from a set of counterfactual contracts described in the text. All exercises are applied to a setting in which the only two options available are the no deductible plan and the high deductible plan under the new benefit options (i.e. option 5 and option 1, respectively; see Table 2). Equilibrium premiums are computed as the incremental (relative) premium for the no deductible plan that equals the expected incremental costs associated with providing the no deductible plan to those who choose it. The no deductible plan share is calculated based on the choice probabilities as a function of equilibrium premiums. Expected spending and total welfare are computed based on these choices. Row 1 assumes the “status quo” asymmetric information contracts, which a “uniform” price that varies only by coverage tier. Row 2 assumes “perfect screening”, so that contracts are priced based on ω_i and all components of $F_i(\lambda)$ and adverse selection is eliminated. Row 3 assumes “imperfect screening”, in which contracts are priced based only on ω_i . Row 4 assumes “perfect monitoring” so that moral hazard is eliminated. Specifically we assume the insurance provider can counterfactually observe (and not reimburse) spending that is associated with moral hazard; spending associated with health – realization of λ – are reimbursed according to the observed contracts. Row 5 assumes “imperfect monitoring” in which, ex ante individuals choose contracts under the assumption that there will be perfect monitoring (i.e. spending associated with moral hazard will not be reimbursed), but ex-post (after they choose their contract but before they make their spending decision) the contracts are changed to be the standard contracts that reimburse all medical spending regardless of its origin.

Appendix Table A1: Impact of change in health insurance options on components of health spending and utilization

	Spending					Utilization		
	Total Spending	Spending on Doctor Visits	Spending on Outpatient Visits	Spending on Inpatient Visits	Remaining Spending	Number of Doctor Visits	Number of Outpatient Visits	Any Inpatient Visits
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Estimated treatment effect	-591.81 (264.26) [0.034]	-220.37 (69.32) [0.004]	-310.32 (137.89) [0.033]	-116.69 (246.17) [0.639]	55.91 (69.34) [0.427]	-1.94 (0.37) [0.000]	-0.0005 (0.27) [0.999]	-0.017 (0.011) [0.155]
Mean Dep. Var.	5392	1475	1922	1804	191	12.2	3	0.14

The table shows the difference-in-difference estimate of the impact of the move from the old to the new options on various components of health care spending and utilization. All columns show the coefficient on *TREAT* from estimating equation 17 by OLS for the dependent variable given in the column heading. Unit of observation is an employee-year. All regressions include year and treatment group fixed effects. We classify employees into one of four possible treatment groups - switched in 2004, switched in 2005, switched in 2006, or switched later - based on his union affiliation which determines the year in which he is switched to the new health insurance options. Standard errors (in parentheses) are adjusted for an arbitrary variance-covariance matrix within each of the 28 unions; p-values are in [square brackets]. Sample is 2003-2006. N = 14,638.

Appendix Table A2: Impact of change in health insurance options on spending (quarterly data)

	Total Spending		Total Spending, Censored at 99th percentile			
	Baseline	Pre-specification test	Baseline	Pre-specification test	Col (4) w treatment-group-specific linear trend	More dynamics
	(1)	(2)	(3)	(4)	(5)	(6)
$TREAT_{jt}$	-147.87 (66.04) [0.034]	-139.44 (85.22) [0.113]	-156.85 (43.60) [0.001]	-157.54 (50.49) [0.004]	-185.65 (74.82) [0.020]	
$TREAT_{jt,0}$		40.78 (158.49) [0.799]		-3.31 (69.21) [0.962]	-5.69 (76.00) [0.941]	
$TREAT_{jt,-3}$						58.59 (60.91)
$TREAT_{jt,-2}$						-2.46 (90.69)
$TREAT_{jt,-1}$						-42.03 (69.75)
$TREAT_{jt,0}$						0 (reference period)
$TREAT_{jt,1}$						-121.79 (53.47)
$TREAT_{jt,2}$						-187.06 (77.21)
$TREAT_{jt,3}$						-118.35 (65.90)
$TREAT_{jt,4}$						-197.82 (61.78)
Mean dep. Var.	1348		1125			

The table shows the difference-in-difference estimate of the impact of the move from the old to the new options. Specifically, columns 1 through 5 show the results from estimating equation 18 (and column 6 shows results from estimating equation 19) by OLS for the dependent variable total quarterly health spending. Unit of observation is an employee-quarter. The variable $TREAT_{jt}$ is an indicator variable for whether treatment group j is offered the new health insurance options in quarter t . The variable $Treat_{jt,0}$ is an indicator variable for whether it is the quarter before group j is switched to the new health insurance options. The variable $TREAT_{jt,k}$ is an indicator variable for whether it is k quarters since quarter 0 (i.e. the quarter before the switch). All regressions include quarter and treatment group fixed effects; column 5 also includes a treatment group-specific linear trend. We classify employees

into one of four possible treatment groups - switched in 2004, switched in 2005, switched in 2006, or switched later - based on his union affiliation which determines the year in which he is switched to the new health insurance options. Standard errors (in parentheses) are adjusted for an arbitrary variance-covariance matrix within each of the 28 unions; p-values are in [square brackets]. Sample is 2003-2006. N = 58,552.

Appendix Table A3: Sensitivity of annual difference-in-differences estimates to controlling for observables

	Baseline (no covariates)	Adding control for coverage tier	Adding additional demographic controls	At Alcoa all four years	At Alcoa all four years, w individual fixed effects.
	(3)	(2)	(3)	(4)	(5)
$TREAT_{jt}$	-591.81 (264.26) [0.034]	-522.74 (267.29) [0.061]	-537.96 (264.33) [0.052]	-965.92 (302.33) [0.004]	-965.92 (349.04) [0.012]
Mean Dep. Var.		5392		5438	
N		14,638		7,580	

The table examines the sensitivity of the annual difference-in-differences estimates of the impact of the move from the old to the new options on total annual medical spending. All columns show the coefficient on $TREAT$ from estimating equation 17 by OLS for the dependent variable total annual medical spending. Unit of observation is an employee-year. All regressions include quarter and treatment group fixed effects. We classify employees into one of four possible treatment groups - switched in 2004, switched in 2005, switched in 2006, or switched later - based on his union affiliation which determines the year in which he is switched to the new health insurance options. Standard errors (in parentheses) are adjusted for an arbitrary variance-covariance matrix within each of the 28 unions; p-values are in [square brackets]. Sample is 2003-2006. Column 1 replicates the baseline results (from Table 5, column 4). In column 2 we control for coverage tier. In column 3 we control for coverage tier, employee age, employee gender, number of dependents insured on the policy, whether the employee is white, the number of years the employee has been at Alcoa, and the employee's annual salary. Column 4 limits the sample to employees who are at Alcoa (and in our data) for all four years. Column 5 adds employee fixed effects to the sample in column 4.

Appendix Table A4: Additional sensitivity analysis

	Dependent variable: total spending		
	Dependent variable: choose a non-PPO option (1)	Baseline quarterly specification (OLS) (2)	GLS estimation at Treatment group - quarterly level (3)
$TREAT_{jt}$	-0.021 (0.024) [0.376]	-147.87 (66.04) [0.034]	-166.43 (61.22) [0.007]
Mean dep var	0.106	1348	1364
N	16366	58,552	64

The table examines some additional sensitivity of the annual difference-in-differences estimates of the impact of the move from the old to the new options on total annual medical spending. All regressions include year and treatment group fixed effects. Column 1 shows the coefficient on $TREAT$ from estimating equation 17 by OLS on the baseline 2003-2006 sample, plus the employees who choose a non-PPO option; the dependent variable is an indicator variable for whether the employee chose a non PPO option; unit of observation is an employee-year. In columns 2 and 3 the dependent variable is total spending. Column 2 shows the coefficient on $TREAT$ from estimating equation 18 by OLS at the employee-quarter level. Column 3 shows the coefficient on $TREAT$ from estimating equation 18 by GLS with a panel-specific auto correlation parameter and variance at the treatment group - quarter level. In columns 1 and 2 standard errors (in parentheses) are adjusted for an arbitrary variance-covariance matrix within each of the 28 unions; p values are in [square brackets].