ATTRITION IN THE RAND HEALTH INSURANCE EXPERIMENT: A RESPONSE TO NYMAN

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Abstract

In a prior article in this *Journal* John Nyman argues that the effect on health care use and spending found in the RAND Health Insurance Experiment is an artifact of greater voluntary attrition in the cost sharing plans relative to the free care plan. Specifically, he speculates that those in the cost sharing plans, when faced with a hospitalization, withdrew. His argument is implausible because: 1) Families facing a hospitalization would be worse off financially by withdrawing; 2) A large number of observational studies find a similar effect of cost sharing on use; 3) Those who left did not differ in their utilization prior to leaving; 4) If there had been no attrition and cost sharing did not reduce hospitalization rates, each adult in each family that withdrew would have had to have been hospitalized once each year for the duration of time they would otherwise have been in the Experiment, an implausibly high rate; 5) There are benign explanations for the higher attrition in the cost sharing plans. Finally, we obtained followup health status data on the great majority of those who left prematurely. We found the health status findings were insensitive to the inclusion of the attrition cases.

John Nyman (Nyman 2007) argues that the usual interpretation of the RAND Health Insurance Experiment – modest cost sharing reduces use of services with negligible effects on health for the average person but adverse consequences for the poor and sick − is an artifact that results from greater attrition by those in plans with cost sharing. In particular, he speculates that if those facing hospitalization on cost-sharing plans differentially dropped out of the Experiment, the observed medical expenditure would be lower and health status would be better among those remaining in the cost-sharing plans.

When we analyzed the data from the Experiment, we were sensitive to the possibility that differential attrition by plan – and although Nyman does not note it, also differential refusal rates by plan at the time of enrollment – could have affected our results. We therefore conducted several analyses of both refusal and attrition that convinced us that Nyman's speculation is unwarranted and that the usual interpretation of our results is correct. Those analyses are described on pages 17-26 of (Newhouse and the Insurance Experiment Group 1993), which is reproduced on Newhouse's home page (http://www.hcp.med.harvard.edu/people/hcp_core_faculty/joseph_newhouse); there is further analysis of these issues in (Newhouse et al. 1987).

We take up Nyman's arguments about the RAND Experiment sequentially and conclude with a comment on the first part of Nyman's paper and its relationship to the RAND Experiment.

Incentives to Attrite. Nyman focuses on differential adult attrition in the free plan relative to the cost-sharing plans – over the 3-5 year period of enrollment of the study, 0.4 percent of the adult participants in the plan with no cost sharing voluntarily dropped out whereas 6.7 percent in plans with some cost sharing did so. He speculates that the differential represented persons who were about to be hospitalized, but instead dropped out and charged the hypothetical hospitalization to their prior insurance in order to save money. He supports this speculation by citing a number of studies that demonstrate participants change plans when it is in their financial interest to do so.

Those observational studies, however, are irrelevant to the Experiment. Unlike them, we designed the Experiment so that it would never be in a person's financial interest to leave the Experiment and return to their old plan. We did so by making unconditional monthly side payments to the participants in the cost-sharing plans that held them harmless against their worst case (the "Participation Incentive"), and we paid all participants an additional lump sum if they completed the experiment (the "Completion Bonus"). These side payments were sufficiently large that it was always in the financial interest of the Experiment's participants to enroll initially and to remain enrolled. Thus, there is no a priori reason to expect that financially motivated attrition occurred. By contrast, in the studies Nyman quotes to support his argument, the persons who changed insurance plans could be and probably were financially motivated.

Furthermore, all the Experimental cost-sharing plans had a stop-loss feature that was at most $1,000 for medical spending for the entire family for a year and less for lower income families. (The $1,000 is in late 1970s dollars, which is about $3,400 in 2007 dollars if one inflates by the all-items Consumer Price Index.) As a result, even had there been no side payments, many individuals facing hospitalization would have been better off financially on the Experimental plan than on the plan to which they could return, many of which had no stop-loss feature.

Could there have been any reason other than financial self-interest among persons about to go to the hospital that motivated the differential attrition in the cost-sharing plans? We discuss other possible explanations below, but first turn to the health status results.

Effects of Attrition on Health Status Results. At the end of the Experiment we were able to collect health status data on 77 percent of those who left the Experiment prematurely (85 percent of those who survived). These people are included in the reported health status results, so attrition should have had *de minimus* effects on those results.

Nyman, however, finds that "the most important implication of these attrition data" is a "reinterpretation of the health status results." He says:

> "Thus, if participants in the cost-sharing plans became ill and received about 23 percent fewer inpatient treatments, it is likely that their lack of equally effective and appropriate treatments would have shown up as reductions in the more important health-status measures. That these measures did not register declines in health suggests that an alternative explanation is likely. That alternative explanation is, of course, that many in the cost-sharing arms left the experiment voluntarily when they learned that they had become ill and needed a hospital procedure."

In other words, if there were really a 23 percent decline in inpatient use, as we believe, we should have observed a health status decline; because no health status decline was observed, there must not have been a true decline in use. We come back to this argument below after laying some groundwork.

Does Cost Sharing Reduce Use? As noted at the outset, Nyman argues that the observed reduction in use in the Experiment's findings is an artifact of differential attrition. The first thing to say about Nyman's speculation is that it flies in the face of results from countless studies using observational data that show cost sharing reduces use, as well as from premium quotes by actuaries for policies with different levels of cost sharing that imply effects on use (e.g., (Cherkin et al. 1989; Cockx and Brasseur 2003; Newhouse and Phelps 1976; Phelps and Newhouse 1974; Scitovsky and McCall 1977; van Vliet 2004; Wedig 1988)). Much recent work has focused on cost sharing for drugs and also finds an effect of differential copayment on use (e.g., (Contoyonnis et al. 2005; Goldman et al. 2004; Huskamp et al. 2005; Joyce et al. 2002; Landon et al. 2007;

Winkelmann 2004)). Even the rate of emergency room visits responds to cost sharing (e.g., (Hsu et al. 2006; Selby et al. 1996)). (That the cost sharing variation was relatively modest in these studies means the income transfer effect of insurance that Nyman describes in the opening part of his paper did not materially affect their estimated effects.) In short, Nyman's dismissal of the Experiment's result that cost sharing causally reduces utilization is greatly undermined by the enormous number of observational studies over many years, in many settings, with many different methodologies, that find utilization of medical services responds to relatively modest variation in cost sharing.

Given the volume of studies that have been conducted on the effect of cost sharing, Nyman's argument that the utilization effect we observed was an artifact and explains why we observed no health status effect for the average person is fanciful. The only real question is whether the differential attrition that did occur biased the observed utilization response away from zero; i.e., whether the observed response was overstated. But the studies cited above generally find a response that is roughly in the range of the RAND data (a price elasticity of -0.1 to -0.2). That is strong evidence that the observed response was not much affected by the differential attrition.

Are the Experiment's Utilization Results Biased? We were, of course, concerned that the differential attrition that did occur may have biased our results. This is why we gathered the health status information on those who left. Unfortunately, we could not obtain comparable utilization data on those who left because our source of the utilization data was claims, and once the family left, they no longer filed claims with us. We could, of course, analyze whether the use of those who left was similar to those who did not for the time we observed them. In our initial work we found that it was similar except for those who died. (About 1 percent of the initial enrollees died during the course of the study, a percentage that did not vary significantly across plans. Not surprisingly, spending among this group was much higher than among those who survived. In any event, the subjects who died were included in the analyses reported in Newhouse et al (1981, 1993) and in Manning et al. (1987).) The similarity between the pre-dropout use of those who left voluntarily and those who did not makes it more plausible that those who left the Experiment were similar to those who did not, although it does not rule out Nyman's argument that hospital use might have been similar prior to withdrawal but that an anticipated new hospitalization precipitated the withdrawal.

To respond to Nyman's argument we have now calculated a worst case analysis. We ask what the hospitalization rate for adults who voluntarily left the Family Pay plans, meaning all plans other than the Free Care and Individual Deductible plans, would have had to have been during the period that they would otherwise have been enrolled in the experiment to wipe out the difference in use that we observed. We focus on the Family Pay plans because the Individual Deductible plan had no inpatient cost sharing, while the Family Pay plans did. (For this purpose we define adult as age 18 or over, so the following numbers differ somewhat from those in Table 2.6 of (Newhouse and the Insurance Experiment Group 1993), which defines adult as age 14 or over. This difference does not affect conclusions about possible bias from attrition.)

At the outset of the Experiment there were 1,533 persons age 18 or over on the Family Pay plans, and the difference in voluntary attrition between those plans and the Free Care plan was 6.62 percent (6.98 percent - 0.36 percent). Thus, the excess number of people who left the Family Pay plans voluntarily was 1,533 x 0.0662, or 101.48 people. This calculation corrects Nyman's methodological error in his Table 1, where he calculates the excess number as 174, the difference in the absolute number of people leaving the Family Pay and Free Care plans, ignoring the fact that there were many more people in the Family Pay plans. In other words, even if the proportion of those leaving had been the same in the two plans, the absolute number of people leaving would be larger in the Family Pay plan.

We next calculated the per person number of hospitalizations over the entire course of participation in the study for the Free Care and Family Pay plans (i.e., total number of hospitalizations/number of enrollees), 0.6311 and 0.4710 respectively. Thus, if the Family Pay plans were to have the same observed hospitalization rate as the Free Care plan, persons enrolled in those plans would have had to have had 1,533 x (0.6311-0.4710), or 245.43 additional hospitalizations. Dividing that number by the 101.48 excess attrition cases from above and by the 2.348 additional years that each person who left voluntarily would have been in the experiment had they not left prematurely, yields a value of (245.55/101.48)/2.348, or 1.03 hospitalizations per attrition case per year. In short, to equate the hospitalization rates of the two types of plans, each person who left would not only have been hypothetically hospitalized at the time of leaving, which is Nyman's assumption, but also would have been hospitalized once in each remaining year.

Needless to say, this is an extraordinary use rate for a group of adult dropouts that on average were 34.7 years old at the beginning of the Experiment and on average remained in the Experiment for only 1.3 years. By comparison, the bottom 5 percent of the distribution of the General Health Index on the Free Care plan only had a mean hospitalization rate of 0.52 admissions per year.

Moreover, Nyman fails to consider that voluntary attrition meant the entire family had to drop out because his calculations in his hypothetical Table 1 apply at the individual level. In effect, in order to substantiate Nyman's speculation, families with multiple members would have had each adult hospitalized at the rate of 1.03 hospitalizations per year in each remaining year after their disenrollment. This scenario is even more far fetched.

Nyman's speculation that a high degree of non-random attrition importantly biased the results on utilization is also not supported in work that is unpublished but posted on Newhouse's home page. In that work from 1993 Manning, Duan, and Keeler carried out additional analyses of those who did not complete the Experiment, including those who terminated involuntarily (e.g., enlisted in the military). Their sample is thus larger than the group analyzed above who left voluntarily. They concluded that there was in fact a modest amount of non-random attrition, but that its effects, if accounted for, would have left our estimated effect of cost sharing on use essentially unchanged.

Besides this quantitative analysis, there are qualitative reasons why Nyman's speculation is implausible. At best it would apply to anticipated hospitalizations. If one were hospitalized in a medical emergency, for example with a heart attack, it would be too late to change insurance policies. And anticipated hospitalizations would likely have generated above average expenses in the period prior to hospitalization, as individuals sought care and physicians carried out diagnostic tests. We did not observe that.

Nyman speculates that cash flow problems may have precipitated the excess attrition. But these were individuals who were getting monthly checks from us with a promise of future checks that would leave them better off financially than withdrawing would. One would have thought that if there were a cash flow problem, at least some of the participants would have checked with our field office about a possible advance. We received no such requests. Or one would have thought the individual might have asked the hospital (or the hospital might have asked us) about paying the bill on an installment plan. Many hospitals would probably have made such a deal or else contacted us about assigning future monthly payments to the hospital. We received no such inquiries.

The Lack of Health Status Effects Again. Nyman asserts in the passage quoted above that hospitalizations could not have been reduced by the 23 percent that we observed without seeing adverse health effects, and since we didn't see adverse effects, it must follow that hospitalizations did not fall. But in fact age-adjusted hospital discharge rates fell 32 percent from 1980 to 2004 without any obvious adverse health consequences (Health United States, 2006, Table 96). Some of this fall was no doubt due to technological change such as outpatient surgery, but there is more direct evidence from the Experiment itself that a number of patients could have safely been cared for less intensively in 1980 without damaging their health.

First, the staff model HMO to which we randomized Experimental participants reduced admission rates 36 percent relative to the Free Care plan without measurable adverse consequences for the average person (the HMO participants also faced no cost sharing). Second, Nyman correctly cites (Siu et al. 1986) as showing that cost sharing reduced both appropriate and inappropriate utilization, but he apparently does not realize that "appropriate" in that study meant the condition of the patient required acute hospital care or that the patient received specific services that required hospitalization. Thus, "inappropriate" meant whatever services were required or rendered could have been offered in an outpatient setting. Note that this definition of inappropriate is conservative in that it accepts that what was done for the patient was clinically appropriate and only asks whether the patient needed to be in the hospital to have it done. And on that definition fully 24 percent of the hospitalizations in the free plan were inappropriate (versus 22 percent in the plans with cost sharing for all services).

So What Could Account for the Greater Attrition in the Cost-sharing plans? Up until Nyman's comment a quarter century after the Experiment ended, we thought there was sufficient evidence that the difference between the 0.4 percent voluntary attrition on the Free Care plan and the 6.7 percent attrition on the various cost-sharing plans did not affect our conclusions. The design of the participation incentive made it

financially disadvantageous to withdraw, and we could find no evidence that those who withdrew were importantly different from those who did not. Nyman, however, rejects this evidence and asserts that the difference in attrition "…is convincing evidence that the participation incentive simply did not work. To my knowledge, RAND researchers have yet to present a benign explanation for this large and statistically significant voluntary attrition rate differential."

It is important to emphasize that Nyman has no evidence that the difference in attrition is not ignorable (in the statistical meaning of the term), that is, no evidence that it biased the results. Nyman simply makes up the story that participants in the cost-sharing plans were financially motivated to withdraw because they were staring a hospitalization in the face.

But Nyman is right that up to this point we have not presented a benign explanation for this difference. That is because any explanation, benign or otherwise, must be speculative, and we saw no need to speculate given the lack of incentive and lack of evidence consistent with bias. But since Nyman thinks that the differential attrition is per se evidence that the participation incentive failed to prevent biased attrition, we present two possible benign explanations for the differential attrition. Both come from behavioral economics.

The first is that some individuals may dislike trading off health and dollars and see cost sharing as forcing them to do so. For example, some individuals simply do not want to answer a hypothetical question of what sum of money they would be prepared to accept in return for a given risk of illness or death (Loomes 2006). In the words of Jonathan Baron, health may be a protected value (Baron and Spranca 1997). Clearly the free plan did not force such a tradeoff, but some individuals on the cost-sharing plans may have wanted to avoid making this tradeoff, independent of their expected utilization. Other work in psychology suggests aversion to such a tradeoff though it does not conclusively show it (Luce et al. 1999; Luce et al. 2000). A related point from neuroeconomics is the notion that parting with money is painful, something that can be avoided on the free plan (Camerer et al. 2005). This is probably why health clubs typically do not charge per visit (della Vigna and Malmendier 2004).

A second explanation is the endowment effect, or the underweighting of opportunity costs (Rabin 1998; Thaler 1980). Many individuals go to greater lengths to avoid losses than to obtain gains (e.g., not wanting to play a lottery with a 50-50 chance of losing $1 or winning $5). If persons had better insurance prior to the Experiment, at least for some types of illness, they may have seen the cost-sharing plans as potentially imposing random losses on them, even though on expectation they did not differ in the magnitude of the expected loss from those who accepted the offer to enroll and remained in the Experiment.

Both these effects could explain not only the differentially higher attrition but also the differentially higher refusal rates of the initial offer on the cost-sharing plans. Importantly, those who refused and those who accepted the offer to enroll did not differ

with respect to prior medical care utilization or self-rated health status (Newhouse and the Insurance Experiment Group 1993), from which we infer that the differentially higher refusal also was benign.

What About the Reductions in Efficacious Use? Nyman incorrectly cites (Lohr et al. 1986) as showing that "both effective and ineffective *hospital* procedures were reduced" (emphasis added). Lohr, et al. actually examined care for many different medical conditions and procedures, the great majority of which would not have resulted in hospitalization (e.g., upper respiratory infection), although some could have (e.g., chest pain, vehicular accidents). Lohr et al. also examined procedures, but all of them were ambulatory procedures. As Nyman says, Lohr et al. did find that cost sharing was non-specific and reduced services that would likely have been medically efficacious as well as those that likely would not have been. Nyman asks if these reductions in medically efficacious care took place, why was there no observed difference in health status? His answer, as already pointed out, is that the reductions must not have taken place because of financially motivated attrition.

We think there is a different explanation. To begin with, note that many of procedures and tests that Lohr et al. examine were not very expensive, even relative to the monthly side payment, e.g., one office visit for a bacterial infection and a prescription for an antibiotic, making the financially motivated attrition Nyman assumes less plausible.

Nonetheless, the question of why we observed no health status effects despite reductions in the use of efficacious services is a good one, and we grappled with it in the concluding chapter of (Newhouse and the Insurance Experiment Group 1993). Our answer there was that the reduction in efficacious services on the cost sharing plan likely did harm some participants, but that the concomitant reduction in inefficacious services offset that by preventing some harmful care. As one example we pointed to the prescription of antibiotics for viral conditions; about half of the antibiotics prescribed in each plan were for viral conditions. These would not have helped but do have a known incidence of adverse side effects. And we pointed out that the Experiment sampled a community dwelling population under 65 years of age, most of whom were relatively healthy. Thus, the odds that medical care would have been beneficial were reduced relative to a poorer and sicker group, where we did see on balance beneficial effects of additional care.

In the fifteen years since the publication of (Newhouse and the Insurance Experiment Group 1993) much more has come out with respect to quality of care in the American medical care system, much of it supporting our inference that there is – or at least at the time of the Experiment was – a non-trivial amount of negative benefit care (Institute of Medicine 1999; Institute of Medicine 2001). Eliminating cost sharing induced additional medical contacts; within a reasonably healthy population some of these contacts likely affected health adversely.

In sum, Nyman's infers solely from a 6+ percentage point difference in cumulative attrition that the usual conclusions from the RAND Health Insurance Experiment results – that cost sharing reduced use but for the average person did not affect health outcomes – is an artifact that resulted from individuals dropping out to use services at a similar rate and having their prior insurance plan pay for them. He has no evidence for this inference other than the difference in attrition. We believe that both the design of the Experiment and the evidence that can be brought to bear – including the many other studies that find an effect of cost sharing on use – strongly support the usual conclusions.

Some Concluding Comments on Nyman. Although Nyman does not say so directly, he seems to suggest that we had a pre-conceived ideological agenda to promote cost sharing as national policy. This was certainly not the case. Those who designed the Experiment, carried it out, and analyzed the data it yielded had diverse views on the usefulness of cost sharing at the start of the Experiment, but all of us thought that the evidence base at that time was weak. Indeed, that was why we invested the effort we did in carrying out the Experiment. Moreover, the principal inferences we drew from our results do not support the notion that we had a hidden agenda. For example, (Newhouse and the Insurance Experiment Group 1993) says in its conclusion: "In fact, the Experiment provides some support for both free care and initial cost sharing," referring to the contrasting results for the average participant and the sick, poor participant.

The foregoing has been solely concerned with the second part of Nyman's paper regarding the RAND Experiment. But we point out two aspects about the first part of his paper, where he suggests that a common economic view of moral hazard is wrong. First, this part of his argument is irrelevant to whether the results from the RAND Experiment are biased or not. Indeed, the participation incentive described above essentially implements the "simple test" for moral hazard that Nyman refers to when he discusses giving the breast cancer patient a cashier's check for the cost of her treatment and allowing her decide what to do with the money.

Second, Nyman characterizes the first part of his paper on moral hazard as "new theory." But it is neither new nor even original with Nyman; it was first pointed out by (DeMeza 1983) nearly a quarter of a century ago.

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