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The Journal of Political Economy, Vol. 108, No. 1. (Feb., 2000), pp. 56-78.

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Testing for Asymmetric Information in Insurance Markets

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The first goal of this paper is to provide a simple and general test of the presence of asymmetric information in contractual relationships within a competitive context. We also argue that insurance data are particularly well suited to such empirical investigations. To illustrate this claim, we use data on contracts and accidents to investigate the extent of asymmetric information in the French market for automobile insurance. Using various parametric and nonparametric methods, we find no evidence for the presence of asymmetric information in this market.

I. Introduction

In the last 20 years, contract theory has developed at a rapid pace. But, until recently at least, empirical applications have lagged behind.¹ One striking illustration, for instance, is that although we can

Paper presented at seminars in Chicago, Toulouse, Princeton, and Paris. We thank the participants and Christian Gouriéroux, Igal Hendel, Bo Honore, Steven Levitt, Costas Meghir, Ariel Pakes, Quang Vuong, and especially Jim Heckman, Georges Dionne, and an anonymous referee for useful comments and suggestions. Financial support from the French federation of insurance companies (FFSA) is gratefully acknowledged. Errors are strictly ours.

¹This is not to deny that important steps have recently been made in some directions. For instance, the econometrics of auctions has evolved in a very promising way (see, e.g., Laffont, Ossard, and Vuong 1995; Florens, Hugo, and Richard 1997). Also, several papers have tried to estimate econometric models based on core con-

[*Journal of Political Economy*, 2000, vol. 108, no. 1]

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precisely describe the kind of problems that may arise from the presence of adverse selection within a competitive setting and even formulate detailed policy recommendations for alleviating these problems, we can hardly provide a list of markets in which adverse selection has empirically been shown to matter.

This lag between theory and empirical applications probably has several explanations. One is the scarcity of adequate data sets that offer a large sample of standardized contracts for which performances are recorded.² However, insurance offers a promising field for empirical work on contracts. In such fields as automobile, housing, or even life and health insurance, contracts are highly standardized and can be exhaustively described by a small set of variables. A large company typically covers hundreds of thousands (or even millions) of clients. In many cases, the company's files contain all the relevant information. Finally, the empirical counterpart of the client's "performance" is the occurrence of an accident and possibly its cost; again, this is precisely recorded in the company's files. Thus most predictions of contract theory can be tested here in a detailed way, using standard econometric tools.³

The first purpose of this paper is to prove this point by providing a simple and general test of the presence of asymmetric information in the contractual relationship. Our basic claim, following Chiappori (1994), is that the theoretical notion of asymmetric information (whether adverse selection or *ex ante* moral hazard) implies, in statistical terms, a positive correlation between two (conditional) distributions. This test, simple as it may be, is surprisingly robust. It does not rely on specific functional forms; neither does it require particular assumptions on preferences, technology, or the nature of the equilibrium.

The second goal of the paper is to provide an empirical application of the technique. We apply our test to a French survey of automobile insurance contracts and find that, when observables are adequately taken into account, there remains no evidence of asymmetric information in the sample we consider. This conclusion is in sharp contrast with those of other studies, for instance, Puelz and

tract theory. A nonexhaustive list includes Joskow (1990), Wolak (1994), Wunsch (1994), Slade (1996), and Dalen and Gomez-Lobo (1997). Salanié (1997, chap. 8) discusses some of this work. Finally, several recent papers provide direct tests of adverse selection, including works by Ausubel (1998), Cardon and Hendel (1998), and Toivanen and Cressy (1998).

² However, the work of Murphy (1986) and Chiappori, Salanié, and Valentin (1999) shows that it is sometimes possible to circumvent the lack of recorded performances.

³ Chiappori and Salanié (1997) argue for the use of insurance data in more detail.

Snow (1994); we discuss in detail the reasons that may explain this difference.

A. *What Does Theory Predict?*

In this paper, we focus on exclusive contracts. That is, we consider only markets in which the insurer can impose an exclusive relationship. This allows firms to implement nonlinear and, in particular, convex price schedules, which are typically needed under asymmetric information.⁴ In this context, the literature on adverse selection in a competitive setting builds on the seminal contribution of Rothschild and Stiglitz (1976). The numerous extensions that can be found in the literature suggest that some of their results may be overturned in slightly different settings. On the other hand, we believe that three conclusions are fairly robust: (1) Under adverse selection, observationally equivalent agents are likely to be faced with menus of contracts, among which they are free to choose. (2) Within the menu, contracts with more comprehensive coverage are sold at a higher (unitary) premium. (3) Within the menu, contracts with more comprehensive coverage are chosen by agents with higher accident probabilities.

The first prediction holds for various types of adverse selection; that is, agents may differ in their risk, as in Rothschild and Stiglitz's paper, but also in their wealth, preferences, risk aversion, and so forth.⁵ Also, it is essentially qualitative and is unlikely to generate very powerful tests. Testing prediction 2 would require an explicit model of the firm pricing policy. This is a difficult task that in any case would necessitate strong assumptions on the technology.

On the other hand, prediction 3 suggests a very simple test: a positive correlation between coverage and frequency of accidents should be observed on observationally identical agents. In fact, this prediction turns out to be surprisingly general and to extend to a variety of more general contexts.

1. It does not depend on the firm's pricing policy; as such, it does not rely on specific assumptions on technology (such as linearity, absence of fixed costs, absence of loading, absence of cross-subsidies, etc.). It applies even when the firm's pricing policy is suboptimal.

2. Similarly, its validity does not depend on specific assumptions

⁴ Chiappori (in press) surveys the literature on insurance under adverse selection and emphasizes the empirical implications of competition under exclusive and non-exclusive contracts.

⁵ When differences are not related to actuarial risk, fair pricing leads to identical contracts for all individuals. However, if either imperfect competition or transaction costs are introduced, then competition results in differentiated contracts.

on preferences. In particular, the single crossing property is not required. As a consequence, (a) it is robust to the introduction of moral hazard, as studied by Chassagnon and Chiappori (1997). In their paper, the mere definition of “lower risk” is complex because riskiness is endogenous to the proposed contracts; agents of a given type may be more or less risky depending on the contract they face. Still, it can be shown that *ex post* (equilibrium) accident probabilities are higher for contracts entailing more comprehensive coverage. (b) Also, it remains valid in the multidimensional adverse selection case studied by Villeneuve (1996), where agents differ by their risk *and* their risk aversion (even though profits may in that case be positive in equilibrium).

3. It remains valid when agents differ not only by the probability of an accident but also by its severity, if some monotone likelihood ratio property holds (see Landsberger and Meilijson 1996).

4. Even more important is the fact that this property still holds in a dynamical setting. The literature on repeated adverse selection clearly indicates that partial pooling may occur, especially during the initial stages, and that revelation mechanisms are much more complex.⁶ But the basic intuition remains valid.

A key remark, however, is that prediction 3 does not imply anything as far as pricing *across* risk classes is concerned. The self-selection issue applies only *within* such classes, that is, to observationally identical agents. In all that follows, we restrict ourselves to this situation. The empirical translation is that the probability distributions we shall consider will be conditional on all observables; our main goal will be to test for a positive correlation between (conditional) laws governing the choice of a contract on the one hand and the occurrence of an accident on the other hand.

Section II deals with the empirical distinction between adverse selection and moral hazard. Section III surveys the empirical literature and particularly the paper by Puelz and Snow (1994). The empirical application is presented in Section IV, and Section V provides some concluding remarks.

II. Adverse Selection versus Moral Hazard: An Empirical Puzzle

Models other than that of Rothschild and Stiglitz (1976) may yield prediction 3. For instance, the choice of contract could be deter-

⁶ Typically, “low-risk” individuals will be proposed contracts with higher deductibles *and* a stronger proportion of experience rating; see Dionne and Doherty (1994).

mined by totally different factors (say, preferences or risk aversion) that, by coincidence, turn out to be positively correlated with risk. But, of course, this explanation is in fact very close to the initial story; the difference is more a matter of interpretation than of structure. Agents certainly know their preferences better than the insurance company. If, in addition, preferences are correlated with risk, then agents indeed have a better knowledge of their risk, although in a somewhat indirect way.

A more convincing alternative relies on moral hazard. If this is prevalent, then agents who (for whatever reason) choose contracts with comprehensive coverage will have less incentive to reduce accident probability through prevention. The final consequence will be a positive correlation between the choice of a contract with a smaller deductible and *ex post* riskiness. But, of course, causality is reversed here. Agents do not select a particular contract because their accident probability is low; on the contrary, their accident probability is low because the contract they are facing provides adequate incentives.

Our paper will concentrate on testing prediction 3 of the Rothschild-Stiglitz model, without checking whether it holds because of adverse selection or moral hazard. Although the general problem of distinguishing between adverse selection and moral hazard from insurance data is left for future research,⁷ in Section IV below we offer a simple test that exploits the specifics of the French experience rating system.

III. Empirical Issues

A basic reference of the empirical literature on adverse selection is provided by the work of Puelz and Snow (1994).⁸ Using individual data from an automobile insurer in Georgia, they build a two-equation model that predicts different estimates for the competing

⁷ An ideal situation for testing moral hazard occurs when a given set of agents experience a sudden and exogenous change in the incentive structure they are facing. See, e.g., the analysis of the Quebec regulatory reform by Dionne and Vanasse (1996). The “natural experiment” approach is all the more convincing when there exists a reference sample for which incentives did not change; this is exploited in Chiappori, Durand, and Geoffard (1998) and Chiappori, Geoffard, and Kyriazidou (1998). Similar ideas may on some occasions apply even in the absence of a proper natural experiment. In principle, any context in which similar individuals are facing different incentive schemes can do—provided, of course, that the selection into the various schemes is exogenous and not related to risk-relevant characteristics. See Cardon and Hendel (1998) as well as Sec. IV below. Finally, investigating the dynamics of the relationship provides a very strong test (see Chiappori and Heckman 1999).

⁸ See also Dahlby (1983, 1992) and Boyer and Dionne (1989) for early references.

models of adverse selection in insurance markets. They conclude that their findings support the Rothschild-Stiglitz model. They first estimate a pricing equation of the form

$$P_i = g(D_i, X_i, \epsilon_i),$$

where P_i and D_i are the premium and the deductible in the contract chosen by individual i , the X_i are individual-specific exogenous variables, and ϵ_i is an econometric error term. The second equation describes the individual's choice of deductible. The decision depends on his "price of deductible" \hat{g}_D ,⁹ as estimated from a third regression using instrumental variables and on his (unobserved) accident probability π_i . Since his accident probability is unobservable, it is proxied by a dummy variable RT_i that equals one if the individual had an accident and zero otherwise. This leads to an equation of the form

$$D_i = h(\hat{g}_D, RT_i, X_i, \eta_i),$$

where η_i is another error term. The Rothschild-Stiglitz model predicts that higher risks buy better coverage, that is, a lower deductible, so that h should decrease in RT .

Puelz and Snow specify their first equation as a linear model and estimate it by ordinary least squares. Since there are only three levels of deductible in their data set, they estimate their second equation (again linear) by ordered logit. Their estimates exhibit the decreasing relationship predicted by Rothschild and Stiglitz, although as evidenced by their table 3, the choice of deductible does not vary much (*ceteris paribus*) with the risk type.

There are, however, several flaws in the Puelz-Snow approach that cast some doubts on the validity of their results. A first one is related to the choice of their risk type variable RT_i . While the expectation of this dummy variable clearly is π_i , this procedure introduces a measurement error into the second equation. In linear models, the estimates would be biased toward zero, which would reinforce the conclusion of Puelz and Snow. In an ordered logit, it is not clear which way the bias goes.

A more damning criticism of Puelz and Snow's work is that they use highly constrained functional forms. They rely on a set of only about 20 variables (we shall use 55 in our application, even though we consider a homogeneous subsample of "young" drivers). This implies that their estimates may suffer from omitted variables bias. Moreover, their functional forms are linear. Assume, for instance,

⁹ This estimate is clearly negative since no rational insured would pay a higher premium for a higher level of the deductible.

that individual i has constant absolute risk aversion σ_i . Then we easily derive the individual's choice of deductible

$$D_i = \frac{1}{\sigma_i} \log \frac{1 - \pi_i}{\pi_i} \frac{-\hat{g}_{Di}}{1 + \hat{g}_{Di}},$$

which is highly nonlinear. In fact, applying the Puelz-Snow procedure to data generated by a *symmetric* information model according to this formula may well result in the kind of negative estimates they get, simply because the accident term in fact captures some of the omitted nonlinearities. Dionne, Gouriéroux, and Vanasse (1997) indeed show that Puelz and Snow's results may be spurious. To do this, they include the predicted probability of accident $\hat{\pi}_i$ in the right-hand side of the second equation (for the choice of deductible).¹⁰ Note that now the impact of $\hat{\pi}_i$ (if any) has nothing to do with adverse selection since $\hat{\pi}_i$ is, by construction, a function of only the observed variables. They show that this completely breaks the results of Puelz and Snow: the variable RT is not significant any more.

Finally, the data set used by Puelz and Snow comprises individuals of various ages and driving records; nothing is done in their paper to control for this source of heterogeneity. Two problems arise at this point. One is heteroskedasticity. Presumably, the distribution of the random shocks, and especially of η_i , will depend on the driver's seniority. Within a nonlinear model such as the ordered logit, this will bias the estimation. The second and more disturbing problem relates to the estimation of adverse selection. Insurers typically observe past driving records; these records are highly informative on probabilities of accident and, as such, are used for pricing. These variables, however, are omitted in Puelz and Snow's regressions. This omission generates a bias that indeed tends to overestimate the level of adverse selection: the corresponding information is treated by the econometrician as being private, whereas it is in fact common to both parties.

This remark clearly suggests that the tests should consider drivers with similar seniority and moreover control for driving experience. The introduction of this variable, however, is quite delicate because it is (obviously) endogenous. Various strategies can be considered, but, ideally, panel data would be required (see Chiappori and Heckman 1999). In our application, we shall circumvent this problem by using only data on beginners.

¹⁰ This is perfectly legitimate since $\hat{\pi}_i$ is just a particular nonlinear function of the observables.

IV. Implementation and Results

This section contains the heart of the paper. We first present the French car insurance system and our data set. Then we specify our econometric methods and present results on beginning drivers. Since our results are negative, we then turn to more senior drivers, for whom the results are equally negative.

A. Car Insurance in France

French law stipulates that all cars must be insured at the *responsabilité civile* (RC) level, a liability insurance that covers damage inflicted to other drivers or their cars. Every car insurance company offers this coverage, along with other noncompulsory coverage. The most typical other contract is *assurance tous risques* (TR), which also indemnifies damage to the insured's car (or driver), for which he or she is held responsible. Comprehensive insurance contracts are differentiated by the value of the deductible (which may be fixed or proportional).

All insurers are required by law to apply a uniform experience rating system, the "bonus/malus." At any date t , the premium is defined as the product of a basic amount, which is freely defined by firms but cannot be related to past experience, and a "bonus" coefficient, the evolution of which is strictly regulated. Precisely, if the bonus coefficient is b_t at the beginning of the period, the occurrence of an accident implies a 25 percent increase ($b_{t+1} = 1.25b_t$), whereas no accident during the year results in a 5 percent decrease ($b_{t+1} = .95b_t$); in addition, several special rules are imposed.¹¹ Moreover, insurance companies are allowed to overprice contracts by young drivers (less than three years of insurance); the surcharge is limited to 140 percent of the basic rate and must decrease by half every year in which the insured has not had an accident.

The law of the single price does not hold very well in the data: even for the RC guarantee, which is quite homogeneous, there are large variations in premia that do not appear to be justified by variations in insured/car characteristics. The following orders of magnitude may still be useful. For the average insured in our sample, the RC premium is about 3,000 francs; the average car costs about 50,000 francs, and the probability of an accident in a given year is about 10 percent (very few drivers—fewer than 2 percent—have more than one accident in a given year). The TR premium is at least twice

¹¹ In the French system, b_t cannot exceed 3.5 and cannot go below 0.5. Also, b_t cannot be larger than one if no accident occurred for the last two years.

as expensive, and that multiple is still higher for high-price cars. Therefore, the decision of buying the RC or the TR contract is an important one.

B. The Data: Concentrating on Young Drivers

The French federation of insurers (FFSA), which groups 21 companies that together have 70 percent of the French automobile insurance market, conducted in 1990 a survey of its members. The sampling rate was 1/20. The resulting data sets consisted of 41 variables on 1,120,000 contracts and 25 variables on 120,000 accidents, covering the year 1989. After a previous team merged them and cleaned them up, we chose to concentrate on a data set of 20,716 “young” drivers, that is, all drivers who obtained their driver’s license in 1986, 1987, or 1988.¹² For each driver, we have all the variables that are used by insurance companies to price their contracts—age, sex, profession, year of driver’s license, age and type of the car, use of the car, and area—plus the characteristics of the contract and the characteristics of the accident if one occurred. There are several variables in most of these categories, so that our data set is fairly rich.

Concentrating on young drivers has several advantages. One is that the heteroskedasticity problem is probably much less severe on a sample of young drivers since the drivers’ experience is much more homogeneous than in a population in which different seniority groups are mixed up. More important, concentrating on young drivers avoids the problems linked to experience rating and the resulting bias mentioned at the end of Section III.

A final issue is the distinction between an accident and a claim. Since our data set comes from insurance companies, it relates to claims, not to accidents. But whether an accident, once it has occurred, becomes a claim (i.e., is declared to the insurance company) is the individual’s decision. This decision is in turn influenced by the nature of the contract. Obviously, an accident that is not covered will not be declared. This “ex post moral hazard” effect typically generates a correlation between the type of contract and the probability of a claim even in the absence of any ex ante moral hazard (i.e., even when the *accident* probability is not affected by the type of contract). Still, it is essential to distinguish between them, if only because the welfare consequences are very different.¹³ Our solution

¹² Following the tradition in the French insurance industry, we call “young” the recent drivers, most, but not all, of whom are also young in age.

¹³ A limitation of insurance coverage may be welfare increasing when it reduces accident probability, much less so when its only effect is to discourage victims from filing a claim, unless, of course, the technology of the insurance industry (say, the presence of fixed costs) makes the processing of small claims especially inefficient.

is to discard all accidents in which only one automobile was involved. Whenever two cars are involved, a declaration is much more likely to be made in any case.¹⁴

C. *The Procedures*

As explained in the Introduction, we want to test the conditional independence of the choice of better coverage and the occurrence of an accident, where “conditional” means conditional on all variables observed by the insurer. To do this, we shall use two parametric methods and three nonparametric methods.

1. A Pair of Probits

We define here our notation. Let $i = 1, \dots, n$ denote individuals. The term X_i is the set of exogenous variables for individual i (these variables will be constants and dummy variables in our application). Also, let w_i denote the number of days of 1989 in which individual i was insured. We now define two 0-1 endogenous variables: (1) $y_i = 1$ if i bought (any form of) comprehensive coverage (a TR contract); $y_i = 0$ if i bought only the minimum legal coverage (an RC contract); (2) $z_i = 1$ if i had at least one accident in which he was judged to be at fault; otherwise (no accident or i not at fault) it is zero.

These definitions call for two remarks. First, there are many different comprehensive coverage contracts on offer, with (say) different levels of deductible. Ideally, these contracts should be treated separately and not bundled together as we do here. However, this would greatly complicate the model.¹⁵ Second, we separate accidents in which the insured is at fault and those in which he is not. The reason is that if the insured has an accident in which another driver is to blame, any information on his risk type may not be conveyed. Also,

¹⁴ In principle, the two drivers may agree on some bilateral transfer and thus avoid the penalties arising from experience rating. Such a deal is, however, quite difficult to implement between individuals who meet randomly, will probably never meet again, and cannot commit in any legally enforceable way (since declaration is compulsory according to insurance contracts). We follow the general opinion in the profession that such bilateral agreements can be neglected. An alternative solution is to concentrate on accidents with bodily injuries, since in that case declaration is compulsory by law (see, e.g., Levitt and Porter 1998). However, this drastically reduces the number of accidents.

¹⁵ In addition, deductibles are in any case quite small. In expected terms, the difference between the various levels of deductible available in our sample is negligible when compared to that between comprehensive and partial coverage, on which we concentrate here. Experience rating is more important, but, by law, experience rating schemes are identical across firms.

we do not exploit the further information linked to drivers who had several accidents in 1989; again, there are very few of these cases.

We now set up two probit models, one for the choice of coverage and one for the occurrence of an accident. Let ϵ_i and η_i be two independent centered normal errors with unit variance. Then

$$y_i = \mathbf{1}(X_i\beta + \epsilon_i > 0)$$

and

$$z_i = \mathbf{1}(X_i\gamma + \eta_i > 0).$$

We first estimate these two probits independently, weighing each individual by the number of days under insurance w_i . Then we can easily compute the generalized residuals $\hat{\epsilon}_i$ and $\hat{\eta}_i$. For instance, $\hat{\epsilon}_i$ is given by

$$\hat{\epsilon}_i = E(\epsilon_i | y_i) = \frac{\phi(X_i\beta)}{\Phi(X_i\beta)} y_i - (1 - y_i) \frac{\phi(X_i\beta)}{\Phi(-X_i\beta)},$$

where ϕ and Φ denote the density and the cumulative distribution function (cdf) of $N(0, 1)$. Now define a test statistic by

$$W = \frac{\left(\sum_{i=1}^n w_i \hat{\epsilon}_i \hat{\eta}_i \right)^2}{\sum_{i=1}^n w_i^2 \hat{\epsilon}_i^2 \hat{\eta}_i^2}.$$

The general results in Gouriéroux et al. (1987) imply that under the null of conditional independence $\text{cov}(\epsilon_i, \eta_i) = 0$, W is distributed asymptotically as a $\chi^2(1)$. This provides us with a test of the symmetric information assumption.

To implement this procedure, we first need to choose what exogenous variables to include in X_i . The most contentious variable here is the past driving record, as represented by the bonus/malus coefficient (defined above). If we exclude this variable, then we neglect some of the insurer's information and our test will be biased. If we include it, it may also be biased since this variable is likely to be correlated with η_i . As indicated above, our solution is to focus in a first step on drivers who have no past driving record, or "beginners." In our data, this refers to drivers who obtained their driver's license in 1988. There are 6,333 of them in our sample. As for the other exogenous variables, we chose to include those that, according to the insurers, are most relevant: thus we have dummy variables for sex (1), make of car (7), performance of the car (5), type of use

TABLE 1
TESTING INDEPENDENCE

Population	$z_i = 0$	$z_i = 1$
$y_i = 0$	N_{00}	N_{01}
$y_i = 1$	N_{10}	N_{11}

(3), type of area (4), age of driver (8), profession of driver (7), age of car (11), and region (9). This gives us 55 exogenous variables plus a constant. They all are clearly observed by insurers since the insurers built this data set themselves.

2. A Bivariate Probit

Estimating the two probits independently is appropriate under conditional independence, but it is inefficient under the alternative. For this reason, we also estimate a bivariate probit in which ϵ_i and η_i are still distributed as $N(0, 1)$ but have a correlation coefficient ρ , which we also estimate. This will allow us to test $\rho = 0$ but also to get a confidence interval for ρ .

3. A χ^2 Test

The two parametric procedures presented above rely on a fairly large number of exogenous variables, as compared to those in Puelz and Snow (1994), for instance. However, the functional forms involved are still relatively restrictive since the latent models are linear and the errors are normal. It is quite possible, for instance, that the data-generating process is driven by cross effects or even more complicated nonlinear functions of the exogenous variables. Then our results would be biased in unpredictable ways. To remedy this, we adopt here a fully nonparametric procedure based on χ^2 tests for independence.

Let x_i be a set of m exogenous 0-1 variables. Then we can define 2^m "cells" in which all individuals have the same values for all variables in x_i . In each cell, we compute a two by two table generated by the values of y_i and z_i , as in table 1. For $j = 0, 1$, define $N_j = N_{j0} + N_{j1}$, $N_{.j} = N_{0j} + N_{1j}$, and $N_{..} = N_0 + N_1$. Now consider the test statistic

$$T = \sum_{j,k=0,1} \frac{[N_{jk} - (N_j N_{.k} / N_{..})]^2}{N_{jk}}$$

This is the well-known χ^2 test for independence. If y_i and z_i are independent in the cell we consider, then T is asymptotically distributed as a $\chi^2(1)$.

Thus we face a collection of $M = 2^m$ statistics. There are many ways to use these data to derive a test for conditional independence. We shall use three different approaches. The first one is to do a Kolmogorov-Smirnov test on the empirical cdf \hat{F}_M . Let F denote the cdf of a $\chi^2(1)$. Under conditional independence, the test statistic

$$K = \sqrt{M} \sup_x |\hat{F}_M(x) - F(x)|$$

converges to a distribution that is tabulated in statistics textbooks. As the Kolmogorov-Smirnov test is known to have limited power, we complement this by two other tests. Our second approach is to reject independence in a cell if the value of T for that cell is greater than 3.84, which is the 5 percent critical value for a $\chi^2(1)$. Then we count the number of rejections, which is distributed as a binomial $B(M, 0.05)$ under conditional independence. We shall also report the value of the statistic S generated by adding up the T values for all cells. This is distributed as a $\chi^2(M)$ under conditional independence.

To implement this procedure, we first need to select the exogenous variables in x_i . Letting $x_i = X_i$ would clearly not do since our total population is much smaller than 2^{55} . More generally, we face a trade-off between better conditioning (and thus a large number of cells) and not too small populations in each cell (to apply asymptotic results). To resolve this dilemma, we again rely on the insurer's experience. This tells us that six variables are highly relevant: (a) the sex of the driver, (b) the make of his car, (c) the size of his car, (d) whether the car is used in a city, (e) the age of the driver, and (f) the age of the car. Thus we construct six 0-1 variables to get x_i . This will give us 64 cells.

D. Results on Beginners

First turn to our pair of independently estimated probits. Since we have 112 estimated coefficients, it would be impractical to comment on them all.¹⁶ We are not really interested in their values or significance levels anyway, since what matters to us is the value of the test

¹⁶ No fewer than 25 coefficients are (individually) significant at the usual 5 percent level. To quote a simple example, men who drive old Japanese cars are less likely to buy comprehensive coverage. On the other hand, only the constant is significant in the probit on accident frequency. Insurers tell us that this is a standard result for beginning drivers. However, as expected, younger people have more accidents.

statistic W . This turns out to be very small, at 0.46. Thus we are very far from rejecting conditional independence.

The bivariate probit gives us a similar message. Our estimate for ρ actually is slightly negative, at -0.029 . However, its estimated standard error is 0.049, so that again we cannot reject conditional independence. Moreover, we can compute a 95 percent confidence interval for ρ : it is $[-0.125, 0.067]$, so that even if ρ is not actually zero, it is bound to be very small.

Turning to our nonparametric procedure, we compute a value of $K = 0.63$ for the Kolmogorov-Smirnov statistic. This is much below the 5 percent critical value, which is 1.35. Moreover, we obtain only five rejections out of our 64 cells. This corresponds to a 15 percent p -value for $B(64, 0.05)$, so that again, we cannot reject independence. It is interesting to look at the cells in detail. The average cell has about 100 individuals, but there is a large dispersion of cell sizes: the smallest cell has six individuals, there are five more with fewer than 20 individuals, and the largest cell has 373. Fortunately, the rejections are not in the smallest cells, so that our procedure seems reasonably reliable. The only anomaly occurs in a large cell (315 individuals) in which independence is very strongly rejected ($T = 13.4$). However, splitting the cell according to a seventh criterion (type of income) leads to more reasonable values for T . Summing up all T values leads to a test statistic of $S = 69.3$, and the 5 percent critical value for a $\chi^2(64)$ is 87.5.

Thus all our procedures give the same, somewhat surprising, result: asymmetric information seems to be at most a negligible phenomenon in the market for automobile insurance, at least for young drivers.

E. Results on More Senior Drivers

Our results so far indicate that beginners have no informational advantage over insurers. This can be interpreted by saying that when signing their first contract, young drivers have very little driving experience and thus do not really know whether they will turn out to be good drivers or not. However, one can imagine that after a couple of years, they will learn something about their driving ability. If they get this information by having accidents, then the insurer will learn at the same pace. However, they may learn that they are bad drivers even if they manage to stay out of trouble. For instance, a driver who has several "near misses" will eventually know more than the insurer about his risk type. Thus it is quite possible that adverse selection emerges endogenously in the market for automobile insurance since

drivers who avoid accidents may obtain an informational advantage over the insurer.

Fortunately, our data also allow us to test this model since we have data for relatively senior drivers, who obtained their driver's license in 1986, and we also know their bonus/malus, which sums up how many accidents they had in the past. Given the French legal bonus/malus system, we know that any driver who has had at least one accident since 1986 will have a bonus/malus coefficient greater than 100 (the reference level) in 1989. Thus we can focus on those drivers who got their license in 1986 and had a bonus/malus no greater than 100 in 1989. There are two caveats in this procedure. First, it is quite possible that a driver who got his license in 1986 started driving only in 1989; we have no way to check this from the data. Second, as discussed in subsection *F* below, French insurers appear to take some liberty with the bonus/malus system; in particular, some beginners start with the (favorable) coefficient of their father, who presumably is a good customer. Thus someone who has a bonus/malus lower than 100 in 1989 may actually have had an accident before 1989. Unfortunately, we do not have data on the history of individuals to sort out these two difficulties. We just hope that they are not too severe.

Here we apply our three procedures to the data set consisting of all drivers who obtained their license in 1986 and started 1989 with a bonus/malus no greater than 100. There are 6,802 such individuals.

After estimating the two probits independently, we compute a test statistic $W = 0.25$. Thus again, there is no way we can reject conditional independence. Estimating the bivariate probit yields the same conclusion: the estimated ρ is 0.024, with estimated standard error 0.057 and 95 percent confidence interval $[-0.088, 0.136]$. Finally, the nonparametric procedure leads to only one rejection of independence, which of course is much below the 5 percent critical value. The value of the statistic S is 59.3, again much smaller than the critical value. The Kolmogorov-Smirnov test gives the same message, as $K = 0.82$.

Thus all our tests consistently give us evidence against the presence of asymmetric information. This holds for both "exogenous" asymmetric information as in the preceding subsection and the less usual endogenously evolved form that we tested here.

F. A Specific Test for Moral Hazard

As argued in the Introduction, distinguishing between adverse selection and moral hazard is a difficult task when only static data are available. We suggest there that the dynamics of contracts, especially

through experience rating, could provide tests in which adverse selection and moral hazard generate opposite predictions. However, such tests require panel data, which are not available at the moment.

In this subsection, we describe a test that, while requiring only cross-sectional data on beginners, keeps part of the flavor of the previous ideas. This test is based on a peculiarity of the French regulation. In the bonus/malus system described earlier, a young driver should in principle start with a bonus coefficient equal to one (i.e., pay the standard premium corresponding to his profile). As it turns out, however, a significant proportion of young drivers (19.5 percent in our sample) started instead with the minimum possible coefficient, namely .5, which implies a 50 percent reduction in the premium. After discussing this apparent anomaly with several insurers, we realized that this reduction could be explained by a constraint that limits the implementation of the bonus system. Many young drivers face a simple choice. If the car they drive is declared as their personal belonging, they will pay whatever premium the insurance company will charge a young driver. However, they may also declare the car as belonging to one of their parents; the parent is then allowed to lend them the car, at an extra cost that reflects the extra premium required for a young driver. This allows young drivers to benefit from their parents' bonus coefficient. Since a high proportion (more than two-thirds) of the drivers in our total sample (all seniorities included) do benefit from the maximum reduction ($b_i = .5$), this constitutes a potentially serious problem.¹⁷ It was explained to us that, in such a case, insurance companies may sometimes simply replicate the parents' bonus in the child's policy.

It is easy to see that any driver, whatever her risk, will be more tempted to buy comprehensive coverage if her bonus coefficient goes from one to .5. The data confirm this: running a probit on the choice of comprehensive coverage with an added explanatory variable that equals one if and only if the bonus is .5 shows that this new variable has a positive and significant coefficient. A more interesting question is whether this reduction has an impact on accident probabilities. Three possible stories can be considered: (1) Assume, first, that the parents' performances are positively correlated with the child's. Then the 50 percent bonus signals a better driver and should be *negatively* correlated with accident probability. (2) A second possibility is that the parents' performances are uncorrelated

¹⁷ It is not totally clear why not all young drivers use this opportunity. We were told that a typical insurance company will be reluctant to accept such a deal unless the father is a good customer and that, in addition, many young drivers prefer to own their car.

with the child's, and there is no moral hazard. Then the 50 percent bonus is allocated randomly and should not matter for accident probabilities. (3) Finally, assume that parents' and child's performances are uncorrelated but there is some moral hazard. Then we are facing a kind of natural experiment, as some drivers face a different incentive scheme for exogenous reasons. Since the marginal cost (to the insured) of an accident is increasing with the bonus coefficient, one should expect that a lower bonus coefficient decreases incentives and thus leads to *larger* accident probabilities.

We hence reestimated the probit on accident frequency with the additional variable. We found that its coefficient is negative and significant, which rejects the moral hazard hypothesis. We also computed our $\chi^2(1)$ test for independence of the residuals of the two probits. This is again very low, so that adverse selection does not seem to be present either.

V. Conclusions

A. *Misspecification and Spurious Correlation: An Asymmetric Danger*

A lesson of this paper, as compared to the earlier literature, is that conditioning on observables is a rather delicate task. It requires large samples and numerous variables. It is quite easy, in particular, to mistakenly conclude that adverse selection does exist; this is likely to occur, for instance, whenever the various nonlinearities existing in either the pricing schedule or the accident probability have not been properly taken into account. We believe, in particular, that the use of simple, linear functional forms (such as logit or probit) should be restricted to homogeneous populations. In our empirical work, we restrict our attention to a sample of "young" drivers and find no evidence of adverse selection.

A second claim is that the problem we just evoked does not arise in a symmetric way. While the omission of some variables can obviously generate spurious correlations between contract choices and accident frequency, it is much more difficult to figure how it could hide some actual relationship. In other words, we believe that a negative conclusion, like the one we get, is probably much more robust than any positive finding could be. This claim should, however, be qualified. In fact, as our final result on the effect of the bonus coefficient clearly suggests, it may be the case that some variable that is observed by the insurers but somehow is not recorded in our data set influences contract choice and riskiness in opposite directions, and that it cancels out a conditional dependence in our estimates. However,

such a situation seems unlikely because (i) our procedures include a large number of explanatory variables and are used on a relatively homogeneous subsample, and (ii) it would require an exact cancellation between the effect of the omitted variable and the adverse selection residual effect, which seems implausible.

B. Adverse Selection: Risk or Preferences?

Our main finding is that, although unobserved heterogeneity on risk is probably very important, there is no correlation between unobservable riskiness and contract choice. In other words, when choosing their automobile insurance contracts, individuals behave as though they had no better knowledge of their risk than insurance companies. This interpretation is fully consistent with the view generally shared by French automobile insurers, namely that the information at the company's disposal is extremely rich and that, in most cases, the asymmetry, if any, is in favor of the company.¹⁸ This is, we believe, the most natural interpretation of our results: we do not find evidence of (risk-related) adverse selection because this phenomenon does not exist (or only to a very limited extent) in the market under consideration. A surprising consequence is that many variables that one might believe to be correlated with risk appear to be in fact irrelevant. A typical example is income. Insurance companies do not ask any question about income (which, incidentally, suggests that the datum they have—mainly the brand of the car—is a good enough proxy). As a result, income acts as an omitted variable in both our regressions. Since it is hard to believe that income is not correlated with the partial versus full coverage decision,¹⁹ it must be the case that it is uncorrelated with accident probabilities, at least after one controls for all observables. Another striking example is annual mileage. This information is obviously available to the driver. Insurers, on the other hand, do not use it in general, although conditioning on actual mileage would not raise major technical problems.²⁰ This fact, by itself, suggests that the issue may not be crucial. Insurers believe that the mileage variable is very correlated with such

¹⁸ To quote the chief executive officer of a major company: "The really risky clients are those who believe they are first-class drivers!"

¹⁹ The sign of this effect is not clear. While the kind of "peace of mind" that insurance buys could be thought of as a normal good, there is strong empirical evidence that risk aversion decreases with wealth.

²⁰ The client could be asked to declare *ex ante* what his mileage will be. The key point is that this declaration will be cross-checked only if an accident occurs, which is easy (since an expert will see the car in any case). Actually, after the survey had been completed, one firm (Mutuelles du Mans) did introduce a contract contingent on mileage. This contract was successful, but by no means overwhelmingly so.

proxies as area of residence or type of use (salespeople, for instance, have special contracts).

Another disturbing implication of our results is that moral hazard is not important either in this context. Two caveats should, however, be introduced. One is that, to some extent, moral hazard is taken care of through deductible and experience rating. If the effort associated with careful driving is not too costly, these tools may be sufficient. In addition, as argued above, we consider only accidents involving several drivers; this restriction is needed to avoid the “accident versus claim” bias. Insofar as moral hazard (or adverse selection, for that matter) is particularly strong for accidents involving only one driver (say, being careful is especially important when exiting one’s own garage), we cannot detect it.

This does not mean, however, that asymmetric information is absent in this particular relationship, but rather that it takes a different form. While theoretical models concentrate on one particular source of adverse selection—the individual’s better knowledge of her risk—the empirical relevance of this exclusive emphasis is not always guaranteed. Risk is not the only possible source of informational asymmetry and probably not the most important one. There are good reasons to believe that individuals know better their own preferences and particularly their level of risk aversion—an aspect that is often disregarded in theoretical models. The presence of preference-related adverse selection would explain both the existence of menus of contracts offered to clients and the absence of correlation between their choice and their accident probability. An extreme version of this is the so-called cherry-picking story. Assume that individuals have different risk aversions and that more risk-averse drivers tend to both buy more insurance and drive more cautiously; this would even suggest a *negative* correlation between insurance coverage and accident frequency.

Appealing as it may be, the cherry-picking story raises several questions. First, the link between higher risk aversion and higher prevention efforts is not direct, especially when a comprehensive insurance contract is available.²¹ More important, the nature of the competitive equilibrium is more complex. Assume that heterogeneity occurs between low-risk but highly risk-averse drivers and high-risk but less risk-averse ones. Without loading, partial coverage just cannot be

²¹ Jullien, Salanié, and Salanié (1999) study this issue and exhibit sufficient conditions for more risk-averse individuals to behave more cautiously. This, however, requires partial (or no) insurance. In any moral hazard model, full insurance is always associated with minimum effort and maximum risk, whatever risk aversion may be. Insurance is indeed partial in the French case (mainly because of experience rating), although the cost left to the insured remains small.

used as a signaling device. If full coverage contracts attract more risk-averse individuals, then the zero-profit equilibrium condition would indeed require their unit price to be lower (reflecting lower accident probability). But such a feature obviously violates revelation constraints.²² As a result, equilibrium would be pooling, and no menu will be offered. This intuition, however, does not hold in the presence of loading. In a recent paper, de Meza and Webb (1999) study a model with moral hazard and adverse selection on risk, in the presence of loading. They show that equilibria can be either pooling or semipooling. In the latter case, their model implies a negative correlation between full coverage and accidents, although the correlation may be small. Whether additional tests can be designed in order to distinguish the cherry-picking story from the simplest explanation (adverse selection is simply absent) is still an open question.

C. Prospects for Future Work

As argued above, the absence of adverse selection is perhaps less surprising in the case of young drivers since they are likely to have a very imprecise perception of their potential riskiness. The question, at that point, is whether learning can be expected to modify this situation and, more precisely, whether the driver's experience allows her to learn about her true ability faster than the insurance company. Our test in Section IVE on drivers with three years' seniority seems to point to the contrary. However, it may be that the effect exists for more senior drivers. If this is the case, then *theoretical models of asymmetric learning* should be further developed. This is an area left for future research (however, see de Garidel [1997]).

Another possible development lies in discriminating between adverse selection and moral hazard. Our simple test of Section IVF is only a first step in that direction. We believe, however, that exploiting dynamic data is the more promising route. An immediate consequence of the experience rating system is that, at least for fairly good drivers, the marginal cost (in dollars) of an accident increases with the number of past accidents. In a pure moral hazard setting, it follows that incentives are stronger the more accidents the driver had in the past. In other words, everything equal, the accident proba-

²² Note that the argument would be different in a principal-agent model because the insurance company could then exploit its monopoly power to charge *higher* rates to low-risk/high-risk aversion individuals, thus discriminating on their higher willingness to pay for insurance. This suggests that the cherry-picking argument might hold in a noncompetitive framework. This requirement probably excludes the example of automobile insurance but may apply in different contexts.

bility, conditional on past experience, should decrease with the number of past accidents. In an adverse selection setting, on the contrary, any accident signals a "risky" driver and thus increases the perceived probability of future accidents. Thus it should be possible to test between adverse selection and moral hazard if panel data are available. This point is further developed in Chiappori and Heckman (1999).

Finally, we should stress that our results are specific to the automobile insurance market. There are good reasons to believe that asymmetric information is much more relevant in other markets. While Cawley and Philipson (1997) do not find evidence for adverse selection on a sample of life insurance contracts, Cardon and Hendel (1998) conclude that moral hazard (although not adverse selection) is present in health insurance. Anecdotal evidence and preliminary studies also suggest that both adverse selection and moral hazard are important in unemployment mortgage insurance. Finally, it should be noted that government regulation can introduce adverse selection when it forbids the insurer to condition the contract on some observables. Whether automobile insurance markets entail adverse selection in more heavily regulated states or countries is an interesting and still open question.

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