

## **APPENDIX G-2.d**

**Evaluations of Three Studies Submitted to the  
National Bankruptcy Review Commission  
(Prepared by Kim Kowalewski, Congressional  
Budget Office)**

EVALUATIONS OF THREE STUDIES SUBMITTED TO  
THE NATIONAL BANKRUPTCY REVIEW COMMISSION

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This memo responds to the request of Mr. Brady Williamson, chairman, National Bankruptcy Review Commission, for my evaluations of the technical aspects of the following three studies submitted to the Commission: 1) the national model of personal bankruptcy filings described in the July 1996 report by Visa U.S.A. Inc. entitled *Consumer Bankruptcy: Causes and Implications*; 2) "Repayment Capacity of Consumers Who Seek Bankruptcy Relief," by Dr. Michael Staten of the Credit Research Center, based on a study funded by the Visa and MasterCard companies; and 3) "Credit Card Defaults and Credit Card Profits" by Professor Lawrence M. Ausubel of the University of Maryland, draft dated February 28, 1997.

THE NATIONAL MODEL OF PERSONAL BANKRUPTCY FILINGS  
REPORTED IN *CONSUMER BANKRUPTCY: CAUSES AND IMPLICATIONS*

The Visa report claims that the model "identifies the major determinants of bankruptcy filings" and that the results of the model are robust, which generally means that the results are relatively invariant to minor changes in the model's assumptions. The results of my analysis suggest the opposite—the model is misspecified and hence is an unreliable explanation of personal bankruptcy filings.

The problems of the model arise from the unsound method used to choose the explanatory variables. This method has been roundly condemned in the economics profession because it increases the chances of choosing the wrong set of explanatory variables and leads to an overstatement of the statistical significance of the results. I demonstrate the deficiencies of the model by reestimating it with a more appropriate assumption about the nature of the model's residuals—the reestimated model has markedly different coefficients than the published model. I also found the report's claim about the importance of "social factors" that Visa believes affect personal bankruptcy filings to be unfounded. The variable used to proxy for these social factors (USTREND) was constructed to follow the trends in the rate of personal bankruptcy filings, and bears no necessary relationship to any of the social factors that Visa cites.

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\* The analysis and conclusions presented in this memo are those of the author and do not necessarily represent the position of the Congressional Budget Office.

To prepare this evaluation, I reviewed the procedure used to select the explanatory variables, examined the eight variables used to estimate the published version of the model, and reestimated the model. I obtained the data from Mr. Bob Holden of the Business Research and Reporting Department, Visa U.S.A. I used these data with two alterations. First, I needed to add one observation to the data set—the value of the installment debt service variable in the fourth quarter of 1979—so that I could reestimate the model with the reported sample period. Second, I recomputed the employment growth variable (with more recent data) because the variable in the Visa data set was incorrect. To reestimate the model, I used the WEFA Group’s computer software AREMOS/32, Large Version 2.10. I also spoke with Mr. Mark Lauritano of the WEFA Group, the company that provided technical expertise and resource support to Visa, in regard to several questions I had about the data and the model.

### The Method of Choosing the Explanatory Variables

The report conveys the impression that the model is a good representation of the behavior of national personal bankruptcy filings because the researchers considered a large number of possible explanatory variables and only used those with the greatest statistical significance. Page 9 of the report indicates that “approximately 65 variables were examined to explain trends in personal bankruptcy filings,” and when “lag” effects were considered, the number of variables examined rose to “more than 100.” Most of these variables were dropped from the analysis “because they were either not statistically significant, were redundant, or were implicit in other drivers that were more significant.” The report further states that the national model “explains more than 99 percent of the variation in personal bankruptcy filing trends.”

Instead of honing in on the best model, the method of choosing the explanatory variables only increases the probability that the wrong variables were chosen. This method of repeatedly trying various combinations of explanatory variables until achieving satisfactory statistical “significance” is known as “data mining.” Because data mining only looks for correlations, not causal relationships, it very often finds spurious correlations among the data. Kmenta notes that “Data mining is definitely unscientific and the results could be seriously misleading.”<sup>1</sup>

Even if the correct variables happened to be chosen, Lovell shows that one nevertheless must be extremely conservative when judging the statistical significance

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1. Jan Kmenta, *Elements of Econometrics, 2<sup>nd</sup> edition* (New York: Macmillan Publishing Company, 1986), p. 599.

of the results of a data mining exercise.<sup>2</sup> The actual statistical significance of the results of a data mining exercise will be less than that reported by computer regression software because the software does not adjust for the number of previous regressions run on the same set of data. The greater the number of regressions estimated on the same body of data, the lower the actual statistical significance. Given that the researchers repeatedly estimated regressions by searching among “more than 100” candidate explanatory variables, Lovell’s work suggests that Visa’s national model actually has little statistical significance. In other words, the probability is very high that at least some of the explanatory variables as they appear in the model are not determinants of personal bankruptcy filings.

One spurious variable is likely to be the median price of existing homes lagged two quarters. The model says that a drop in the median price of existing homes increases personal bankruptcy filings exactly and only two quarters later. It is not clear why this variable should be an important determinant of the number of personal bankruptcy filings. A large fraction of personal bankrupts do not own homes, so a change in the median price of existing homes should not affect their bankruptcy decision. It is also puzzling why only the price from two quarters ago affects the bankruptcy decision, and not from one or three or more quarters ago. Finally, no explanation is given for why the house price is important but not principal and interest payments on home mortgage debt (the debt service variable used in the model excludes home mortgages). This peculiar choice of variables is usually a good indication that data mining has uncovered a spurious correlation.

### Reestimating the Model

My reestimation revealed a serious lack of robustness in the model, contrary to the report’s claims. The model is estimated without a correction for serial correlation in the residuals. If the model were correctly specified, the correction would have little effect on the estimated coefficients. When I make the correction, however, the results of the model change dramatically—the coefficients on the debt service variables increase while those on the other variables decrease, and the coefficient on the population variable reverses sign and would not longer be a part of the model using Visa’s method for choosing the explanatory variables. These results confirm that the data mining method has produced an invalid model.

The table on the next page shows the results of my reestimations. The column titled “As Published” shows the results as published on pages 25 and 26 of the report. The column titled “Reestimate” shows the results I obtained by reestimating their model with their data. Comparing the two columns shows that my results are very

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2. Michael C. Lovell, “Data Mining,” *Review of Economics and Statistics*, 65 (February, 1983), pp. 1-12.

Results of Reestimating the National Model				
Variable	As Published	Reestimate	Correcting the Employment Term	Correcting for Serial Correlation
USTREND	0.453 (10.670)	0.460 (10.686)	0.446 (10.053)	0.514 (5.485)
Median existing home price, lag 2	-0.690 (2.035)	-0.726 (2.118)	-0.720 (2.085)	-0.618 (3.189)
Share of population aged 25 to 44	1.907 (5.238)	1.882 (5.120)	1.857 (5.038)	-0.590 (0.427)
Installment debt service, lag 2	0.172 (3.164)	0.166 (2.971)	0.170 (3.027)	0.389 (2.722)
Installment debt service, lag 3	0.115 (3.164)	0.110 (2.971)	0.113 (3.027)	0.259 (2.722)
Installment debt service, lag 4	0.057 (3.164)	0.055 (2.971)	0.057 (3.027)	0.130 (2.722)
Number of bank card accounts per adult, lag 2	0.251 (6.744)	0.242 (6.511)	0.261 (6.753)	0.209 (2.867)
Number of bank card accounts per adult, lag 3	0.125 (6.744)	0.121 (6.511)	0.131 (6.753)	0.104 (2.867)
Employment growth, lag 1	-3.492 (9.501)	-3.494 (9.387)	-3.511 (9.298)	-2.143 (3.140)
Employment growth, lag 2	-1.746 (9.501)	-1.747 (9.387)	-1.755 (9.298)	-1.071 (3.140)
Divorce rate, lag 1	1.585 (5.668)	1.586 (5.494)	1.559 (5.378)	0.912 (2.575)
Constant	-17.068	-15.459	-1.092	-3.363
R Bar Squared	0.993	0.993	0.993	0.996
Sum of squares	0.060	0.062	0.062	0.032
Durbin-Watson statistic	0.828	0.748	0.732	1.662
Mean of LHS	-0.319	1.067	1.067	1.075
Serial correlation coefficient	n.c.	n.c.	n.c.	0.761 (8.003)
Sample period	1980Q4 to 1996Q1	1980Q4 to 1996Q1	1980Q4 to 1996Q1	1981Q1 to 1996Q1
Notes:	t-statistics are shown in parentheses. n.c. means not computed. The model corrected for serial correlation uses the correct employment growth variable and was estimated using the Hildreth-Lu method.			

close but not exactly the same as the published results. The differences are not important, though it is curious that they do not match. The table also indicates a typographic error in the reporting of the mean of the dependent variable (Mean of the LHS) as published in the report.

The column titled “Correcting the Employment Term” shows my results from estimating the model using the correct employment growth term. As supplied to me by Visa, the variable did not correspond to the definition given in the report. For unknown reasons, the variable was scaled by a factor of 1/16. Comparing this column to the “As Published” column shows that the error in the employment variable only had a large effect on the constant term. The other coefficients change slightly because I used a more recent version of the employment data.

A serious problem with the model is indicated by its low Durbin-Watson statistic. This statistic tests for the presence of serially correlated residuals—in particular, whether the residual (or unexplained part of the model) in one time period is correlated with the residual in the previous time period. If a model is estimated correctly, a residual will not be correlated with another residual in any previous time period, so that if the residuals are plotted against time, they will show no obvious pattern. But it is clear from the bar chart in Figure 3 on page 9 of the report that the errors show a regular up-and-down pattern like a sine wave. The published value of the Durbin-Watson statistic, 0.828, indicates that the residuals are serially correlated with high probability, contrary to the claim on page 28 of the report that it does not.<sup>3</sup>

When the model is estimated with a correction for serially correlated residuals, the results change dramatically, indicating that the model is defective and not robust. The results of this estimation are shown in the column titled “Correcting for Serial Correlation.” The serial correlation coefficient is statistically significant, meaning that the correction is important. Two striking changes stand out. First, the coefficient on the share of the population aged 25 to 44 changes sign, from positive to negative, and becomes statistically insignificant. Second, the importance of the debt service variable increases—the coefficients on the installment debt service variables double in size—while those on all of the other variables except the meaningless USTREND variable become smaller.

While my reestimation points out a serious flaw in the model, it does not correct the model. The population variable has the wrong sign, and the Durbin-Watson statistic may still indicate the presence of serial correlation in the residuals. To

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3. The hypothesis of no serial correlation can be rejected with a 5% significance level for a model with eight explanatory variables and 60 observations if the value of the Durbin-Watson statistic is less than a critical value of 1.335. With 65 observations, the critical value is 1.370. These critical values come from N. E. Savin and K. J. White, “The Durbin-Watson Test for Serial Correlation with Extreme Sample Sizes or Many Regressors,” *Econometrica*, 45 (November, 1977), pp. 1989-1996.

accept the hypothesis at a 5% significance level that the residuals are not positively serially correlated, the Durbin-Watson statistic would have to be between 1.85 and 2. More work is needed to correct the model.

### The Misleading Social Trends Variable

For the past twenty years, some observers have claimed that such factors as an erosion of the social stigma of bankruptcy, an increase in advertising by lawyers, an increased awareness of consumers' rights, and the overhaul of the bankruptcy laws specified by the Bankruptcy Reform Act of 1978 are important factors behind the increase in personal bankruptcy filings during this period. These claims are difficult to evaluate, however, because these factors are not observed. Indeed, Visa states on page 13 of the report that:

Many social factors are not easily quantifiable, as consistent, historical data do not exist. Such factors include changes in the bankruptcy laws, the reduced stigma associated with filing for personal bankruptcy and broader advertising of legal assistance with bankruptcy filings.

Yet, on the basis of their analysis, Visa concludes that:

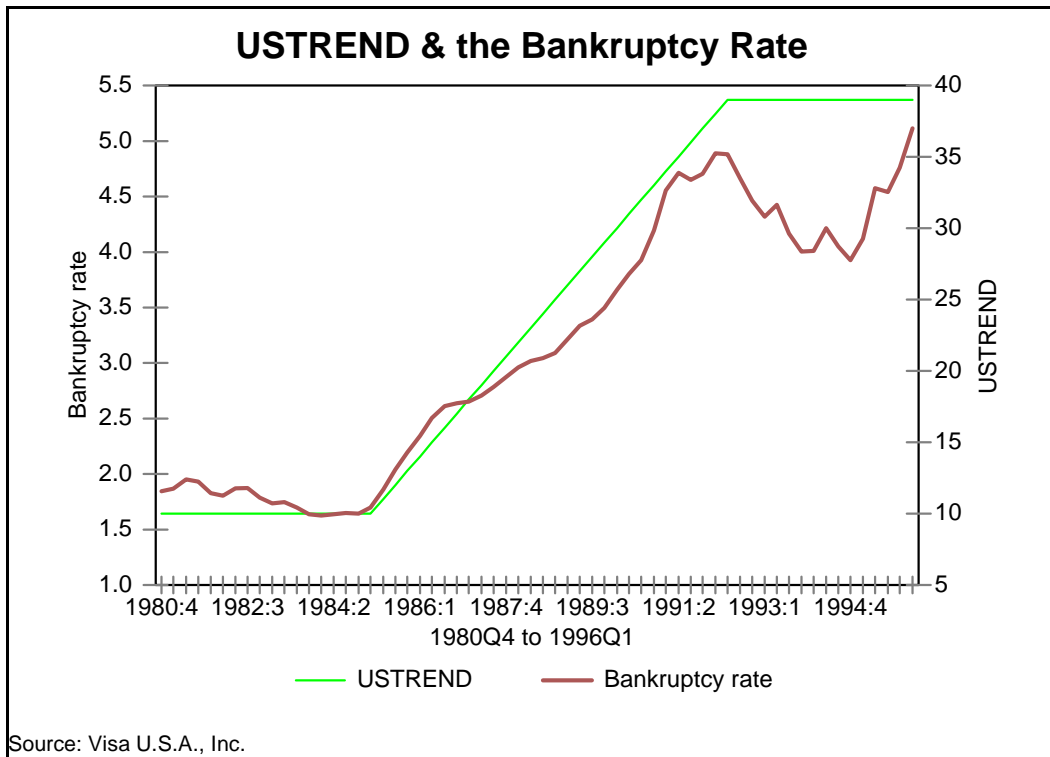
In addition to economic drivers, social factors play an important role in influencing personal bankruptcy rates. These *may* [emphasis mine] include an erosion of the social stigma against filing for bankruptcy, increased legal advertising, and the increased availability of credit to individuals who have declared bankruptcy in the recent past.

Visa bases its conclusion on the apparent statistical significance and positive sign of the coefficient on their variable USTREND. However, this variable does not measure any of these social factors; it is a simple trend that bears no necessary relationship to these factors.

The figure on the next page displays the variable USTREND and the number of personal bankruptcy filings per 1000 adults, both from the Visa data set. Even though they had no observations on the various social factors they cited, Visa assumed that the social factors behaved in a surprising way: the factors were assumed to have been unchanged between the fourth quarter of 1980 and the first quarter of 1985 and between the second quarter of 1992 and the first quarter of 1996, but to have risen between these two periods.<sup>4</sup> Visa's report does not explain how they arrived at this unusual assumption, but the close correspondence between the two series, particularly the dates when the trends in the two series change, strongly suggests that USTREND was constructed specifically to follow the trends in the

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4. It is interesting to observe that because the social trends variable is flat during 1995 and early 1996, Visa believes that their social factors played no role behind the increase in personal bankruptcy filings in that period.



personal bankruptcy rate. Consequently, Visa’s conclusion about the importance of social factors is unfounded. By defining their social trends proxy to mimic the broad trends of the variable they were attempting to explain, they arrived at their conclusion by assumption.

Also note that Visa uses the divorce rate as another social variable, but it is not an important factor behind the rise in bankruptcy filings according to the model. The model says that an increase in the divorce rate increases the personal bankruptcy rate in the following quarter. However, the divorce rate has trended down since 1981, indicating that, according to the model, the divorce rate has reduced personal bankruptcy filings.

#### REPAYMENT CAPACITY OF CONSUMERS WHO SEEK BANKRUPTCY RELIEF

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The Commission has seen several versions of this study—very preliminary results on December 17, 1996; an update on January 23, 1997; and another update that was first presented to the Subcommittee on Administrative Oversight and the Courts of the



Judiciary Committee of the U.S. Senate on April 11, 1997. A final draft of this study, as well as detailed information about the analysis and the sample, was not available at the time of this writing. Consequently, I focus on the April 11 draft for my review and refer to the earlier versions and to communications with the Commission about the study for details not included in the last version. I list these source documents at the end of this evaluation.

It is important to state at the outset that the lack of a fully annotated report and access to the data make it impossible to determine the reliability of the study's findings. The reliability of the study depends importantly on how well the sample represents the population of Chapter 7 debtors. If the sample is not representative, the results may overestimate or underestimate what is actually happening nationwide. The quality of the sample depends on not only how well it was collected, but also how the data were readied for analysis. For example, were missing values imputed or were observations thrown out? Were there anomalous values for certain variables that needed special treatment?<sup>5</sup> Lacking the necessary technical information about the sample and the analysis, my review of this study cannot be as detailed as my earlier review of the national model of personal bankruptcy filings by Visa. Apart from commenting on the definition of several key variables, I can only raise questions about the reliability of the sample.

This study addresses an important policy question: whether the bankruptcy code should include a "needs-based" bankruptcy requirement for consumers. According to Dr. Staten:

Because the law does not require the Court to evaluate a debtor's repayment capacity, it appears that discharges under Chapter 7 are awarded to many debtors who could repay a substantial portion of their debts.

The implication is that creditors would suffer significantly fewer losses if those debtors were required to repay at least some of their debts under a Chapter 13 or similar repayment plan instead. To test this hypothesis, the study estimates the capacity of consumers who filed a Chapter 7 bankruptcy to repay their nonhousing debts out of their future income minus living expenses. The study concludes that 25 percent of Chapter 7 debtors in 1996 could repay at least one-third of their nonhousing debt over a five-year period, 10 percent of the debtors could repay 79 percent, and 5 percent could repay all of their debts over five years.

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5. An indication of a potential anomaly is the mean value of other unsecured debt for the first decile shown in Table 1 of Dr. Staten's April 11, 1997 testimony. The definition of this variable is not given, but the value, \$32,830, is considerably greater than the means for the other deciles and the mean value of \$12,171 for the sample as a whole. The mean values for credit card debt and personal loans also are quite a bit larger for the first decile than for the sample as a whole. Unfortunately, there is no additional information about this decile.

There are three reasons why these estimates may give a misleading indication of the amount of losses that creditors could hope to recover under a “needs-based” bankruptcy requirement. One reason is because the study makes the wrong comparison. The amount of losses creditors could hope to recover under a “needs-based” bankruptcy requirement is the difference between the losses under Chapter 7 and the losses creditors would have experienced had the consumers filed under Chapter 13 instead. This difference is largely the amount of unsecured debt that the consumers would repay under Chapter 13. The study, however, estimates how much nonhousing debt could be repaid, where nonhousing debt is defined to include not only unsecured debt but also secured debt and priority claims. Thus the study includes as repayable those debts that are repaid, to the extent possible, in both Chapter 7 and Chapter 13.<sup>6</sup> The study also ignores reaffirmations.<sup>7</sup>

Another reason why the estimates may be misleading is that several assumptions of the study may contribute to an overstatement of repayment capacity. Dr. Staten’s testimonies are not clear whether estimates for administrative expenses of the bankruptcy filing are removed from the estimated repayment capacity, though others who have looked at this issue believe they are not.<sup>8</sup> The estimated repayment capacity also does not account for the possibilities that income may fall and living expenses may increase faster than income during the repayment period. These latter assumptions may be important because a large percentage of Chapter 13 repayment plans are never completed.<sup>9</sup>

A final reason why the estimates may be misleading is because the sample may not be representative of the population of consumers who filed under Chapter 7 in 1996. In his April 11 testimony, Dr. Staten says: “We believe the data in our sample are representative of all personal petitions filed in these 13 bankruptcy districts during the sampling period.” Nevertheless, Dr. Staten uses his results to make an inference about the number of consumer debtors nationwide who could repay some

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6. Professor Domowitz, in his letter to Mr. Williamson dated June 9, 1997, makes this point with reference to secured debt. Priority claims in personal bankruptcy are typically very small relative to secured and unsecured debts; they largely consist of alimony, maintenance, and support claims and taxes and certain other debts owed to governmental units. Dr. Staten shows in his April 11 testimony that priority claims have a minor impact on his results, but he does not indicate the impact of the much larger nonhousing secured debt.

7. Dr. Staten reported that he is collecting supplemental information about signed reaffirmation agreements from the courts in his sample to examine the importance of reaffirmations.

8. See the letter to Mr. Brady Williamson dated June 11, 1997 from Professors Marianne Culhane and Michaela White. They note that up to 10 percent of all payments under a Chapter 13 plan currently go to pay the Chapter 13 trustee.

9. In his letter to Mr. Brady Williamson, Professor Domowitz cites a study by the Administrative Office of the U.S. Courts that found about two-thirds of confirmed Chapter 13 plans filed from 1979 to 1991 were not successfully completed. This time period includes three recessions as defined by the National Bureau of Economic Research.

portion of their debts in 1996.<sup>10</sup> If the sample is not representative of the population as a whole, then at best his results apply only to consumers who filed under Chapter 7 in the 13 sampled courts that year.

Questions about the representativeness of the sample arise in part from the way the sampled areas were chosen. According to the testimony by Dr. Staten before the Commission on December 17, 1996, the researchers collected 300 observations from 13 bankruptcy courts.<sup>11</sup> Dr. Staten indicates that the courts “were selected to obtain variance in regional economic conditions, state-specific asset exemption levels, proportion of bankruptcies filed under Chapter 7 vs. Chapter 13 and recent growth rates in the number of personal bankruptcies.” However, choosing a sample of bankruptcy courts is not necessarily the same as choosing a sample of personal bankruptcies. It is not clear that these variations across courts are sufficient to capture the variations in the population of consumers who filed under Chapter 7 in 1996.

Another reason why the sample may not be representative of the population of Chapter 7 consumer bankruptcies is because the list of sampled bankruptcy courts seems very narrow. It contains only one court in the New England region and none in the Mid-Atlantic region, and Dr. Staten’s April 11 testimony indicates that the sampled courts accounted for only 30 percent of all personal bankruptcies filed in 1996.<sup>12</sup> This may not be a problem if appropriate sample weights were computed and used to account for the seemingly narrow focus of the sampled courts, but Dr. Staten’s testimonies do not indicate that sample weights were used.<sup>13</sup>

Finally, the sample may not be representative of all Chapter 7 personal bankruptcies in 1996 because it may include what are in fact small business bankruptcies. Dr. Staten’s testimonies do not indicate whether any effort was made to purge business bankruptcies from their sample.

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10. In his April 11, 1997 testimony, Dr. Staten says his estimates “imply that about 195,000 Chapter 7 debtors (25% of the total) had capacity to repay over one-third of their non-housing debt over a 60 month repayment period.” The 195,000 figure is about 25 percent of the total number of Chapter 7 filings nationwide in 1996.

11. Dr. Staten’s December 17, 1996 testimony indicates that the study began with 12 bankruptcy courts, but a thirteenth, Pittsburgh, as added after the data for the first 12 were collected. Dr. Staten indicated in a telephone conversation that Pittsburgh was added so that the sample would be more comparable to the sample used by Teresa Sullivan, Elizabeth Warren, and Jay Lawrence Westbrook for their study *As We Forgive Our Debtors*, (New York: Oxford University Press, 1989). Compared with the other 12 cities in the sample, Pittsburgh has the lowest number of filings per 1000 in 1995 according to Dr. Staten’s presentation before the Merchants Research Council.

12. In a telephone conversation, Dr. Staten indicated that this 30 percent figure is incorrect. In fact, the sampled courts represent 19.3 percent of all nonbusiness Chapter 7 filings in 1996.

13. In a telephone conversation, Dr. Staten indicated that he uses sample weights only to make sure that his sample is representative of the personal bankruptcy filings in the 13 courts.

Resources provided by Commission staff for my review:

1. "A Profile of Debt, Income, and Expenses of Consumers in Bankruptcy," testimony of Dr. Michael Staten before the National Bankruptcy Review Commission on December 17, 1996;
2. Letter dated December 20, 1996 from Professor Warren to Dr. Staten with follow-up questions to Dr. Staten's December 17 testimony;
3. Letter from Dr. Staten to Professor Warren dated January 16, 1997 and attached "Responses to Data Questions, 1996 Bankruptcy Study;"
4. "Repayment Capacity of Consumers in Bankruptcy," testimony by Dr. Michael Staten before the National Bankruptcy Review Commission on January 23, 1997;
5. "Do Consumers Take More Bankruptcy Relief Than They Need?," a copy of visuals used for a talk by Dr. Staten before the Merchants Research Council, February, 1997;
6. Letter from Professor Warren to Dr. Staten dated February 19, 1997;
7. Letter from Professor Warren to Dr. Staten dated March 31, 1997;
8. Fax from Dr. Staten to Professor Warren dated March 31, 1997;
9. "Repayment Capacity of Consumers Who Seek Bankruptcy Relief," testimony by Dr. Michael Staten before the Subcommittee on Administrative Oversight and the Courts of the Judiciary Committee of the U.S. Senate dated April 11, 1997;
10. Letter from Professor Warren to Dr. Staten dated May 22, 1997;
11. Letter from Professor Ian Domowitz to Mr. Brady Williamson dated June 9, 1997;
12. Letter from Professors Marianne Culhane and Michaela White to Mr. Brady Williamson dated June 11, 1997.

## CREDIT CARD DEFAULTS AND CREDIT CARD PROFITS

The latest draft of this paper, dated February 28, 1997, differs in one important respect from the draft Professor Ausubel delivered to the Commission on January 23, 1997. Professor Ausubel has modified his view about the drivers behind the current rates of consumer default and bankruptcy. In his earlier draft, Professor Ausubel largely blamed credit cards:

two factors – the cyclical state of the economy and the extranormal profitability of the credit card industry – combine to generate the high levels of consumer default.

In his later draft, credit cards appear to take a back seat to the total household debt burden:

two general economic factors – the cyclical state of the economy and the growing household debt burden – help to explain rates of consumer delinquency and bankruptcy. It will further be argued that a third factor – the extranormal profitability of the credit card industry – is important in generating the high levels of default.

I think Professor Ausubel is correct in placing greater emphasis on the total indebtedness of the household sector rather than on one aspect of it.

The main message of the paper has not changed, however. The paper addresses the likely impact on consumer credit default rates from tighter restrictions on the dischargeability of credit card debt in bankruptcy. Professor Ausubel argues that such restrictions will lead to an expansion of the supply of credit card debt and consequently to an increase in the frequency of overextended consumers and in the rate of credit card delinquencies. His argument is that the high profit margin of bank credit cards has encouraged banks to relax their credit standards on bank cards. Efforts to restrict the dischargeability of credit card debt would only raise the expected return on credit card lending, and thereby encourage banks to increase the supply of credit card debt to marginal borrowers.

This reasoning appears sound. What Professor Ausubel is not clear about is why such a development would be suboptimal. Although delinquencies would rise, creditors would not expand the supply of credit card debt unless it was profitable to do so. Professor Ausubel simply states in the later draft that an increase in overextended consumers would be a social problem. However, he never explains why.

## FOLLOW-UP COMMENTS ON DR. STATEN'S STUDY

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This memo contains additional comments on Dr. Staten's study of the repayment capacity of consumers who file for bankruptcy. It is based on the draft "Personal Bankruptcy: A Report on Petitioners' Ability-to-Pay," which is co-authored with Dr. John M. Barron of the Krannert Graduate School of Management and dated September 25, 1997. I received this draft on October 7, 1997, and spoke with Dr. Staten on October 9<sup>th</sup>. While the draft allays most of my concerns about the reliability of the sample data, it does not alter my view that the study gives a misleading indication of the amount of losses that creditors could hope to recover under a "needs-based" bankruptcy requirement.

In my previous memo, I raised a concern over the reliability of the data because Dr. Staten's testimonies did not describe the procedures he used to prepare the data for analysis. Dr. Staten's latest draft briefly describes these procedures. Section II.B notes that a potential seasonal bias does not seem to be a big problem. Dr. Staten compared samples of petitions filed in the Indianapolis court in November/ December, 1996 and in February/March, 1997 with those collected in May/July, 1996 and found "no significant differences in total debt, net income, and net expenses." (Dr. Staten notes that "Chapter 7 petitions filed in the November/December period were slightly more likely to have positive available income," but he does not report that this was a statistically significant result.) This section also states that there appeared to be no statistically significant differences between the usable petitions and those that were thrown out because they did not have any schedules other than a cover page. Section II.C, entitled "Data entry," notes that efforts were made to minimize data entry errors and detect outliers and inconsistencies that were created when transcribing the numbers from the petitions to his data base.

My conversations with Dr. Staten also cleared up two concerns stated in my previous memo. I noted in my footnote 5 that there appeared to be a potential anomaly in the debt measures of the Chapter 7 debtors in the first decile of available income. Dr. Staten told me this was not an anomaly—40 percent to 50 percent of the cases in the first decile of available income were petitioners who suffered a major loss of income. Dr. Staten also reported to me that he removed business bankruptcies from his sample.

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Dr. Staten also mentioned in our conversation that some attorney offices use computer software to prepare petitions. He did not know, however, if his sample had consecutive filings from the same attorney office (a possibility because he used a sequential sampling procedure). This raises a question about whether the sample contains idiosyncracies related to the attorney offices represented in the sample. Dr. Staten said that he has the data to check this possibility.

On the basis of this information, I feel more confident that Dr. Staten took reasonable care in collecting and cleaning his data. Without actually examining the data for oneself, of course, it is always difficult to judge the quality of any survey sample, and typically the best one can do is attempt to judge the procedures used to collect and clean the data. Dr. Staten's efforts to look for systematic errors in his sample and to minimize the inevitable errors that arise when entering data into a data base suggests that there are no gross errors in the sample from these sources. (It is somewhat surprising, however, that out of 3,798 petitions, Dr. Staten did not report finding any errors on the petitions themselves.) It would be helpful to know if the sample contains idiosyncracies related to the attorney offices represented in the sample.

Nevertheless, the draft makes clear that Dr. Staten believes that the sample is only representative of the 13 sampled courts, and not the population of consumer bankruptcies nationwide. The discussion in Section II.B indicates that sample weights were computed and used so that the reported means and medians are unbiased estimates of the population of consumer bankruptcies only in the 13 sampled courts. Consequently, Dr. Staten should not extrapolate his findings to the nation as a whole, as he does on page 25, where he says that Chapter 7 debtors nationwide had the capacity to repay \$4.9 billion in 1996 and later in conclusion 5 on page 31, where he applies his estimates to the total number of personal bankruptcies in 1996. In addition, the deciles of available income in the sample do not necessarily match the deciles for the population as a whole.

Apart from issues concerning the quality of the sample, there are still issues concerning research strategy. The definition of nonhousing debts in this draft now excludes priority debts, which is sensible, but still includes secured debts. As I mentioned in my previous memo, the relevant question is what percent of losses to unsecured creditors could be recovered if some Chapter 7 cases were forced into Chapter 13. Dr. Staten's study answers a different question: what is the capacity to repay all debts out of future income. In his latest draft, Dr. Staten presents another estimate of repayment capacity that assumes the Chapter 7 petitioners surrender all collateral (apparently even if the collateral value is greater than the associated debt) and apply all available income to repay the secured and unsecured debts. Dr. Staten does not indicate the relevance of this estimate.

It is not difficult to derive an answer to what I believe is the relevant question for the study. An important problem is deciding how to treat secured debts. If petitioners reaffirm any secured debts, then they will have less available income to repay unsecured creditors. And even though petitioners indicate their intention to reaffirm certain debts, they do not always follow through. Nevertheless, it is not difficult to put some bounds around the answer—an upper bound for losses to unsecured creditors would be the losses assuming all secured debt is reaffirmed, and a lower bound would be the losses assuming all collateral is surrendered.

On the matter of reaffirmations, Dr. Staten reports some figures on intended reaffirmations in his latest draft, but he still does not factor them into his estimated repayment capacity for the purposes of looking at the question of losses to unsecured creditors. He also suggests that there is a puzzle in his intended reaffirmation data—the percentage of petitioners in his sample with intended reaffirmations does not increase as he expects with either available income or net income. I do not find this puzzling. In Figure 7, the pattern appears right. The percentage increases after decile 6 because only the upper deciles have substantial and growing amounts of available income. The first six deciles all have no or a trivial amount of available income, so it is not surprising that the percentages are pretty much the same. I also do not expect the percentage who reaffirm secured nonhousing debts to rise with net income deciles because net income ignores living expenses and because the decision to reaffirm depends importantly on whether the petitioner wants to keep the collateral. I do agree with Dr. Staten that Figure 8 is a puzzle, however. Why do so many petitioners with no available income intend to reaffirm some secured, nonhousing debt?

Among the reasons I previously cited for why the study’s estimates may overstate repayment capacity are two additional assumptions. Table 3 in the latest draft indicates that petitioners have unpaid attorney fees, especially under Chapter 13, that should be removed from capacity. Moreover, Dr. Staten indicated to me that he assumed current overtime income for petitioners would remain unchanged over the five-year repayment period—undoubtedly an optimistic assumption.

Finally, I believe Dr. Staten’s conclusion 6 is too strong. Dr. Staten concludes that his estimate of the repayment capacity of Chapter 13 petitioners “contradicts the oft-heard assertion that Chapter 13 imposes excessive hardship on over-extended debtors.” The question of whether Chapter 13 imposes excessive hardship on over-extended debtors depends on the approved repayment plans for the 50 percent of Chapter 13 petitioners who do not have the capacity to repay all of their debts. Dr. Staten does not take these plans into account. Consequently, the observed failure rate for Chapter 13 plans cannot all be due to a lack of incentives to stay with the plan as Dr. Staten suggests.





CONGRESSIONAL BUDGET OFFICE  
U.S. CONGRESS  
WASHINGTON, D.C. 20515

June E. O'Neill  
Director

October 31, 1997

Mr. Brady Williamson, Chairman  
National Bankruptcy Review Commission  
One Columbus Circle N.E., Suite 5-130  
Washington, D.C. 20544

Dear Mr. Williamson:

I am responding to your request for my comments on the rebuttal to my review of Visa's model of personal bankruptcy filings. The rebuttal was prepared by Mr. Mark Lauritano of the WEFA Group. I did not find any arguments in the WEFA Group's rebuttal persuasive, and I stand by my original comments. I briefly address each of the WEFA Group's arguments below.

1. The WEFA Group's rebuttal argues that I am incorrect in criticizing their model as being a product of "data mining." In the WEFA Group's view, data mining only occurs when a researcher, "based on observed correlations defines a question or problem that is answered by the data." Because "the question of what causes bankruptcy and an initial set of hypotheses were all well developed before a single regression was performed," the WEFA Group believes it is not guilty of data mining.

The WEFA Group's argument is groundless because they have chosen a narrow definition of data mining that clearly does not apply in their case. Data mining can also occur and incorrect statistical inferences can be made even when, as in their case, the research question and an initial set of hypotheses exist before the statistical analysis begins. In the WEFA Group's case, data mining is the act of repeatedly testing the various hypotheses on the same body of data; that is, on a same set of observations on the rate of personal bankruptcy filings. The Kmenta and Lovell references I cited make this clear. Kmenta notes: "Probably the most common way of choosing a model in empirical research is by 'data mining.' A researcher confronted by a list of regressors tries various combinations of variables until satisfactory results (high  $R^2$ , 'correct' signs of regression coefficients, a reasonable value of the Durbin-Watson test statistic, etc.) are obtained." Lovell states: "When a data miner uncovers  $t$ -statistics that appear significant at the 0.05 level by running a large number of alternative

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regressions on the same body of data, the probability of a Type I error of rejecting the null hypothesis when it is true is much greater than the claimed 5%.” Put more simply, if you search for some good fits among 65 variables plus their lags, you are almost certain to find some just by chance. However, those good fits are as likely or more likely to reflect coincidence rather than genuine relationships. Consequently, the WEFA Group’s results very likely have little statistical significance.

2. The WEFA Group’s rebuttal dismisses my finding that the Durbin-Watson statistic indicates a serious problem with the model. They suggest that autocorrelation is not a serious problem because it only affects the efficiency of the coefficient estimates; in their view the coefficients in the original model are “best-linear-unbiased-estimates.” They also argue that “the inclusion of the autocorrelation term was not justified by the marginal change in the research findings” and that “the underlying conclusions remain unchanged” after including the autocorrelation term. I disagree with both of these points.

With regard to their first point, econometric textbooks note that many types of misspecification can give rise to autocorrelated residuals when using time-series data. Consequently, the low Durbin-Watson statistic of their model may indicate that the model is incorrectly specified. Their argument is valid only under the assumption that the true specification of the model is known, an assumption that does not hold in their case. Moreover, their statement that the ordinary least squares estimates are the “best-linear-unbiased-estimates” is incorrect because of the data mining and the autocorrelation in the error terms.

With regard to the WEFA Group’s second point, the changes in the model that arise when it is estimated with the autocorrelation correction are not marginal, but are in fact significant. The coefficient on the population variable becomes negative and statistically insignificant, while the coefficients on the debt service variables more than double in size and those on the other variables except the trend variable become smaller. Consequently, the underlying conclusions from the corrected model are considerably different from those of the original model. This indicates that the model is not a robust description of personal bankruptcy filings.

Rather than addressing this central issue—the reliability of the inferences from the model—the WEFA Group chooses to rely on the fact that certain statistical measures of goodness-of-fit do not change much when the autocorrelation correction is made. However, these measures are suspect because of the data mining. Moreover, because a model with an autocorrelation correction is a

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different specification from a model without one, it is difficult to compare goodness-of-fit across such different specifications.

3. The WEFA Group's rebuttal fails to provide any independent justification for the "social factors" trend term. They argue that because measures of the various social factors do not exist, it is "necessary" to construct a trend variable that is similar to the rate of bankruptcy filings. There is no reasonable explanation for why such a construction is "necessary." As I stated in my memo, the definition of this variable amounts to assuming the conclusion that social factors are important determinants of the rise in personal bankruptcy filings.

The WEFA Group incorrectly states that "The shape of this trend term reflects that the influence of these factors diminishes over time." If this were true, the values of the variable USTREND would fall at the end of the sample period. However, as I show in the chart in my memo, the values do not fall but remain unchanged at their largest value.

Because the WEFA Group admits that the social factors are not measured, and hence that we do not know what a correctly measured social trends variable would look like, they cannot say that "the term was included in the model to properly specify the other factors influencing bankruptcy."

4. The WEFA Group's rebuttal states that I am incorrect in arguing that the use of the median price of existing homes in their model likely reflects a spurious correlation. I based my argument on three observations: a) a large fraction of personal bankrupts do not own homes; b) the choice of only a two-quarter lag for the variable is puzzling; and c) no explanation was given for why house prices are important but not principal and interest payments on home mortgages. The WEFA Group's rebuttal confirms my first observation by noting that a study by Visa found that only 20 percent of Chapter 7 debtors and only 50 percent of Chapter 13 debtors own homes. Given that Chapter 7 filings represent roughly 70 percent of personal filings, their figures imply that only about 30 percent of personal bankrupts own homes. This small percentage of homeowners suggests that changes in the median price of existing homes is unlikely to be an important determinant of personal bankruptcy filings in the aggregate.

Their rebuttal does not directly address my other two observations, but they indicate that the home price variable is a business cycle indicator. It is not clear why the model needs this business cycle indicator because it already contains a better business cycle indicator—the employment growth term.

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5. The WEFA Group's rebuttal states that the differences between the published regression coefficients and my reestimates are due to rounding error. The differences are not important, as I stated in my memo. However, their rebuttal incorrectly states that the rounding error arises from the fact that they used "timeseries [sic] with 15 significant digits," while the data Visa gave me were "truncated at three digits." None of the source data used in their model are available with 15 significant digits. I suspect that they are confusing the number of significant digits with the number of digits the computer software uses when making the transformations of the raw data. Moreover, the data Visa gave to me contains a varying number of digits, which Mr. Lauritano should be aware of since I sent him the data that Visa sent to me.
  
6. The WEFA Group's rebuttal misinterprets my comment on the influence of the divorce rate. As I said in my memo, I understand that the divorce rate in their model has a positive influence on bankruptcy filings. I do not dispute this. My point is that the divorce rate did not contribute to an increase in bankruptcy filings in their sample period because the divorce rate itself falls in their sample period.

Sincerely,

Kim J. Kowalewski

cc: Mr. Mark Lauritano, WEFA Group  
Dr. Thomas Layman, Visa U.S.A. Inc.



CONGRESSIONAL BUDGET OFFICE  
U.S. CONGRESS  
WASHINGTON, D.C. 20515

June E. O'Neill  
Director

October 30, 1997

Mr. Brady Williamson, Chairman  
National Bankruptcy Review Commission  
One Columbus Circle N.E.  
Suite 5-130  
Washington, DC 20544

Dear Mr. Williamson:

This is in response to your request for an explanation of the meaning of the disclaimer which appeared as a footnote in a paper written by Kim Kowalewski to you, as Chairman of the National Bankruptcy Review Commission.

The paper, entitled Evaluations of Three Studies Submitted To The National Bankruptcy Review Commission, includes the following disclaimer:

"The analysis and conclusions presented in this memo are those of the author and do not necessarily represent the position of the Congressional Budget Office, (CBO)."

This language is used by CBO analysts on occasion to prepare papers outside of the normal course of our work for the United States Congress. The disclaimer is intended to serve as notice to the reader that, while the work has received informal vetting within the CBO, the work did not go through our office's full internal editorial procedures and was not reviewed or approved by the Director of CBO. I hope this answers your question. If you have other questions please call.

Sincerely,

James L. Blum  
Deputy Director