ESTIMATES OF OWN LETHAL RISKS AND ANCHORING EFFECTS

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Abstract

The paper presents an experiment showing that some of the results in Benjamin et al. (2001) should be attributed to an anchoring effect. More precisely, it appears that, when asked to evaluate successively the number deaths for two different populations, respondents anchor their answers in the second survey, on the answers they gave in the first survey. The experimental outcomes also indicate that agents' estimates of their own lethal risks exhibit the traditional biases (i.e. agents overestimate rare risks, and underestimate common risks). However, if the quality of an estimate is measured not only by its mean, but also by its variance, then the present experiment cannot dismiss unambiguously Benjamin et al.'s hypothesis that agents have better information about their own risks.

Keywords: Anchoring, Risks Perception, Health and Safety Hazard, Rational Expectation.

JEL Classifications : D8, C9.

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1. Introduction

During the last two decades, several experimental studies, both in economics and psychology, have identified a number of systematic biases in agents' perception of uncertain events.[1] In particular, experimental subjects consistently overestimate the risks of death due to low-probability causes, while they underestimate lethal risks from high-probability causes.[2]

Benjamin and Dougan (1997), however, contest the presence of a systematic bias in agents appraisals of lethal risks. Instead, they suggest that the bias observed in population-wide surveys are consistent with rational decision making in the presence of scarce information. Indeed, according to the authors, agents may be able to evaluate accurately the risks of death for their own-age group, since they are most relevant to them, but they are typically poorly informed about the population-wide risks.

To test this hypothesis, Benjamin, Dougan and Buschena (2001) (hereafter BDB) conducted an experiment consisting in two surveys that the same subjects had to complete successively. In the first survey, subjects were asked to evaluate a series of population-wide lethal risks, while in the second, they had to estimate the number of deaths from the same lethal risks within their own-age cohort. The population-wide survey yielded the traditional biases for low and high causes of death. By contrast, the age group estimates were much more accurate. In particular, high lethal risks were correctly estimated. Subjects appraisals, however, were not perfectly unbiased since small risks were still slightly overestimated. The authors suggest that a truncation effect at zero may plausibly explain the remaining bias. BDB then conclude that the experiment essentially confirms their theory, and discredits the widely accepted consensus that agents have biased perceptions of lethal risks.

The experimental outcomes in BDB, however, may be alternatively explained by the

[1]See e.g. Kahneman, Slovic and Tversky (1982), Slovic (1987), Viscusi (1989), Viscusi (1993), or Camerer (1995).

[2]See e.g. Lichenstein et al. (1978), Granger et al. (1983), Viscusi et al. (1997), as well as Hakes and Viscusi (1997).

well known Anchor theory.[3] The anchoring effect results from insufficient adjustments from an initial, arbitrary, and sometimes completely uninformative value. For instance, the outcome of a wheel of fortune has been shown to influence subjects' evaluation of the percentage of African countries in the United Nations (see Tversky and Kahneman 1974). Anchoring has been found to be a robust psychological effect in numerous experiments. In particular, Lichtenstein et al. (1978), as well as BDB find that providing subjects with a low (high) frequency cause of death as a benchmark before they complete the survey, leads them to underestimate (overestimate) most lethal risks, compared to the case in which no benchmark is provided.

The contention in the present paper is that the outcome of BDB's experiment may be explained by the fact that subjects anchored their answers in the second survey, on the answers they gave in the first survey. If this contention is correct, then BDB's experiment does not necessarily dismiss the presence of systematic perception biases, and it does not confirm their hypothesis that agents have accurate perceptions of their own-age cohort lethal risks.

The Anchor hypothesis is plausible since subjects were first asked an evaluation for the entire population, and then for a sub-population (their own-age cohort). If the responses in the population-wide survey act as anchors, then subjects are likely to overestimate the lethal risks for the sub-population. Anchor theory, therefore predicts that the overestimation of rare causes of death observed in the population-wide survey should still be present, and potentially amplified in the per-cohort survey. In addition, the traditional underestimation of common lethal risks should be attenuated, and may possibly vanish in the per-cohort survey. This is precisely the outcome observed in BDB's experiment.

To test between the presence of an anchoring effect, and BDB's hypothesis that agents' perception of their own risks is unbiased, an experiment consisting in five treatments is conducted. The first treatment (the control treatment) is identical to BDB's.

[3] See Tversky and Kahneman (1974), Camerer (1995), as well as Chapman and Johnson (2000) for surveys.

In the second treatment, the order of the surveys is reversed (i.e. the per-cohort survey is completed prior to the population-wide survey). Treatment 3 is similar to BDB, except that subjects are asked to evaluate the number of deaths for an age-group different than theirs. Finally, subjects are asked in treatment 4 (respectively, 5) to evaluate only the causes of death for their own-age-cohort (respectively, for a different age-cohort).

If BDB's hypothesis is correct, then there should not be any treatment effect. In other words, the own-age cohort perceptions in treatments 2 and 4 should remain unchanged, and therefore be similar to the control treatment. In addition, the bias observed in the population-wide surveys should be comparable in the first two treatments. Finally, the estimates of the number of deaths in a different age-group in treatments 3 and 5 should exhibit the traditional biases. By contrast, if subjects anchor on the answers provided in the first survey completed, then the perception biases for their own-age cohort risks should be more pronounced in treatment 2 and 4 than in the control treatment. On the other hand, the answers in treatment 3 for the different age-group should be roughly as accurate as in the own-age cohort estimates in the reference treatment.

Following precisely BDB's methodology to conduct the experiment and analyze the data, I find i) a strong anchoring effect, ii) the systematic biases for both population-wide and own-age cohort surveys, and iii) some evidence suggesting that subjects may be better informed about their own lethal risks.

The remainder of the paper is structured as follow: the experimental design is presented in section 2; the experimental outcomes are discussed in section 3; finally, section 4 concludes

2. The Experimental Design

The experiment was conducted with volunteer undergraduate students at the State University of New York at Stony Brook. Five experimental sessions, one for each treatment, and each with 35 subjects, were scheduled on different days during the winter 2001. All subjects within the same treatment were asked to meet in a large room, where they had to complete independently the survey(s). No time limit was imposed, but most surveys were completed within 20 minutes. Note that subjects were not allowed to participate in more than one session.

The reference treatment is essentially similar to BDB's experiment.[4] Subjects were first asked to evaluate the total number of deaths in 1999 in the U.S due to 40 causes listed in Table 1. Once this survey was completed and returned to the experimenter, subjects were then asked to evaluate the number of deaths in their own-age group from the same lethal risks.[5] Four main differences with BDB's experiment should be noted:

- First, the lethal risks in the present survey are the same as in BDB, except for three small risks, "Poisoning by Solid or Liquid", "Poisoning by Vitamins" and "Small Pox Vaccination" for which no data were available. These have been replaced by "Cholera", "Sail Boat Accidents", and "Accidental Poisoning due to Pesticides" which produce approximately the same number of deaths as the lethal causes used in BDB.

- Second, each subject in BDB's experiment received a fixed payment of $10. In the present experiment, subjects within the same treatment were ranked by order of accuracy for each cause of death. The rankings were then aggregated for each participant across all causes of deaths. The subject with the best aggregated ranking within his own treatment received $50. The others did not receive any payment. This approach offers two potential advantages: first, it is relatively inexpensive to conduct an experiment with several treatments and numerous participants; second, it is believed to provide subjects with a better incentive to accurately answer the survey than a guaranteed payment.[6]

[4] The instructions given to the experimental subjects are similar to the one found in BDB and, therefore, they are not reported here.

[5] Actual 1999 population-wide and per-age cohort figures may be found on the National Center for Health Statistics web-site at http://www.cdc.gov/nchs/datawh/statab/unpubd/mortabs/gmwki10.htm.

[6] The consequences of paying only a randomly selected subset of players are discussed in Bolle (1990). See also Camerer (1995), and Camerer and Hogarth (1999) for an analysis of the effects of incentives on experimental outcomes.

- Finally, half of the subjects in BDB's experiment were provided with a high anchor (the number of deaths due to motor vehicle accidents) and the other half with a small anchor (the number of deaths due to electrocution). The object of the present study is not to analyze this form of anchoring. Therefore, all subjects are only provided with the high anchor.

As we shall see, these design modifications do not appear to generate any significant treatment effect in the subsequent experiments.

The other four treatments are essentially similar to the reference treatment. However, the order in which the surveys are completed is switched in treatment 2 (i.e. the per-age cohort survey is completed first, followed by the population-wide survey). After completing the population-wide survey, subjects are asked, in treatment 3, to evaluate the number of deaths from the same lethal risks for a different age-group (i.e. for people between 40 and 44 years of age). Finally, subjects are only asked in treatment 4 (5) to complete the survey for their own (the 40-44) age-cohort.

3. Experimental Results

The distribution of subjects per-age group in each treatment is represented in Table 2. Like BDB, the wide majority (90.4%) of participants belong to one of the 15-19 or 19-24 age brackets. The average age of the respondents, however, is slightly lower (20.7) than the approximated age of the students in BDB's study (21.4), but it is well within one standard deviation.

Before describing the experimental outcomes, it is important to note that I have strived to precisely follow the same methodology as BDB. In particular, the analysis of the data is based upon two econometrics models: a Log-Log specification, and a median regression model. The Log-Log specification has been previously used by Lichenstein (1978) to mitigate the effects of heteroscedasticity, and reduce the influence of the numerous outliers present in the data. The median regression model has been specifically developed to address these estimation problems by minimizing the sum of the absolute value (rather than sum of the square) of the residuals (see e.g. Buchinsky 1994). I will therefore concentrate essentially on the median regressions when presenting the results, since, as in BDB, they tend to better describe the data than the Log-Log specifications.

Following BDB, the endogenous variable Y_i ($i = 1, ..., 40$) in each of the following regressions is defined as the geometric mean of individual responses in a given survey for the *ith* lethal cause. The exogenous variable X_i ($i = 1, ..., 40$) in the population-wide regressions is represented by the actual total number of deaths in the US in 1999 for the corresponding lethal risk. Since respondents were from different age groups, the exogenous variable in the per-cohort regressions is defined as the geometric mean of the per-age group actual number of deaths in the US in 1999. Following BDB, these geometric means were calculated with weights equal to the number of subjects in each age group.[7]

Results of the median and Log-Log regressions are presented in Tables 3 and 4. Note that the number of observations (N) is lower than the number of risks to be evaluated in the surveys (i.e. 40), and it also differs slightly across columns. Indeed, following BDB, the lethal risks which did not generate any death in 1999 have been ignored for the population-wide analysis. Likewise, the lethal risks which did not generate any death in 1999 for one of the respondent's age-group have been ignored in the per-age cohort analysis.

- *Treatment 1*

Let us first concentrate on the median regressions in the reference treatment. The objective here is to verify whether the slight design modifications have introduced a treatment effect compared to the results obtained by BDB.

If the respondents have an unbiased perception of the lethal risks, then the constant and the slope in the simple regression $Y_i = \alpha_0 + \alpha_1 X_i + \varepsilon_i$ should be respectively equal

[7] Log-Log and median regressions have been run with individual responses, arithmetic means, and linear regressions. These alternative specifications generate results that, although less accurate, do not alter the nature of the conclusions.

to 0 and 1. Table 3 shows that subjects' perception of the population-wide lethal risks are clearly biased in treatment 1, since the slope is close to 0, while the constant is very large. In sharp contrast, the slope in the own-age-cohort regression is close to, but significantly different than 1.[8] In addition, the constant term is much smaller, and it is not significantly different than 0. In other words, subjects appear to have an almost unbiased perception of the lethal risks in their own-age-cohort.

This observation is partially confirmed in Graphs 1 and 2 where the observations and the regression lines are plotted.[9] These graphs indicate that subjects' estimates are much more precise for their own-age-cohort, especially for frequent causes of deaths, since the estimated values bracket the diagonal. Note, however, that respondents still appear to over-estimate small causes of deaths, just like in BDB's experiment.

To summarize, although the estimated parameters are slightly (but not significantly) different, the results in the reference treatment are qualitatively similar to BDB's. As previously explained, the experimental outcome is consistent with both anchor theory, and the hypothesis that agents perceptions of their own risks are unbiased. To distinguish between these two hypothesis, let us now turn to treatment 2.

- *Treatment 2*

Table 3 indicates that switching the order in which the surveys were completed has a significant impact on subjects' estimates. Indeed, although the constants cannot be statistically distinguished in the population-wide regressions, the slope is slightly, but significantly, smaller in treatment 2 than in the reference treatment. In addition, Graph 3 suggests that most lethal risks, and more specifically frequent lethal risks, are slightly underestimated compared to graph 1.

However, the most striking difference between the first two treatments, may be found in the per-age cohort regressions. Indeed, the slope is much smaller in treatment 2 (0.222)

[8] All statistical tests in the paper are conducted with a 5% significance level.

[9] The regression lines appear to be non-linear since a logarithmic scale is used.

compared to the reference treatment (0.906). Consequently, as illustrated in graph 4, subjects own-age-cohort estimates of lethal risks are now clearly biased.

In other words, switching the order of the surveys yields the traditional biases both for the population-wide, and the own-age cohort estimates. In addition, the treatment effect appears to slightly increase the bias for frequent causes of deaths in the population-wide survey.

This result is clearly at odds with the idea that agents have a better knowledge of their own risks. However, it is perfectly consistent with the hypothesis that subjects anchor their responses in the second survey on the responses given in the first survey. Indeed, subjects' perceptions of lethal risks for a sub-population (their own-age cohort) exhibit the traditional systematic biases in the first survey. In addition, these low anchors may have lead subjects to lower their population-wide estimates for the same lethal risks in the second survey. As a result, population-wide perceptions in treatment 2 appear relatively less (slightly more) imprecise for infrequent (frequent) causes of death.

- *Treatment 3*

Let us now examine the experimental results in treatment 3, in which subjects had to evaluate risks for the entire population, and then for a different age group. The regression results presented in Table 3 cannot be statistically distinguished from the one found in the reference treatment.

Graphs 5 and 6 also indicate that, although the estimates of each cause of death are often more volatile than in Graphs 1 and 2, the regression lines are essentially similar. In other words, the outcome in treatment 3 would suggest that respondents are as accurate to evaluate the lethal risks for a different age-cohort, as they are for their own cohort in the reference treatment.

This result again appears to contradict the BDB's hypothesis of rational risk assessment under scarce information. On the other hand, the experimental outcomes remain perfectly compatible with anchor theory, as subjects appear to anchor on the answers given in the first survey.

- *Treatments 4 and 5*

Finally, let us compare the regressions results in treatments 4 and 5, in which two different sets of subjects only had to evaluate risks for either their own, or a different age group. Both regressions in Table 3 yield relatively high constants, and slopes near 0. Note, however, that the slope coefficient is slightly, but significantly, larger in treatment 4. In other words, perceptions exhibit the traditional biases in both treatments, but subjects estimates may be slightly more accurate for their own-age cohort. This observation finds additional support in Graphs 7 and 8, since subjects responses are much less volatile, and slightly closer to the diagonal for the own-age group survey.

In other words, if better knowledge is measured not only by the bias, but also the standard deviation of the estimates, then the present experiment does not find any conclusive evidence to reject the BDB's hypothesis that subjects have better knowledge about their own lethal risks.

Finally, note that the results in treatment 4 and 5 are still consistent with anchor theory. Indeed subjects' perceptions of their own (respectively, other age-group) risks are less accurate than in treatment 1 (respectively, 3) in which respondents first had to complete the population-wide survey. On the other hand, the own-age regressions in treatments 2 and 4 are statistically similar, since subjects in treatment 2 also had to first complete the own-age cohort survey.

4. Conclusion

The presence of systematic biases in the perception of lethal risks has generated numerous debates about the salience of the results, and the origin of the phenomenon. In particular, Benjamin and Dougan (1997) contest the existence of systematic biases, and suggest that the biases observed in population-wide surveys may be explained by the fact that agents are only aware of their own risks, since these are most relevant to them. Benjamin, Dougan, and Buschena (2001) (BDB hereafter) conducted an experiment that appeared to confirm this hypothesis.

The present paper however, demonstrates that the experimental outcomes observed by BDB may be explained by anchor theory, and that agents have biased appraisals of their own lethal risks. More precisely, it appears that, when asked to evaluate successively the number deaths for two different populations, respondents anchor their answers in the second survey on the answers they gave in the first survey. As a result, subjects gave the illusion of unbiasness for their own-age cohort in BDB's study, as they were first asked about population-wide estimates. However, the different experimental treatments conducted in the present paper indicate that subjects exhibit the traditional systematic biases for population-wide, as well as their own-age cohort estimates.

In conclusion, it appears that the salience and the robustness of systematic biases in the evaluation of lethal risks cannot be rejected. However, the BDB's hypothesis that agents have better information about their own risks cannot be unambiguously dismissed either. Indeed, if the quality of an estimate is measured not only by its mean, but also by its variance, then the present experiments tend to confirm BDB's view. Indeed, we have seen that appraisals for other age-groups exhibit slightly larger biases, and they are also much more volatile than the own-age-cohort estimates. This result is also consistent with Viscusi and O'Connor (1984), and Gerking, et al. (1988) who find that workers have slightly more accurate evaluations risks related to their own job.

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Table 1							
Cause of Death In the US in 1999	**Total**	**Age Groups**					
		15-19	20-24	25-29	30-34	35-39	40-44
Small Pox	0	0	0	0	0	0	0
Cholera*	1	0	0	0	0	0	0
Measles	2	0	0	0	0	0	0
Botulism	4	0	0	0	0	0	0
Sail Boat Accident*	6	0	1	0	0	0	0
Fireworks	7	1	0	1	1	0	0
Accidental Poisoning due to pesticides*	12	1	1	0	0	2	0
Flood	15	0	2	3	0	3	1
Contact with Venomous Animals	17	0	1	0	2	0	4
Syphilis	33	0	0	1	0	1	0
Lightning	64	4	6	4	10	10	7
Cataclysmic Storm	129	2	4	7	4	11	17
Nonvenomous Animals	148	1	3	4	4	4	7
Collision between Car and Train (car Occupants)	160	22	23	11	11	9	12
Polio (Sequelae)	263	0	0	0	0	0	0
Appendicitis	331	2	3	2	1	12	10
Pregnancy and Childbirth	406	46	51	99	92	51	60
Electrocution	437	32	45	46	41	59	60
Exposure to Excessive Cold	598	5	7	8	9	24	32
Firearms Accidents	824	111	140	77	66	62	74
Tuberculosis	1,116	1	10	6	17	28	36
Smoke, Fire and Flames	3,348	97	104	136	146	175	246
Accidental Drowning	3,529	289	358	231	215	225	250
Asthma	4,172	78	72	90	110	168	201
Hepatitis	4,853	3	11	19	69	204	518
Stomach Cancer	12,711	5	8	47	89	189	316
Accidental Falls	13,162	79	163	152	191	287	358
Homicide	16,889	1,889	3,109	2,431	1,800	1,534	1,672
Emphysema	17,787	0	1	1	8	19	78
Leukemia	21,014	200	282	234	266	377	422
Suicide	29,199	895	3,006	2,841	2,265	2,641	3,825
Breast Cancer	41,528	3	10	86	353	986	1,755
Motor Vehicle Accidents	42,201	4,256	5,872	3,621	3,157	3,473	3,265
Diabetes	59,873	37	78	118	303	535	945
All Accident	97,860	5,265	8,391	6,421	5,469	8,103	7,128
Lung and Bronchus Cancer	152,063	6	14	43	135	668	2,054
Stroke	199,450	25	35	101	333	935	2,314
All Cancer	549,838	452	560	831	893	1,942	2,063
Hearth Diseases	725,192	453	616	1,245	1,821	5,388	8,212
All Disease	2,293,539	8,513	8,487	11,282	17,894	28,701	45,324

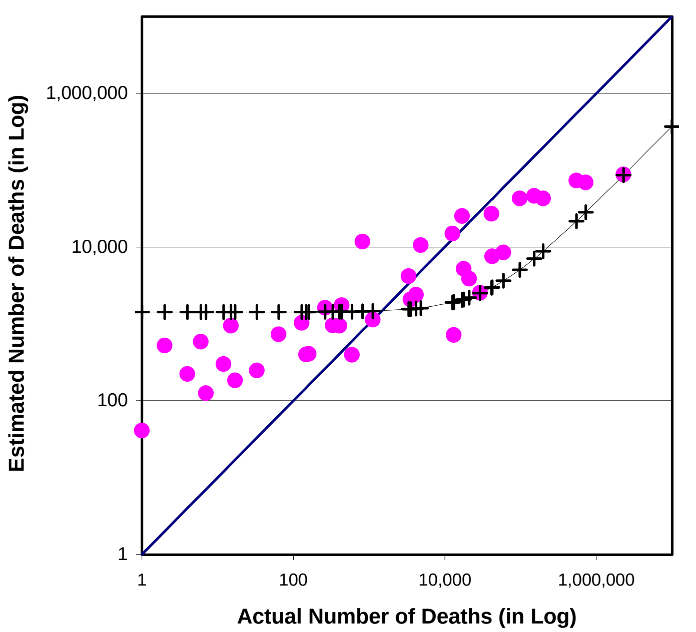
* indicates a cause of death different from the ones used by Benjamin et al (2001).

Table 2 : Age Distribution of Subjects						
Age Group	Treatment 1	Treatment 2	Treatment 3	Treatment 4	Treatment 5	Total
15-19	14	18	16	18	15	48
20-24	17	15	15	16	15	47
25-29	3	2	2	1	3	7
30-34	1	0	2	0	2	3
Total	35	35	35	35	35	105

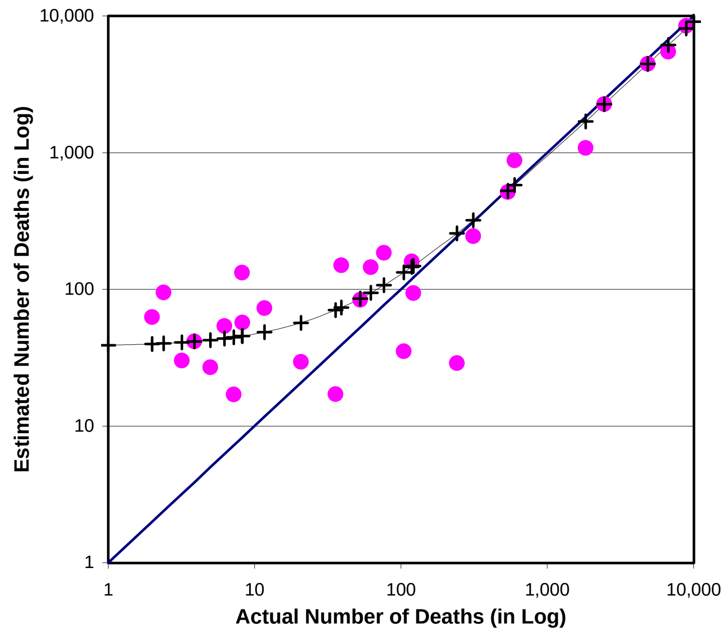
Table 3 : Median Regression Estimation Results								
	Treatment 1		Treatment 2		Treatment 3		Treatment 4	Treatment 5
	Population-Wide	Own Age Cohort	Population-Wide	Own Age Cohort	Population-Wide	40-44 Age cohort	Own Age Cohort	40-44 Age cohort
Constant	1435.88 (413.23)	38.02 (28.10)	943.23 (341.77)	21.42 (13.96)	1516.95 (487.03)	26.44 (40.01)	18.36 (11.40)	64.17 (39.73)
Slope	0.038 (0.002)	0.906 (0.017)	0.025 (0.002)	0.222 (0.013)	0.034 (0.001)	0.945 (0.032)	0.226 (0.011)	0.095 (0.030)
R^2	0.563	0.756	0.494	0.643	0.521	0.582	0.662	0.516
N	39	28	39	28	39	31	28	31

Table 4 : Log-Log Regression Estimation Results								
	Treatment 1		Treatment 2		Treatment 3		Treatment 4	Treatment 5
	Population-Wide	Own Age Cohort	Population-Wide	Own Age Cohort	Population-Wide	40-44 Age cohort	Own Age Cohort	40-44 Age cohort
Constant	4.459 (0.299)	2.508 (0.356)	4.052 (0.308)	2.339 (0.306)	4.629 (0.310)	2.450 (0.410)	2.044 (0.327)	2.142 (0.438)
Slope	0.455 (0.038)	0.594 (0.072)	0.455 (0.038)	0.467 (0.063)	0.423 (0.038)	0.620 (0.068)	0.470 (0.065)	0.508 (0.072)
R^2	0.813	0.721	0.797	0.678	0.771	0.749	0.647	0.624
N	39	28	39	28	39	31	28	31

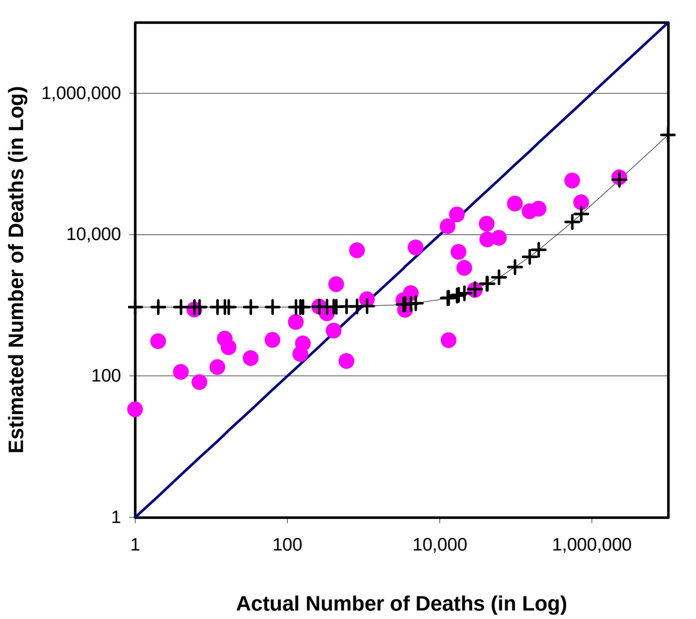
GRAPH 1
Treatment 1 : Actual and Predicted Total Number of Deaths In the US



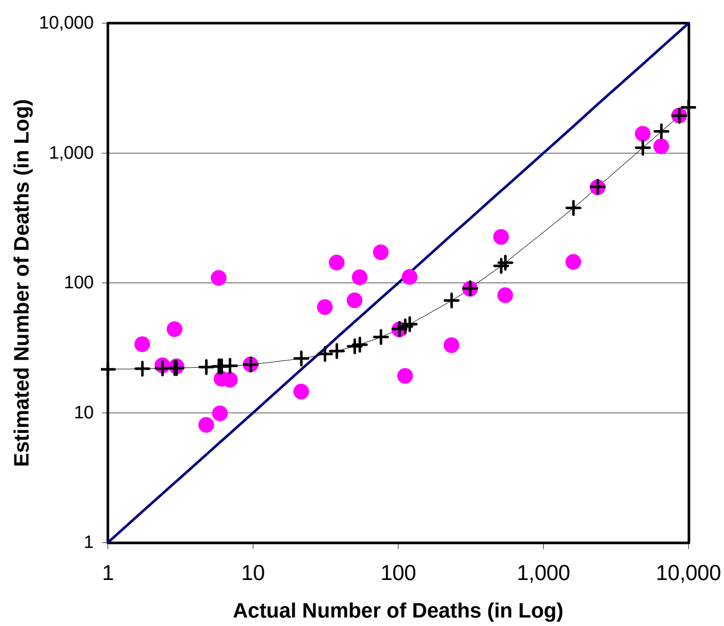
GRAPH 2
Treatment 1 : Actual and Predicted Number of Deaths for Own Age-Cohort

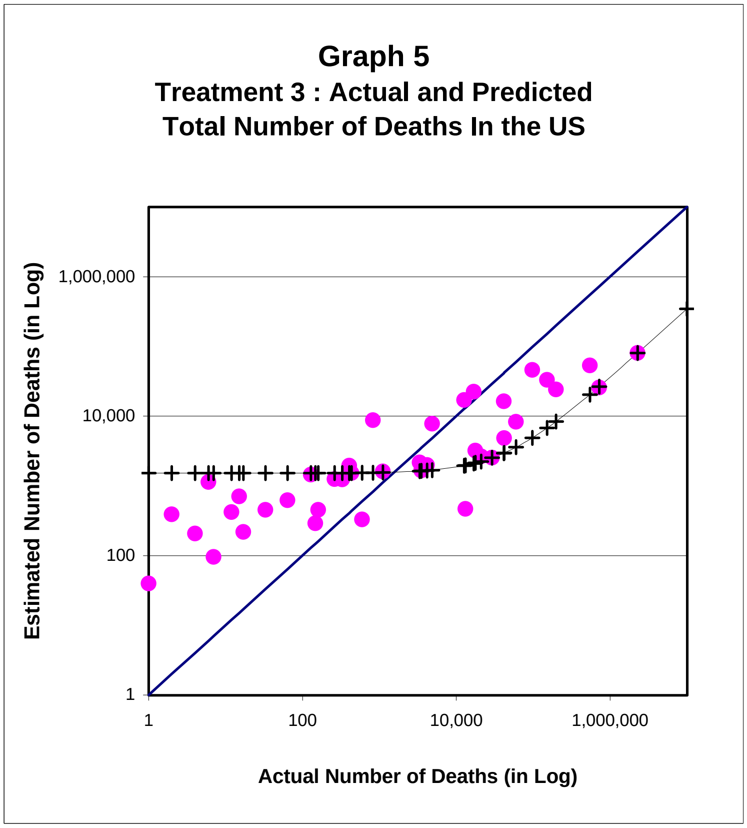


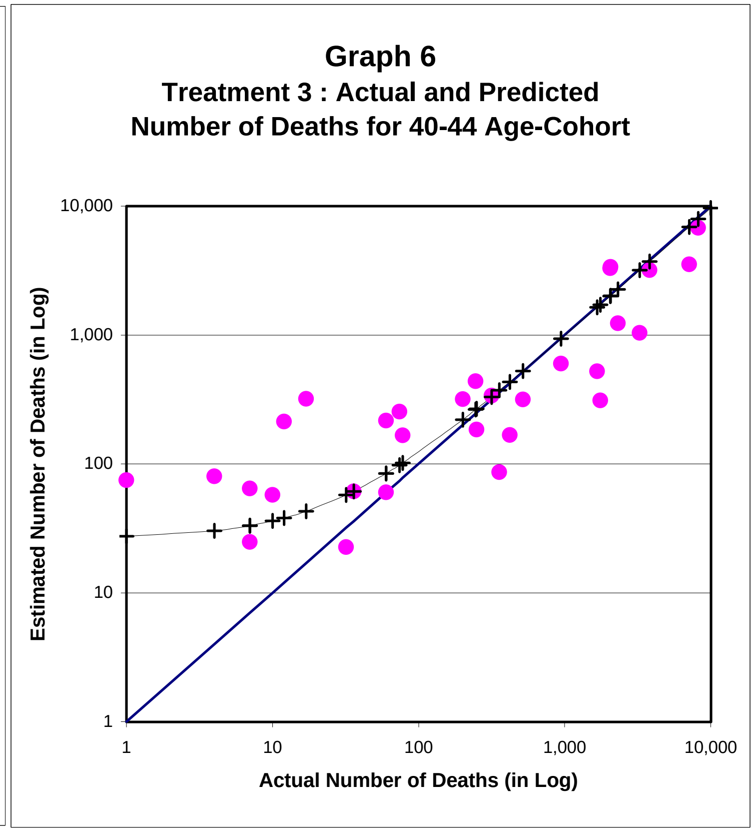
GRAPH 3
Treatment 2 : Actual and Predicted Total Number of Deaths In the US

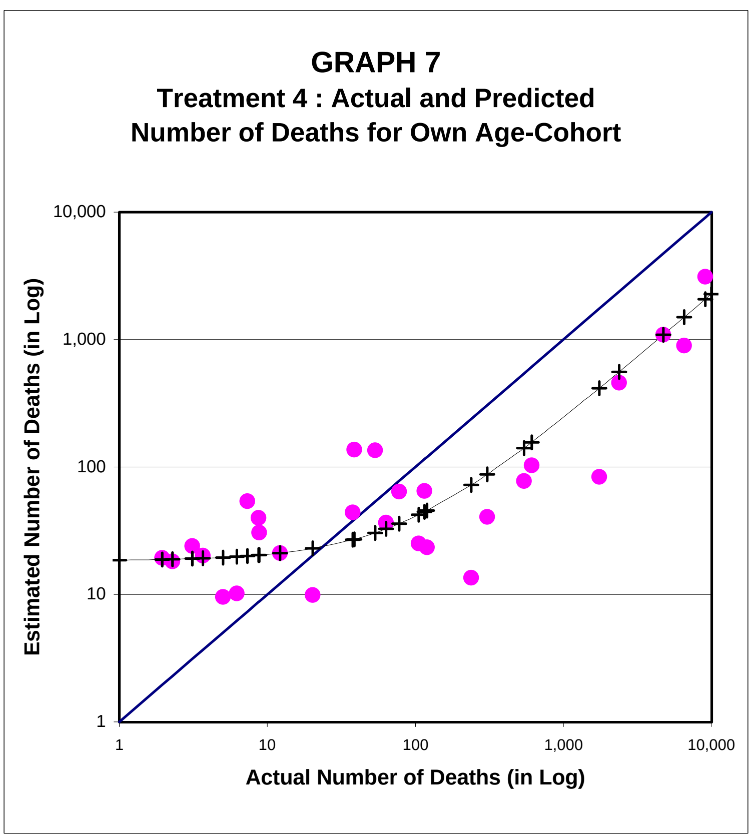


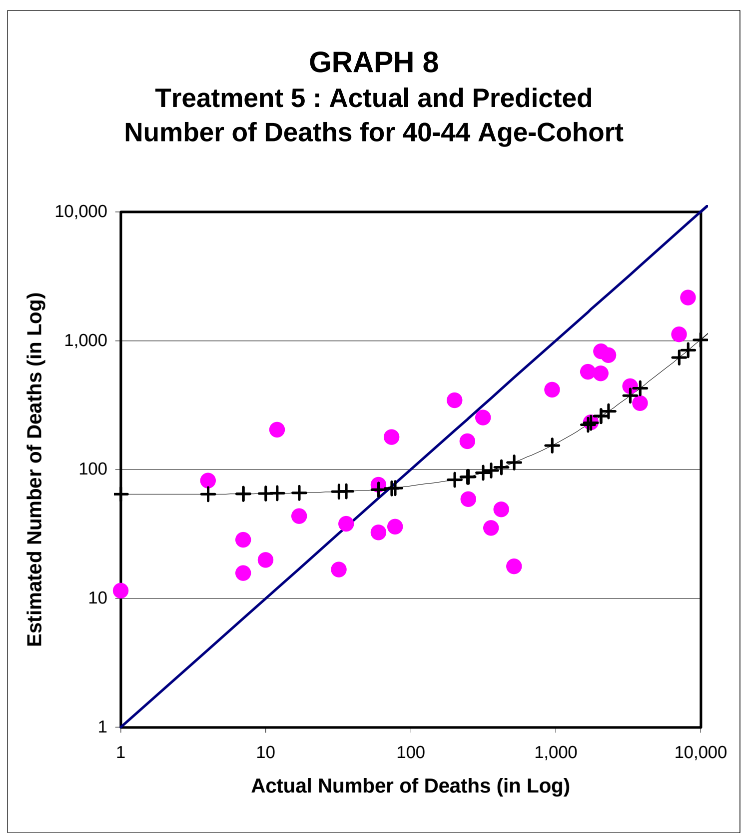
GRAPH 4
Treatment 2 : Actual and Predicted Number of Deaths for Own Age-Cohort




Graph 5
Treatment 3 : Actual and Predicted
Total Number of Deaths In the US
Estimated Number of Deaths (in Log)
1,000,000
10,000
100
1
1
100
10,000
1,000,000
Actual Number of Deaths (in Log)


Graph 6
Treatment 3 : Actual and Predicted
Number of Deaths for 40-44 Age-Cohort
Estimated Number of Deaths (in Log)
10,000
1,000
100
10
1
1
10
100
1,000
10,000
Actual Number of Deaths (in Log)


GRAPH 7
Treatment 4 : Actual and Predicted
Number of Deaths for Own Age-Cohort
Estimated Number of Deaths (in Log)
10,000
1,000
100
10
1
1
10
100
1,000
10,000
Actual Number of Deaths (in Log)


GRAPH 8
Treatment 5 : Actual and Predicted
Number of Deaths for 40-44 Age-Cohort
Estimated Number of Deaths (in Log)
10,000
1,000
100
10
1
1
10
100
1,000
10,000
Actual Number of Deaths (in Log)