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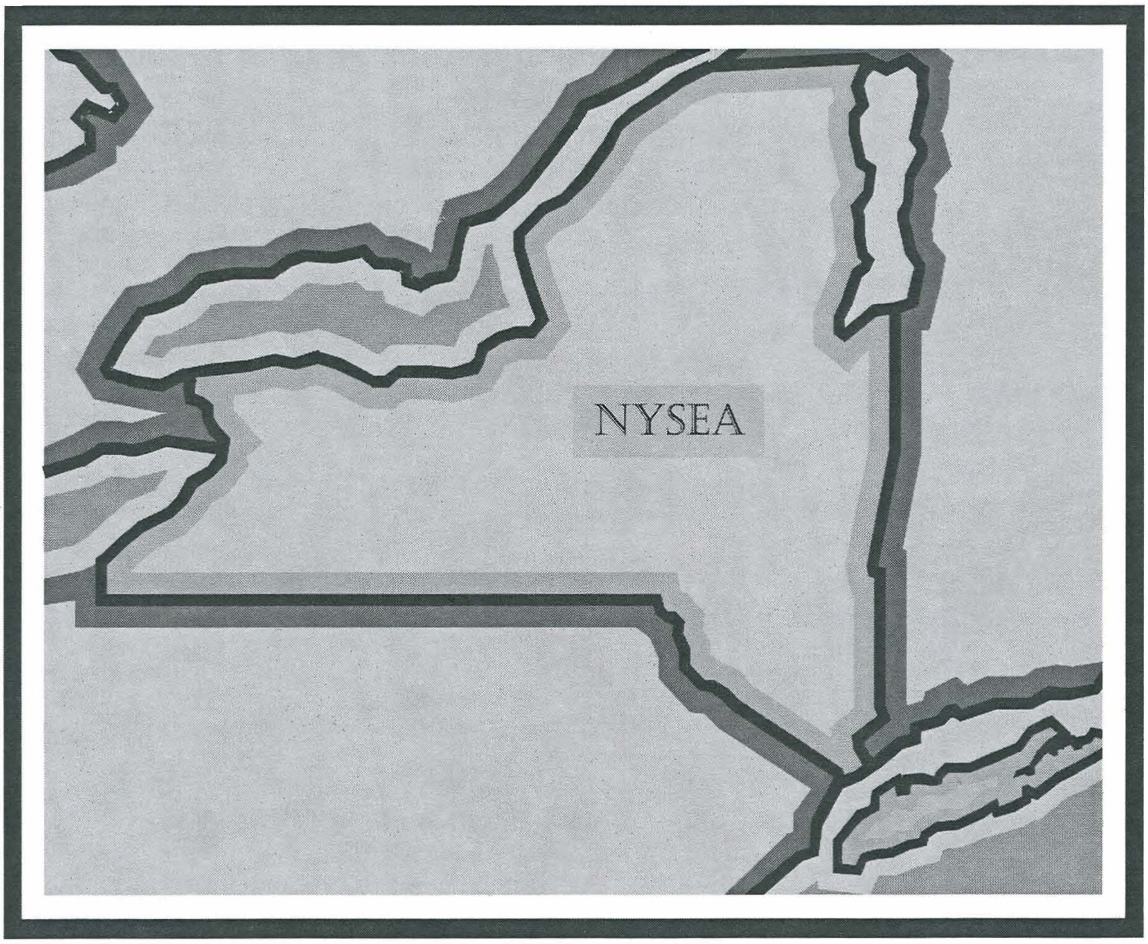

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EDITORIAL

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The Slope of the U.S. Nominal Treasury Yield Curve and the Exchange Rate

Matiur Rahman* and Muhammad Mustafa**

Abstract

This paper examines the role of the exchange rate in the determination of the slope of the nominal Treasury yield curve in the context of the U.S.A. Monthly data from June 1976 through June 2005 are utilized. This paper concludes that changes in the U.S. dollar value index significantly influence the changes in the slope of the U.S. nominal Treasury yield curve. As a result, the exchange rate should be included as one of the explanatory variables in the yield curve empirics.

I. Introduction

The study of the Treasury yield curve in terms of its level, slope and curvature is of profound importance as it provides information on the current monetary policy stance, expected future economic activity, inflation and the real interest rate [Bernanke and Blinder (1992), Estrella and Hardouvelis (1991), Blanchard (1985), Mishkin (1990)].

The treasury yield curve, which is the plot of the term structure or varying yields of Treasury bonds at increasing maturity, has been the subject of many studies because it sets the basis for expected movements of interest rates in the future. Fixed-income investors use it as a reference point in forecasting interest rates, in pricing bonds and in setting strategies to boost their portfolio returns. The assumption behind a steep yield curve, for example, is that interest rates will rise in the future. As the economy expands, the associated risks of higher inflation and interest rates can hurt investors or bondholders due to dropping prices of bonds that move inversely to yields.

On the other hand, monetary policymakers use the yield curve in making decisions about long-run interest rates and inflation-targeting. A monetary policy tightening usually causes an upward shift in the entire yield curve with a stronger impact on the short-term end. With the threat of higher inflation as the economy expands, a monetary tightening, signals that the central bank expects upward pressure on

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inflation and higher interest rates. Investors respond by selling government securities in the secondary market resulting in lower prices and higher yields.

The yield curve has been characterized in several ways but the model of Nelson and Siegel (1987) is the one widely used by central banks around the world according to a survey by the Bank for International Settlements (1999). They identified three factors which they call the *level*, *slope* and *curvature*. These factors represent the long-, short- and medium-term interest rates and could account for 96 percent of the shape of the yield curve.

Studies relating macroeconomic variables to the components of the yield curve are fairly recent and the work of Ang and Piazzesi (2003) led investigations. In their paper, they find that inflation largely accounts for the movement in the level and slope of the yield curve. Evans and Marshall (2001) and Diebold, Rudebusch and Aruoba (2003) show that monetary policy shocks affect the slope. While Dewachter and Lyrio (2003) and Hordahl, Tristani and Vestin (2002) claim that shocks to inflation and the output gap account for curvature. .

Irving Fisher (1930) was the first paper to clearly distinguish between the real and the nominal interest rates. Several theories have been proposed to characterize the relationship between the interest rate and term to maturity, a relationship that is called the term structure of interest rates. Fisher's theory predicts that the yield curve, which is the plot of the term structure, will be upward sloping if expected inflation is positive.

Traditionally, there are three main theories on the term structure of interest rates. The first is the expectations theory which states that the observed long-term rate is the average of current short rates and expected future short rates. This means that an upward-sloping yield curve implies that investors anticipate increases in short-term interest rates in the future. On the other hand, the downward sloping yield curve suggests that investors anticipate the opposite.

The second is the liquidity preference theory, which assumes that long-term bonds are more risky and investors will demand a premium for holding them. Long-term rates are equal to the sum of average future short rates and a liquidity premium. An extension of this theory incorporates not just the liquidity premium but also the risk premium for inflation, default and maturity mismatch. This proposes that even if the expected future short-term rates stay constant (or even decline), the yield curve could be upward sloping, if the liquidity premium is high enough. But if the yield curve is downward sloping and the liquidity premium is positive, then the future short-term rates are expected to drop.

Last is the market segmentation theory where it is assumed that there are two distinct markets for short- and long-term bonds. Demand and supply in the long-term bond market determine long-term yields, and the demand and supply in the short-term bond market determine the short-term rates. This implies that the expected future rates have little to do with the shape of the yield curve. Along this line, the preferred habitat theory further modifies market segmentation by stating that investors will switch out of their preferred bond markets if premiums are inadequate.

The question of how to respond to changes in the slope of the yield curve received some attention in the United States at the end of 1995. In the United States, the yield curve became very flat in 1995. Based on the historical relationship between economic activity and the slope of the yield curve, the probability of a recession would have been very high (Estrella and Mishkin, 1997). The policy question was whether in that situation short-term interest rates should be reduced to avoid a downturn. However, the low-term spread in 1995 was mainly the result of falling long-term rates due to a smaller term premium as a result of reduced inflationary expectations in the U.S. bond market. Interestingly enough, this fall in the term premium coincides with the pre-emptive tightening by the Federal Reserve in the same period. By implication, the particularly flat yield curve should not have been interpreted as signaling an economic downturn because inflation scare shocks have only nominal effects.

In all of these prior studies, the impact of exchange rate movements on the slope of the yield curve has been ignored. In this age of globalization, the rising international mobility of capital and the foreign investment component of global portfolios are affected by exchange rate changes. This paper seeks to explore the effects of the changes in the U.S. overall exchange rate (external value index of the dollar) on the slope of the U.S. nominal Treasury yield curve using monthly data.

The remainder of the paper is organized as follows. Section II provides a survey of the literature. Section III outlines the empirical methodology. Section IV reports results, and section V offers conclusions.

II. Survey of Literature

Models of the term-structure of interest rates have been mostly formulated in continuous time and in an arbitrage-free framework. Typically, bond yields are affine functions of a number of state variables that capture the uncertainty present in the economy. In many specifications, the state variables are unobserved. Econometrically, the latent factors are extracted from bond prices or yields by either assuming that a few bonds are priced perfectly by the model or by filtering techniques if all bonds are assumed to be priced with error. When three factors are specified, they are often interpreted as the level, slope, and curvature of the yield curve, following Litterman and Scheinkman (1991). Dai and Singleton (2003) and Piazzesi (2003) provide thorough surveys of this class of models.

Recently, several researchers have added observable macroeconomic variables to the latent factors in an attempt to understand the channels through which the economy influences the term structure, and not simply describe or forecast the movements of the term structure. Ang and Piazzesi (2003) and Ang, Dong, and Piazzesi (2007) estimate Taylor (1993) rules and identify monetary policy shocks using no-arbitrage pricing techniques. They find that inflation and the output gap account for over half of the variation of time-varying excess bond returns and most of the movements in the term spread. Models with more macroeconomic structure have also been proposed recently by Hordahl, Tristani, and Vestin (2006), Rudebusch and Wu (2004), and Bekaert, Cho, and Moreno (2003). These models combine the affine arbitrage-free dynamics for yields with a New Keynesian macroeconomic model, which typically consists of a monetary policy reaction function, an output equation, and an inflation equation.

In each of the aforementioned models, risk premiums for the various sources of uncertainty are obtained by specifying time-varying prices of risk that transform the risk-factor volatilities into premiums. The prices of risk, however, are estimated directly from the data without accounting for the fact that investors' preferences and technology may impose some constraints between these prices. Indeed, according to Diebold, Piazzesi, and Rudebusch (2005), the goal of an estimated no-arbitrage macro-finance model specified in terms of underlying preference and technology parameters (such that the asset pricing kernel is consistent with the macro dynamics) remains a major challenge.

In Piazzesi (2003), affine general-equilibrium models are specified with preference shocks that are related to state variables, as in Campbell (1986) and Bekaert and Grenadier (2003). Wachter (2006) also proposes a consumption-based model of the term structure of interest rates, where nominal bond yields depend on past consumption growth and inflation. This model is essentially the same as the habit model of Campbell and Cochrane (1999), but the sensitivity function of the surplus consumption to innovations in consumption is chosen so as to make the risk-free rate a linear function of the deviations of the surplus consumption from its mean. Moreover, Wachter calibrates her model so as to make the nominal risk-free rate in the model equal to the yield on a three-month bond at the mean value of surplus consumption.

The dynamic interaction between the macro economy and the term structure is explored by Diebold, Rudebusch, and Aruoba (2006) in a Nelson-Siegel empirical model of the term structure, complemented by a VAR model for real activity, inflation, and a monetary policy instrument. They find that the causality from the macro economy to yields is much stronger than in the reverse direction.

III. Empirical Methodology

First, it is necessary to examine the stationary/nonstationarity property of time series data to determine the most appropriate econometric technique in order to avoid incorrect conclusions. Provided the time series data are found to be stationary, the most appropriate procedure is the simple Granger causality test. In the case of nonstationarity in the time series data, the most appropriate procedures are cointegration and error-correction models.

To begin with this examination, the cointegration regressions are specified as follows:

$$x_t = \alpha_0 + \alpha_1 y_t + e_t \qquad (1)$$

$$y_t = \alpha_0' + \alpha_1' x_t + u_t \qquad (2)$$

where, x_t is the slope of Treasury yield curve (as common practice, this is the difference between 10-year T-bond yield and 2-year T-bond yield), y_t is the U.S. dollar value index, and e_t is the stochastic error term.

The variables x_t and y_t are integrated of order d (i.e., I(d)) if the time series data on x_t and y_t have to be differenced d times to restore stationarity. For d equal to 0, x_t and y_t are stationary in levels and no differencing is needed. For d equal to 1, first differencing is needed to restore stationarity.

Modified Dickey–Fuller and modified Phillips-Perron procedures are applied to test for nonstationarity in each variable. The KPSS test for level stationarity, developed in Kwiatkowsxi, Phillips, Schmidt and Shin (1992), is widely used as a counterpart of the ADF test. The test is outlined by considering the model:

$$y_t = \mu + \alpha y_{t-1} + u_t, \ t = 1, \dots, T. \tag{3}$$

For convenience, it is assumed that T is an even number. The interest is in the null hypothesis of level stationarity,

H_0: $|\alpha| < 1$,

Against the alternative hypothesis of a unit root,

$H_1: \alpha = 1$.

The KPSS test for the null of level stationarity is

$$KPSS = \sum_{t=1}^{n} s_t^2 / \hat{\sigma}_u^2$$

where, $s_t = \sum_{i=1}^{t} \hat{u}_i$ and $\hat{\sigma}_u^2$ is the long − run variance estimator using $\{\hat{u}_t\}$.

The co-integration procedure developed in Johansen (1988) and Johansen and Juselius (1990, 1992), avoids the above drawback by allowing interactions in the determination of the relevant economic variables and being independent of the choice of endogenous variable. Most importantly, it allows explicit hypotheses tests of parameter estimates and rank restrictions using likelihood ratio tests. The empirical exposition of the Johansen and Juselius methodology is as follows:

$$\Delta V_t = \tau + \Omega V_{t-1} + \sum_{j=1}^{k-1} \Omega_j \Delta V_{t-j} + m_t \tag{4}$$

where, V_t denotes a vector of log of relevant variables, and Ω equals $\alpha\beta$. Here, α is the speed of adjustment matrix and β is the cointegration matrix. Equation (4) is subject to the condition that Ω is a less-than-full rank matrix, i.e., $r < n$. This procedure applies the maximum eigenvalue test (λ_{max}) and the trace test (λ_{trace}) for the null hypotheses on r. Of these two tests, the λ_{max} test is expected to offer a more reliable inference as compared to the λ_{trace} test (Johansen and Juselius (1990)). Again, the Johansen and Juselius test procedure suffers from its super-sensitivity to the selection of the lag structures. As a result, this study employs both the ADF and the Johansen-Juselius procedures for cointegration. It is likely that these two procedures will provide contradictory evidence in some instances.

If x_t and y_t are found to be cointegrated by either the ADF procedure or the Johansen-Juselius procedure or both, there will exist an error-correction representation (Engle and Granger (1987)). The error-correction model may take the following form:

$$\Delta x_t = \beta_1 e_{t-1} + \sum_{i=1}^{k} \phi_i \Delta x_{t-i} + \sum_{j=1}^{k} \delta_j \Delta y_{t-j} + u_{1t} \tag{5}$$

$$\Delta y_t = \beta_2 u_{t-1} + \sum_{i=1}^{k} \pi_i \Delta x_{t-i} + \sum_{j=1}^{k} \gamma_j \Delta y_{t-j} + u_{2t} \tag{6}$$

The reverse specification is considered due to plausible bidirectional causality. In these two equations, the series x_t and y_t are cointegrated when at least one of the coefficients β_1 or β_2 is not zero. If $\beta_1 \neq 0$ and $\beta_2 = 0$, then y_t will lead x_t in the long run. Again, if $\beta_2 \neq 0$ and $\beta_1 = 0$, then x_t will lead y_t in the long run. If δ_j's are not all zero, movements in y_t will lead those in x_t in the short run. If π_i's are not all zero, movements in x_t will lead movements in y_t in the short run.

The error-correction model (ECM) was first introduced by Sargan (1964) and subsequently popularized by numerous papers, (e.g., Davidson et al. (1978), Hendry et al. (1984)). It has enjoyed a revival in popularity due to the recent work of Granger (1986, 1988), and Engle and Granger (1987) on cointegration. Its importance lies in its ability to combine short-run dynamics and long-run relationships in a unified system. If two variables are cointegrated, the long-run Granger causality will stem from at least one direction. Sometimes, it is desirable to exclude insignificant lags to improve the efficiency of OLS estimates of parameters (Baghestani and Mott (1997)). A lack of cointegration does not, however, preclude the short-run dynamics and Granger causality. In the absence of a long-run relationship, equations (8) and (9) should not include the error-correction term for the detection of Granger causality between two variables (Bahmani and Payesteh (1993)). The optimum lag-lengths are determined by the FPE (Final Prediction Error) Criterion (Akaike, 1969).

Monthly data from June, 1976 through June, 2008 are employed. The data source includes various issues of the Federal Reserve Bulletin. The sample period begins from June, 1976 as the U.S. dollar value index data are available since then.

IV. Results

The unit root test results are shown in Table 1.

As observed in Table 1, both the modified Dickey-Fuller and the KPSS tests reveal nonstationarity in the slope of the U.S. Treasury yield curve and the U.S. dollar value index at the 1 percent level of significance with I(I) behavior.

Table 1: Unit Root Tests

SERIES	LEVEL		DIFFERENCES	
	DF-GLS	KPSS	DF-GLS	KPSS
x	-2.6017	0.389	-13.890	0.0536
y	-1.623	0.603	-13.542	0.0831

* The modified Dickey-Fuller (DF-GLS) critical values [in Elliot et al., (1996)] are-2.653 and -1.954 at 1 percent and 5 percent levels of significance, respectively. The KPSS critical values are 0.739 and 0.463 at 1 percent and 5 percent levels of significance, respectively.

Next, the Johansen-Juselius tests (λ_{trace} and λ_{max}) for cointegration are implemented. The results are shown in table 2.

Table 2: Johansen-Juselius Cointegration Tests

	Between x and y	
Hypothesized No. of CE(s)	λ_{trace}	λ_{max}
None	17.04048	11.88798
	(15.49471)	(14.26460)
At most 1	5.152505*	5.152505*
	(3.841466)	(3.841466)

* Trace test indicates two cointegrating relationships and Max-eigenvalue test indicates one cointegrating relationship at 5 percent level of significance. The associated critical values of λ_{trace} and λ_{max} tests are reported in parentheses.

Table 2 depicts cointegrating relationship between the variables as the null hypothesis of no cointegration is rejected at the 5 percent level of significance. This inference is based on the comparisons of the computed and the respective critical values of λ_{trace} and λ_{max} statistics.

Finally, the relevant error-correction models (5) and (6) are estimated. The results are:

Table 3: Error-Correction Models

Dependent Variable	Causal Variable	Lag Order	F-statistics	Error-Correction Terms
Δx	Δy	2	12.07921*	-0.029129 (-2.19956)**
Δy	Δx	2	9.8411*	0.000191 (2.2498)**

* Significant at the 1 percent level. ** Significant at the 5 percent level. The optimum lag orders are determined by the FPE criterion (Akaike, 1969). The associated t-values are reported in parentheses.

Table 3 discloses a long-run causal relationship flowing from the changes in the U.S. exchange rate to the changes in the slope of the U.S. Treasury yield curve. This long-run relationship between variables seems fairly strong. The negative sign of the coefficient of the error-correction term and its statistical significance in terms of the associated t-value suggests that U.S. dollar fluctuations affect the U.S.

Treasury yield curve. This long-run relationship between the variables seems to be fairly strong. The F-statistic at 12.08 suggests short-run feedback relationships between the variables. The estimates of the reverse specification of the error-correction model suggest otherwise, though.

V. Conclusions

The slope of the U.S. nominal Treasury yield curve and the U.S. dollar value index are non-stationary in levels depicting I(I) behavior. The variables are cointegrated. There is an evidence of a long-run causal flow from the changes in the external dollar-value index to the changes in the slope of the U.S. nominal Treasury yield curve with short-run interactive feedback effects. This finding suggests that the U.S. overall external dollar-value index should be included as one of the determinants in the empirical estimates of the slope of the U.S. nominal Treasury yield curve.

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Fuel Efficiency and the Determinants of Traffic Fatalities: A Comparison of Empirical Models

Mark Gius*

Abstract

The present study has three primary purposes. First, this study will attempt to determine the effects of fuel efficiency standards on traffic fatalities; this is a long-running topic of contention in the area of vehicle safety. Second, this study will look at the effects of legal restrictions on traffic fatalities. Once again, there is dispute regarding the potential effects that laws may have on driver behavior. Finally, the present study will compare and contrast three commonly used empirical techniques in order to determine which variables are most robust or consistent in their effects on traffic fatalities. Using data from 48 states over a 23 year time period, the results indicate that fuel efficiency standards have a negative effect on traffic fatalities, irrespective of the type of empirical technique employed. Regarding other pertinent variables, the results of two of the three models suggest that socioeconomic factors, such as the age distribution of the state and per capita alcohol consumption, had much more significant effects on traffic fatalities than state-imposed legal restrictions, such as minimum legal driving ages.

INTRODUCTION

In 1975, Congress passed legislation that established fuel economy standards for all new automobiles sold in the United States. This legislation was enacted in reaction to the Arab Oil Embargo of 1973 and the rapidly escalating gasoline prices that followed. Initial fuel economy standards were set at 18 miles per gallon (mpg) for passenger cars and, at the most, 17.2 mpg for light trucks. Current standards are 27.5 mpg for passenger cars and 20.7 mpg for light trucks.

Ever since the imposition of these fuel economy standards, which are known as the Corporate Average Fuel Economy Standards (CAFE), an ongoing debate has ensued regarding the impact of these standards on traffic fatalities. Auto manufacturers, for example, contended that the only real way to achieve these standards in the early years was to decrease the weights of cars and trucks. According to this argument, these reductions in weight would increase the probability that the occupants of the vehicles would be severely injured or die in traffic accidents. In fact, Noland (2005) noted that one obstacle to increasing fuel efficiency standards has been its potential effect on vehicle safety.

Others, however, countered that, although manufacturers initially may have had to reduce vehicle weights to achieve the required fuel economy standards, advances in engine technology and safety

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design have resulted in vehicles being manufactured today that are not only much more fuel efficient, but also much safer, than the cars of thirty years ago. In addition, many of these advances in engine technology would not have been undertaken without fuel economy standards; the reason for this is because the real price of gasoline fell from the early 1980s to the early 2000s. Hence, auto manufacturers would have had little incentive to improve engine technology, especially in the area of fuel consumption, if it had not been for the fuel economy standards set by the federal government.

In order to properly estimate the effects of fuel efficiency standards on traffic fatalities, a model of traffic fatalities must be devised. As will be noted in this paper, there have been numerous studies on this topic. Unfortunately, few prior studies looked at an all-encompassing model, one that incorporates not only fuel efficiency but also such factors as legal restrictions on driving, road conditions, vehicle characteristics, and driver attributes. Using such a model would reduce any potential specification bias that a traffic fatalities model may have.

Prior research in this area has used a plethora of empirical techniques and a number of ways to define traffic fatalities. Unfortunately, little research has been conducted on the potential statistical differences between these various models, and what effect, if any, the use of different empirical techniques has on the results and thus the implications that policymakers may draw from this research. If different empirical techniques produce vastly different results, then research on traffic fatalities is of limited value unless robust results, or results that are similar between the different models, may be identified.

The present study has three primary purposes. First, this study will attempt to determine the effects of fuel efficiency standards on traffic fatalities. Second, this study will look at the effects of legal restrictions on traffic fatalities. Finally, the present study will compare and contrast three commonly used empirical techniques in order to determine which factors are the most robust or consistent in their effects on traffic fatalities.

The organization of the present study is as follows. This paper will first examine the prior literature in this area. Second, the empirical model employed in the present study will be discussed. Finally, the data used and the results obtained will be presented.

Results of the present study suggest that fuel efficiency standards have a negative effect on traffic fatalities, regardless of the type of empirical technique employed. These results indicate that, holding all other factors constant, the federal government's efforts to reduce gasoline consumption by increasing the fuel efficiency of cars did not result in an increase in deaths due to traffic accidents. Regarding other pertinent variables, the results of two of the three models suggest that socioeconomic factors, such as the age distribution of the state and per capita alcohol consumption, had much more significant effects on traffic fatalities than state-imposed legal restrictions, such as the minimum legal driving age.

LITERATURE REVIEW

The literature in the area of traffic fatalities and the causes of automobile accidents is extensive. Bester (2000) and Lourens, Visser, and Jessurun (1999) both offer excellent summaries of the current literature in this area. The present study's focus, however, is on the factors that affect traffic fatalities at the state-level, with special emphasis on the effects of fuel efficiency standards and legal restrictions on fatalities. Hence, the following literature review will focus on those articles that had as their primary focus the effect of fuel efficiency standards and other legal restrictions on traffic fatalities.

One of the earliest studies in this area was Loeb (1987). Using OLS and only looking at state-level data for the year 1979, Loeb concentrated his efforts on determining whether or not a vehicle inspection system at the state-level reduced traffic fatalities. The dependent variable, traffic fatalities, was defined as number of traffic fatalities per 100 million vehicle miles of travel. Loeb also included in his regression a variety of state-level socioeconomic variables, such as personal income per capita, population density, and the age distribution of the state's population. The author found that the minimum legal drinking age, income, and miles of highway at the state-level had no statistically-significant effects on traffic fatalities, while the population age distribution, per capita alcohol consumption, the average speed on interstate highways, and annual vehicle inspections were all statistically significant.

Using a sample of domestic sedans, Crandall and Graham (1989) estimated the weight of cars and found that, because of the government-imposed fuel efficiency standards (CAFE), cars weighed 18 percent less than they would have without the binding fuel constraints. Extrapolating their data to 1989, the authors contend that CAFE reduced the average car's weight by 500 pounds in 1989. Using prior research on weight-safety relationships, Crandall and Graham estimated that CAFE was associated with a 27 percent increase in occupant fatality risk.

Crandall and Graham estimated a traffic fatalities model using OLS and aggregate national data. The dependent variable was the ratio of fatalities to total motor vehicle miles traveled per year, which was the same as Loeb (1987). A few of the more pertinent explanatory variables used were per capita income, per capita alcohol consumption, and the average weight of cars on the road. Results indicated that income had a positive effect on traffic fatalities while vehicular weight had a negative effect.

Khazzoom (1994) examined the effect of vehicle weight and size on single-vehicle passenger car fatalities. In addition to estimating a model of CAFE, the author also estimated a traffic fatalities model. Using state-level data for the years 1985-1989, the author employed ridge estimation in order to account for potential problems with multicollinearity. He defined his dependent variable as total fatalities, and he used a log-log functional form. His results suggested that seat belt usage had an insignificant impact on traffic fatalities, while all of his other explanatory variables, which included the percentage of drivers older than 70, the percentage of drivers who drive while intoxicated, and per capita income, were significant with the expected sign.

Yun (2002) examined the role that CAFE had in creating or encouraging offsetting behaviors among drivers. The offsetting behavior hypothesis states that drivers are aware that the fuel economy standards have reduced the weights of cars and have thus increased the occupant fatality risk. In reaction to this increased risk, drivers operate their vehicles much more safely than they would have if CAFE had not been enacted. Estimating three equations where the dependent variable was measured as fatalities per 100 million vehicle miles and using national-level data for the period 1963-1993, Yun found that the fuel economy standards have caused a 21.1 percent decrease in the accident rate but a 14.99 percent increase in the vulnerability rate; hence, the net effect was a 6.11 percent decrease in the annual fatality rate. In addition, it was found that per capita expenditures on alcoholic beverages, the speed limit, and the percentage of very young and very old drivers all positively affected the fatality rate, while per capita income had a negative effect on traffic fatalities.

Noland (2004) used state-level data and incorporated into his analysis safety-belt usage laws in order to determine if CAFE had an effect on traffic fatalities. Noland used total fatalities as a dependent variable and employed a negative binomial model. His estimation of state-level fuel efficiency, however, does not take into account the various model years of cars that are present in any given state's stock of cars. Instead, he estimated fuel efficiency by dividing state-level fuel consumption by total vehicle miles traveled. This estimate is a proxy at best and does not take into account the potential model year mix of the cars on the road in any given year or state.

Using panel data estimation techniques, the author found that the fuel economy standards may have had an effect on traffic fatalities in the 1970s and early 1980s, but that since the mid-1980s, fuel economy standards have not had a statistically significant effect on traffic fatalities. Regarding his other explanatory variables, Noland found that per capita income, per capita alcohol consumption, and a greater share of drivers under the age of 25 all increased traffic fatalities.

Noland (2005) looked at international data regarding the effect of fuel economy standards on traffic fatalities. Using a methodology similar to his earlier work, the author once again used as his dependent variable total fatalities and employed a negative binomial model. Using data from thirteen countries for the years 1970-1996, the author found that fuel economy standards had no statistically-significant effect on traffic fatalities. Regarding the other independent variables in the total fatality regression, per capita alcohol consumption, motor vehicles per capita, vehicle miles traveled per capita all had positive and significant effects on fatalities. The percentage of population over 65 years of age and physicians per capita had negative effects on traffic fatalities.

In both of his studies, Noland noted that the use of count data and the negative binomial model were a vast improvement over the work of earlier studies, such as Khazzoom (1994) and Crandall and Graham (1989). He noted that both studies used techniques (OLS or time series estimation techniques) that are inappropriate when using count data (Noland, 2005, p.2186). Noland further noted that, although one may use a Poisson model for count data, using such a technique for traffic fatality data would be inappropriate given that the mean and variance of traffic fatality data typically are not equal. Hence, a fixed effects, negative binomial regression, which is a type of Poisson regression, was used instead.

What Noland failed to mention , however, was that neither Khazzoom (1994) nor Crandall and Graham (1989) actually used count data; rather, as noted above, they used fatality rate data which is not count data. Their primary reason for using least squares rather than the negative binomial model was because fatality rates can be assumed to be continuous; hence, the use of a Poisson-type regression in such a situation would actually be inappropriate.

One of the more recent studies on traffic fatalities was Vereeck and Vrolix (2007). Looking at data for 15 European countries and for the period 1996-2000, the authors attempted to determine if the willingness of citizens to obey traffic regulations had any effect on traffic fatalities. Defining their dependent variable as total fatalities, using a log-log functional form and panel data, Vereeck and Vrolix found that those nations that had citizens who were more likely to obey traffic laws had fewer traffic fatalities. Regarding the other explanatory variables used in their study, it was found that speed limits, vehicle miles traveled, and per capita alcohol consumption were all positively related to traffic fatalities, while GDP per capita was negatively related. It should be noted that the authors used at most five explanatory variables in order to estimate the determinants of traffic fatalities; this is rather low when compared to other studies in this area.

Finally, Vereeck and Vrolix noted that they used as a dependent variable the total number of fatalities and not a fatality rate, primarily because using a fatality rate would lead to spurious correlation (Vereeck and Vrolix, 2007, p.393). The authors made this comment assuming that traffic exposure, as measured by total vehicles miles traveled or vehicle miles traveled per capita, would be used as an explanatory variable. However, if a fatality rate is used as the dependent variable, then traffic exposure does need to be used as an explanatory variable since a rate does not necessarily depend upon traffic exposure as much as total fatalities do. The authors do not comment on the appropriateness of using a fatality rate model without an exposure variable. Finally, Vereeck and Vrolix do not reference Noland's 2004 or 2005 study at all and make no mention of the appropriateness of using a negative binomial model to estimate the determinants of total fatalities.

In summation, Loeb (1987), Crandall and Graham (1989), and Yun (2002) used OLS and fatalities per vehicle mile traveled as their dependent variables, while Khazzoom (1994), Noland (2004, 2005), and Vereeck and Vrolix (2007) used total fatalities as their dependent variables. However, three different estimating techniques were used by these researchers. Khazzoom used a ridge estimation model; Noland used a negative binomial model; and Vereeck and Vrolix used panel data estimation techniques.

Concerning possible statistical problems with their results, although several of the above studies used either time series data or panel data, none of them addressed the possibility of serial correlation; hence, as far as can be ascertained from the results presented, none tested for it, and none corrected for it. Regarding heteroskedasticity, most stated that either the use of logarithms or the use of panel data should mitigate its effects (Noland, 2005, p.2186). Finally, although several of the authors noted the possibility of multicollinearity, only Khazzoom believed that multicollinearity was a serious enough problem to address from a statistical standpoint; in order to correct this problem, he utilized the ridge estimation technique.

It is important to note that, in most of the prior research, little effort was made to develop a structural, individual-level model of vehicular safety. Most of the factors that affect highway safety are individual in nature; for example, people who drink and drive greatly reduce highway safety for not only themselves but also for their fellow travelers. Drinking and driving, however, is an individual-level decision, and no attempt was made in prior studies, nor is any made in the present study, to address the structural modeling of this individual-level decision. Instead, due to data limitations, most studies use aggregate state or national data to explain an individual-level phenomenon. Although this aggregate, reduced-form approach is less than ideal, it nonetheless is useful in providing indicators regarding the effectiveness of various public policy measures in reducing traffic fatalities.

EMPIRICAL TECHNIQUE

In examining the effects of traffic fatalities, one of the most important issues to be resolved is the way in which traffic fatalities, the dependent variable we are estimating, is defined. There are two general ways to do this: a fatality rate or a total fatality count. Fatality rates, however, can also be defined in several possible ways. The first fatality rate is the number of traffic fatalities per 100,000 population; this is a measure of the health risk associated with driving (Vereeck and Vrolix, 2007, p.386). A second way to define a fatality rate is to look at the traffic risk; the number of traffic fatalities per the total number of vehicles. Finally, the third way to define traffic fatalities is to look at the number of traffic fatalities per vehicle mile traveled. Vereek and Vrolix (2007) noted that this third measure is a much more accurate reflection of the true risk involved with driving. This third method will be the fatality rate employed in the present study.

In contrast to the use of fatality rates, however, one may use total fatalities as a dependent variable; this type of variable is a count variable since the total fatality data is a count of deaths due to traffic accidents. If one uses total fatalities, though, then one must take account of the total number of vehicle miles traveled.

In the present study, the fatality rate, as defined by fatalities per vehicle mile traveled, and total fatalities, the count variable, will be used as dependent variables. In order to estimate these dependent variables, three different empirical techniques will be examined: the first will be a fatality rate model using a least squares, fixed effects model; the second will be a total fatality model using a negative binomial, fixed effects model; and the third will be a total fatality model using a least squares, fixed effects model.

All three models will have almost all of the same explanatory variables; the only variable that will differ will be the exposure variable that was noted in Noland (2005) and Vereeck and Vrolix (2007). The reason for using almost all of the same explanatory variables is because one of the purposes of the present study is to compare the models to determine if the results obtained depend upon the type of model being used. Simply comparing the results of prior works would not achieve this since other researchers used a wide variety of explanatory variables in their traffic fatality models. The comparison of models in the present study should prove useful in determining the validity of the various empirical techniques, the veracity of the results obtained by prior researchers, and the robustness of any statistically-significant variables.

Regarding the common explanatory variables that will be used in all three empirical techniques, there are several different factors that may affect traffic fatalities. First, there are the regulatory constraints, the rules of the road (Peltzman, 1975). These include the following:

(1) speed limits (Vereeck and Vrolix, 2007; Ossiander and Cummings, 2002; Vernon, et al., 2003)
(2) mandatory seat belt laws (Cohen and Einav, 2001)
(3) minimum legal drinking ages (Vereeck and Vrolix, 2007; Bernhoft and Behrensdorff, 2003; Voas, et al., 2003; Sommers, 1985; Cook and Tauchen, 1984)
(4) minimum legal driving ages
(5) airbag requirement regulations.

Second, there are the roadway condition and vehicle characteristic variables:

(1) road maintenance (Noland and Oh, 2003)
(2) population density (Loeb, 1987)
(3) vehicles per capita (Noland, 2005)
(4) fuel efficiency (Noland, 2005; Noland, 2004; Khazzoom, 1994; Crandall and Graham, 1989).

Finally, there are personal characteristics of drivers in the state:

(1) age distribution (Noland, 2005; Noland, 2004; Yun, 2002; Khazzoom, 1994; Loeb, 1987)
(2) income (Noland, 2004; Yun, 2002; Khazzoom, 1994; Loeb, 1987)
(3) alcohol consumption (Vereeck and Vrolix, 2007; Noland, 2005; Noland, 2004; Voas, et al., 2003; Yun, 2002, Schechtman, et al., 1999).

Year is also included in order to capture linear trends in traffic fatalities not readily attributable to other factors. As noted, many of these explanatory variables have been used in prior studies. All variables used in the present study are defined in Table 1.

Given the above, the following three equations will be estimated in the present study:

$$\begin{aligned}FA = {} & a_0 + a_1 \ln AGE18 + a_2 \ln AGE25 + a_3 \ln AGE65 + a_4 \ln DENS + \\ & a_5 \ln SPEED + a_6 \ln DRINK + a_7 \ln DRIVE + a_8 \ln CAR + \\ & a_9 \ln TRUCK + a_{10} \ln VMT + a_{11}\, SEAT + a_{12} \ln INC + \\ & a_{13} \ln ALC + a_{14} \ln MAIN + a_{15}\, YEAR + a_{16} \ln MPG + a_{17}\, AIRBAG\end{aligned} \qquad (1)$$

Table 1 Variable Definitions	
Variable	Definition
Fatalities (FA)	Total traffic fatalities
Fatalities per vehicle mile traveled (FR)	Ratio of total traffic fatalities to vehicle miles traveled
AGE18	Percentage of state population between the ages 18 to 24
AGE25	Percentage of state population between the ages 25 to 34
AGE65	Percentage of state population older than 65
DENS	Ratio of population to area
SPEED	Speed limit on rural, limited access, highways
DRINK	Minimum legal age to drink alcoholic beverages
DRIVE	Minimum legal age to operate motor vehicle
CAR	Ratio of number of passenger cars to population
TRUCK	Ratio of number of light trucks to population
VMT	Vehicle miles traveled per capita
SEAT	Equals one if state has mandatory seat belt law and zero otherwise
INC	Per capita annual income in real dollars
ALC	Per capita ethanol consumption in gallons
MAIN	Per capita expenditures on state highway and road maintenance
MPG	Ratio of vehicle miles traveled to gallons of gasoline consumed
AIRBAG	Equals one for years airbags were mandated for new vehicles (1987) and zero otherwise.

$$\begin{aligned} \ln FR = {} & a_1 \ln AGE18 + a_2 \ln AGE25 + a_3 \ln AGE65 + a_4 \ln DENS + \\ & a_5 \ln SPEED + a_6 \ln DRINK + a_7 \ln DRIVE + a_8 \ln CAR + \\ & a_9 \ln TRUCK + a_{10}\, SEAT + a_{11} \ln INC + a_{12} \ln ALC + \\ & a_{13} \ln MAIN + a_{14}\, YEAR + a_{15} \ln MPG + a_{16}\, AIRBAG \end{aligned} \tag{2}$$

$$\begin{aligned} \ln FA = {} & a_0 + a_1 \ln AGE18 + a_2 \ln AGE25 + a_3 \ln AGE65 + a_4 \ln DENS + \\ & a_5 \ln SPEED + a_6 \ln DRINK + a_7 \ln DRIVE + a_8 \ln CAR + \\ & a_9 \ln TRUCK + a_{10} \ln VMT + a_{11}\, SEAT + a_{12} \ln INC + \\ & a_{13} \ln ALC + a_{14} \ln MAIN + a_{15}\, YEAR + a_{16} \ln MPG + a_{17}\, AIRBAG \end{aligned} \tag{3}$$

Equation (1), the negative binomial regression, is similar to the work done by Noland (2004, 2005); equation (2), the least squares fatality rate model, is based on Loeb (1987), Crandall and Graham (1989), and Yun (2002); and equation (3), the least squares total fatalities model, is similar to Vereeck and Vrolix (2007).

As noted earlier, each equation uses the same explanatory variables except for the traffic exposure variable needed in the total fatality regressions (equations (1) and (3)). The exposure variable used in the present study is vehicle miles traveled per capita (Noland, 2005). All explanatory variables, where appropriate, are expressed in terms of natural logarithms. The use of logarithms should mitigate any potential problems associated with heteroskedasticity. In addition, the use of a log-log model means that the estimated coefficients may be interpreted as elasticities, although it is important to note that the equations being estimated in the present study are reduced-form equations, and not structural equations. Thus, although the coefficients will not be true elasticities, they may be interpreted as such given the functional form of the equations estimated.

DATA AND RESULTS

Data on seat belt laws, implementation of air bag regulations, maintenance expenditures, and traffic fatalities were all obtained from various agencies within the US Department of Transportation. Data on the minimum legal age to consume alcoholic beverages, the minimum legal age to operate a motor vehicle, and the state speed limit were obtained from *The Book of the States*. Data on income, population, age distributions, and alcohol consumption were obtained from the *Statistical Abstract of the United States*.

Data on state level fuel efficiency were calculated as follows: data on state-level consumption of gasoline for motor vehicle use was obtained from the Energy Information Administration. This value was then divided by vehicle miles traveled. Thus, an estimate of the fuel efficiency of all vehicles on the road was obtained. A version of this methodology was also employed by Noland (2005, 2004). Although this value is not the CAFE standard, it is a reasonable estimate of the fuel efficiency of all vehicles on the road in a given state for a particular year. If CAFE alone were used as an explanatory variable, then the fuel efficiencies of the new model cars sold in a particular year would be accounted for, while the fuel efficiencies of older model cars would be ignored. Hence, this measure of fuel efficiency would be biased upwards and would not be an accurate representation of the true average fuel efficiency of all vehicles on the road.

Regarding AIRBAG, the federal government mandated that over a three year period starting in 1987 all new cars would be equipped with air bags. Hence, by 1990, all new cars would have this equipment. Of course, many older vehicles that did not have air bags were still on the road. Over the years, however, the percentage of vehicles on the road that did not have air bags dropped significantly. Nonetheless, it should be noted that this variable is a somewhat less-than-accurate proxy of air bag implementation.

For the variable SPEED, it is important to note that, although many drivers do not obey their state's posted speed limits, it is assumed that if a state has a higher speed limit, then drivers in that state will drive faster than in a state with a lower posted speed limit. Hence, SPEED is considered to be a reasonable proxy for the average vehicular speed in a state.

All data is state level and is for the years 1978 through 2000. Due to incomplete data, Hawaii and Alaska were excluded from the analysis. All dollar values were deflated using the Consumer Price Index, base year 1982-1984. The panel data set employed in the present study thus covers 48 states and 23 years; the total number of observations is 1104.

Serial correlation was found to be a problem for equations (2) and (3). A first-order autoregressive process was assumed, and the results were corrected. Multicollinearity was not found to be a serious problem; a correlation matrix was estimated, and it was found that the correlations among regressors were low. In addition, further evidence that multicollinearity was not a problem was the fact that the R^2s for equations (2) and (3) were relatively high, and most of the explanatory variables were statistically significant.

Descriptive statistics are presented in Table 2. Results for equations (1), (2), and (3) are presented on Tables 3, 4, and 5, respectively. In addition, the results for all three regressions are compared on Table 6.

Table 2
Descriptive Statistics
N=1104

Variable	Mean	Minimum	Maximum
FA	899	63	5542
FR	0.0234	0.0078	0.06
AGE18	0.112	0.077	0.167
AGE25	0.163	0.108	0.246
AGE65	0.121	0.075	0.186
DENS	165.9	0.64	1137
SPEED	60.5	55	75
DRINK	20.6	18	21
DRIVE	17.4	15	21
CAR	0.52	0.307	0.79
TRUCK	0.205	0.03	0.674
VMT	8714	4169	16376
SEAT	0.505	0	1
INC	13319	8060	23636
ALC	2.47	1.2	6.69
MAIN	0.074	0.0096	0.48
MPG	18.1	10.82	25.5
AIRBAG	0.434	0	1

Table 3: Negative Binomial Results Model (Equation (1))			
Variable	Coefficient	Standard Error	Test Statistic
Constant	38.11	4.21	9.057***
AGE18	0.171	0.0414	4.123***
AGE25	-0.169	0.0458	-3.683***
AGE65	-0.0536	0.0499	-1.073
DENS	0.421	0.0317	13.263***
SPEED	0.078	0.056	1.398
DRINK	-0.106	0.047	-2.259**
DRIVE	-0.258	0.0617	-4.184***
CAR	-0.326	0.0388	-8.414***
TRUCK	0.00092	0.0166	0.056
VMT	0.799	0.045	17.767***
SEAT	0.0321	0.0094	3.433***
INC	0.165	0.0464	3.545***
ALC	0.599	0.041	14.636***
MAIN	-0.035	0.0149	-2.343**
YEAR	-0.0159	0.0024	-6.657***
MPG	-0.377	0.0619	-6.099***
AIRBAG	-0.0764	0.0148	-5.174***
Log-Likelihood = -5781.542 Significant at 1% Level = *** Significant at 5% Level = ** Significant at 10% Level = *			

Table 4: Fatality Rate Model (Equation (2))			
Variable	Coefficient	Standard Error	Test Statistic
AGE18	0.174	0.0518	3.357***
AGE25	-0.0638	0.0449	-1.420
AGE65	0.0367	0.0632	0.582
DENS	-0.0967	0.0518	-1.867*
SPEED	0.016	0.06	0.265
DRINK	-0.115	0.0959	-1.194
DRIVE	-0.246	0.156	-1.572
CAR	-0.09	0.051	-1.761*
TRUCK	-0.0106	0.0183	-0.583
SEAT	0.0192	0.0109	1.774*
INC	0.182	0.0572	3.181***
ALC	0.742	0.0607	12.212***
MAIN	-0.0412	0.0143	-2.867**
YEAR	-0.0182	0.0025	-7.213***
MPG	-0.542	0.0515	-10.529***
AIRBAG	-0.0378	0.0126	-3.008***
$R^2 = 0.93$ F = 221.36 Significant at 1% Level = *** Significant at 5% Level = ** Significant at 10% Level = *			

Table 5: Total Fatality Least Squares Model (Equation (3))			
Variable	Coefficient	Standard Error	Test Statistic
AGE18	0.194	0.0528	3.661***
AGE25	-0.0496	0.0455	-1.091
AGE65	0.0486	0.0634	0.766
DENS	0.861	0.0564	15.27***
SPEED	0.055	0.0637	0.862
DRINK	-0.111	0.0958	-1.164
DRIVE	-0.241	0.156	-1.544
CAR	-0.089	0.051	-1.743*
TRUCK	-0.0046	0.0186	-0.246
VMT	0.869	0.069	12.424***
SEAT	0.0213	0.0109	1.956*
INC	0.239	0.0647	3.688***
ALC	0.748	0.0607	12.312***
MAIN	-0.0394	0.0144	-2.739***
YEAR	-0.0183	0.0025	-7.287***
MPG	-0.474	0.063	-7.506***
AIRBAG	-0.0316	0.0129	-2.44**
R^2 = 0.99 F = 1797.03 Significant at 1% Level = *** Significant at 5% Level = ** Significant at 10% Level = *			

Table 6: Comparison of Models			
Variable	Negative Binomial	Fatality Rate	Total Fatality, Least Squares
AGE18	+	+	+
AGE25	-	?	?
AGE65	?	?	?
DENS	+	-	+
SPEED	?	?	?
DRINK	-	?	?
DRIVE	-	?	?
CAR	-	-	-
TRUCK	?	?	?
VMT	+	N/A	+
SEAT	+	+	+
INC	+	+	+
ALC	+	+	+
MAIN	-	-	-
YEAR	-	-	-
MPG	-	-	-
AIRBAG	-	-	-
Significant at 10% Level or Less and Positive: + Significant at 10% Level or Less and Negative: - Insignificant: ?			

The results of all three models indicate that fuel efficiency has a statistically significant and negative effect on traffic fatalities at the state level; given that the results of all three models are the same, this result would be considered robust. This result runs counter to the results of most of the prior research. In addition, this result is important, since, given the recent dramatic increases in oil prices, any increase in fuel efficiency standards should not result in greater traffic fatalities.

Regarding the other explanatory variables, out of fifteen, excluding MPG, that were common to all three models, the following eight were significant with the same sign: AGE18, CAR, SEAT, INC, ALC, MAIN, YEAR, and AIRBAG. AGE18, SEAT, INC, and ALC were positive, while the others were negative; all of these results would be considered to be robust.

The model that had the most significant explanatory variables was the negative binomial regression (only three out of seventeen were insignificant). The model with the least number of variables significant was the least squares total fatalities model (six out of seventeen were insignificant). Only three variables were insignificant in all three models (AGE65, SPEED, and TRUCK). This comparison of the models suggests that most variables are robust, with the only inconsistencies being with the legal variables. Hence, use of any of the three most common empirical models should provide one with similar results, with the exception of the legal restrictions. Unfortunately, the effect of legal restrictions on traffic fatalities is a popular research topic, and few researchers utilize multiple models in order to determine if their results are robust.

As noted earlier, since a logarithmic or log-log model is used, the estimated coefficients for equations (2) and (3) may be interpreted as elasticities. Hence, regarding the result for MPG, for equation (2), the fatality rate model, a ten percent increase in fuel efficiency would reduce the fatality rate by 5.42 percent. For the total fatality model, a ten percent increase in fuel efficiency would reduce traffic fatalities by 4.74 percent. Although these elasticities are inelastic, they are nonetheless significant and suggest that increasing fuel efficiency would reduce traffic fatality by a significant amount. For example, at a fuel efficiency of 25 miles per gallon, an increase in this efficiency to only 27.5 would result in 263 fewer fatalities on average at the state level.

Regarding other factor elasticities, most are very small in magnitude. There are two explanatory variables, however, that have rather large elasticities. The first is ALC. A ten percent increase in per capita alcohol consumption would result in a 7.48 percent increase in total fatalities and a 7.42 percent increase in the fatality rate. Hence alcohol consumption has a statistically-significant and large effect on traffic fatalities, which is not unexpected. The second variable is VMT; for every ten percent increase in VMT, traffic fatalities increase by 8.69 percent. Since it is reasonable to assume that more auto accidents will occur if people drive more, this result is also not unexpected.

Most of the results of the present study are reasonable and consistent with prior research. These results suggest that road conditions, driver attributes, and vehicle characteristics are the most important determinants of traffic fatalities at the state level, irrespective of the type of empirical model used.

The one result not consistent with prior research was the positive sign on SEAT; this result suggests that the implementation of mandatory seat belt laws resulted in an increase in traffic fatalities. One possible reason for this result is that those individuals who most likely would wear seat belts were already using them; the law had no effect on their behavior. The results of the present study suggest that the mandatory seat belt laws may have been less than effective in convincing others to wear their seat belts.

Most importantly, however, several legal restrictions were insignificant in one or more of the models. SPEED, a measure of speed limits, was insignificant in all three models. The minimum legal drinking age and the minimum legal driving age were insignificant in two of the models. These results are consistent with the results of prior research. Vereeck and Vrolix (2007) found that most of the legal restrictions on driving that they examined were statistically insignificant; they noted that this confirmed their hypothesis that traffic laws could not explain the international differences in traffic fatalities. Apparently differences in laws cannot explain interstate differences in traffic fatalities either.

Conclusion

The purpose of the present study was to ascertain the determinants of state-level traffic fatalities and to compare the three most common models used in such research. Using data from 48 states over a 23 year time period, results of the present study suggested that fuel efficiency has a statistically significant and negative effect on traffic fatalities at the state level. This result, which is consistent across all three models and thus robust, runs counter to the results of most of the prior research. Holding all else constant, it appears that increases in the fuel efficiency of vehicles actually reduces traffic fatalities.

The results of the present study also suggest that road conditions, driver attributes, and vehicle characteristics are the most important determinants of traffic fatalities at the state level, irrespective of the type of empirical model used. Several legal restrictions were insignificant in one or more of the models. Thus, differences in laws cannot adequately explain the interstate differences in traffic fatalities. It appears that traffic fatalities are most affected by factors that policymakers have little control over, such as alcohol consumption and the percentage of drivers who are young; laws appear to have limited effects on driving behavior and ultimately the safety of driving on America’s roads.

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The Determinants of Scoring in NFL Games and Beating the Over/Under Line

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Abstract

In this paper we attempt to predict the total points scored in National Football League (NFL) games for the 2005-2006 season. Separate regression equations are identified for predicting points for the home and away teams in individual games based on information known prior to the games. The predictions from the regression equations (updated weekly) are then compared to the over/under line on individual NFL games in a wagering experiment to determine if a successful betting strategy can be identified. All predictions in this paper are out-of-sample. Using this methodology, we find that several successful wagering procedures could have been applied to the 2005-2006 NFL season. We also estimate a single equation to predict the over/under line for individual games. That is, we test to see if the variables we have collected and formulated are important in predicting the line for NFL games.

I. Introduction

Bookmakers set over/under lines for virtually all NFL games. Suppose the over/under line for total points in a particular game is 40. Suppose further that a gambler wagers with the bookmaker that the actual points scored in the game will exceed 40, that is, he bets the "over." If the teams then score more than 40 points, the gambler wins the wager. If the teams score under 40 points, the gambler loses the bet. If the teams score exactly 40 points, the wager is tied and no money changes hands. The process works symmetrically for bets that the teams will score fewer than 40 points, or betting the "under." The over/under line differs, of course, on individual games. Since losing bets pay a premium (often called the "vigorish," "vig," or "juice" and typically equal to 10 percent of the wagered amount), the bookmakers will profit as long the money bet on the "over" is approximately equal to the amount of money bet on the "under" (bookmakers also sometimes "take a position," that is, will welcome unbalanced bets from the public if the bookmaker has strong feelings regarding the outcome of the wager. See Levitt (2002). It is widely known a gambler must win 52.4 percent of wagers to be successful. That particular calculation can be established simply. Let P_w be the proportion of winning bets and $(1 – P_w)$ the proportion of losing

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bets. The equation for breaking even where every winning wager nets $10 and each losing wager represents a loss of $11 is:

$P_w(\$10) = (1 - P_w)(\$11)$,

and solving for P_w,

$P_w = 11/21 = .5238$, or approximately 52.4 percent.

This research attempts to identify methods for predicting the total points scored in a particular game based on information available prior to that game. The primary research question is whether or not these methods can then be utilized to formulate a successful gambling strategy for the over/under wager.

The remainder of this paper is organized as follows: in section II we describe the efficient markets hypothesis as it applies to the NFL wagering market; section III offers a brief review of the literature; in section IV we describe the data and method; section V provides descriptive statistics and the main regression results; section VI presents the wagering simulations; section VII contains our investigation of the determinants of the over/under line; and the final section offers conclusions.

II. NFL Betting as a Test of the Efficient Markets Hypothesis

A number of important papers have treated wagering on NFL games as a test of the Efficient Market Hypothesis (EMH). This hypothesis has been widely studied in economics and finance, often with focus on either stock prices or foreign exchange markets. Because of the difficulties of capturing EMH conclusions given the complexities of those markets, some researchers have turned to the simpler betting markets, including sports (and the NFL) as a vehicle for such tests.

If the EMH holds, asset prices are formed on the basis of all information. If true, then the historical time series of such asset prices would not provide information that would allow investors to outperform the naïve strategy of buy-and-hold. See, for example, Vergin (2001). As applied to NFL betting, if the use of past performance information on NFL teams cannot generate a betting strategy that would exceed the 52.4 percent win criterion, the EMH hypothesis holds for this market. Thus, the thrust of much of the research on the NFL has taken the form of attempts to find winning betting strategies, that is, strategies that violate the weak form of the EMH.

III. A Brief Review of the Recent Literature

Nearly all of the extant literature on NFL betting uses the point "spread" as the wager of interest. The spread is the number of points by which one team (the favorite) is favored over the opponent (the underdog). Suppose team A is favored over team B by 7 points. A wager on team A is successful only if team A wins by more than 7 points (also known as "covering" the spread). Symmetrically, a wager on team B is successful if team B loses by fewer than 7 points or, of course, team B wins or ties the game—

in any of these cases, team B "covers". Vergin (2001) and Gray and Gray (1997) are examples of research that focuses on the spread.

Based on NFL games from 1976 to 1994, Gray and Gray (1997) find some evidence that the betting spread is not an unbiased predictor of the actual point spread on NFL games. They argue that the spread underestimates home team advantage, and overstates the favorite's advantage. They further find that teams that have performed well against the spread in recent games are less likely to cover in the current game, and teams that have performed poorly in recent games against the spread are more likely to cover in the current game. Further Gray and Gray find that teams with better season-long win percentages versus the spread (at a given point in the season) are more likely to beat the spread in the current game. In general, they conclude that bettors value current information too highly, and conversely place too little value on longer term performance. That conclusion is congruent with some stock market momentum/contrarian views on stock performance. Gray and Gray then use the information to generate probit regression models to predict the probability that a team will cover the spread. Gray and Gray find several strategies that would beat the 52.4 percent win percentage in out-of-sample experiments (along with some inconsistencies). They also point out that some of the advantages in wagering strategies tend to dissipate over time.

Vergin (2001), using data from the 1981-1995 seasons, considers 11 different betting strategies based on presumed bettor overreaction to the most recent performance and outstanding positive performance. He finds that bettors do indeed overreact to outstanding positive performance and recent information, but that bettors do not overreact to outstanding negative performance. Vergin suggests that bettors can use such information to their advantage in making wagers, but warns that the market may adjust and therefore this pattern may not hold for the future.

A paper by Paul and Weinbach (2002) is a departure from the analysis of the spread in NFL games. They (as do we in this paper) target the over/under wager, constructing simple betting rules in a search for profitable betting strategies. These authors posit that rooting for high scores is more attractive than rooting for low scores. Ceteris paribus, then, bettors would be more likely to choose "over" bets. Paul and Weinbach show that from 1979-2000, the under bet won 51 percent of all games. When the over/under line was high (exceeded the mean), the under bet won with increasing frequency. For example, when the line exceeded 47.5 points, the under bet was successful in 58.7 percent of the games. This result can be interpreted as a violation of the EMH at least with respect to the over/under line.

Levitt (2002) approaches the efficiency question from a different perspective. It is clear that if NFL bets are balanced, the bookmaker will profit by collecting $11 for each $10 paid out. As we suggested earlier, bookmakers at times take a "position" on the assumption that they know more (or think they do) about a particular wager than the bettors. Levitt presents evidence that the spread on games is not set according to market efficiency. For example, using data from the 2001-2002 seasons, he shows that home underdogs beat the spread in 58 percent of the games, and twice as much was bet on the visiting favorites. Bookmakers did not “move the line” to balance these bets, thus increasing their profits as the visiting favorite failed to cover in 58 percent of the cases.

Dare and Holland (2004) re-specify work by Dare and MacDonald (1996) and Gray and Gray (1997) and find no evidence of the momentum effect suggested by Gray and Gray, and some, but less, evidence of the home underdog bias that has been consistently pointed out as a violation of the EMH. Dare and Holland ultimately conclude that the bias they find is too small to reject a null hypothesis of efficient markets, and also that the bias may be too small to exploit in a gambling framework.

Still more recently, Borghesi (2007) analyzes NFL spreads in terms of game day weather conditions. He finds that game day temperatures affect performance, especially for home teams playing in the coldest temperatures. These teams outperform expectations in part because the opponents were adversely acclimatized (for example, a warm weather team visiting a cold weather team). Borghesi shows this bias persists even after controlling for the home underdog advantage.

IV. The Current Project: Data and Method

This project differs from the extant literature in several ways. First, we focus on the over/under wager. The vast majority of previous work relates to the spread on NFL games. Second, virtually all prior work employs betting rules that are commonly referred to in the literature as “naïve” strategies. For example, betting the home team against the spread when they are the underdog is a simple rule, or naïve strategy. Third, our method is not backward looking—for example, looking back through prior NFL seasons to test the efficiency of the over/under wager. Our method generates “out-of-sample” predictions for each week of the season and tests those predictions against the outcomes. Finally, we also offer evidence on the determinants of the line on NFL games.

The paper by Paul and Weinbach (2002) is a departure from the norm in that they focus on the over/under bet. They, however, like much of this literature also employ a simple rule, suggesting that betting the “under” can be a successful strategy in some circumstances, because bettors prefer to wager that teams will score many points. In this paper, in contrast to prior work, we attempt to formulate regression equations to model points for the home and away teams for specific team match-ups, and to use the sum of predictions from the regression equations for comparison to the over/under. Our modeling method produces out-of-sample symmetrical wagering opportunities, since our predictions for total points suggest wagering the “over” in some cases and the “under” in others. To our knowledge, no prior work attempts to use this type of econometric modeling as a guide to wagering strategies.

With the objective of estimating regression equations for home and away team scoring, data were gathered for the 2005-06 season. The data were collected from the NFL website (www.NFL.com), the Super NFL website (www.supernfl.com), and *The Richmond Times-Dispatch* newspaper. The variables include:

TP = total points scored for the home and visiting teams for each game played

PO = passing offense in yards per game

RO = rushing offense in yards per game

PD = passing defense in yards per game

RD = rushing defense in yards per game

D = a dummy variable equal to 1 if the game is played in a dome, 0 otherwise

PP = points scored by a given team in their prior game

L = the over/under betting line on the game.

1. Match-ups Matter

The general regression format is based on the assumption that "match ups" are important in determining points scored in individual games. For example, if the team "A" with the best passing offense is playing the team "B" with the worst passing defense, ceteris paribus, team "A" would be expected to score many points. Similarly, a team with a very good rushing defense would be expected to allow relatively few points to a team with a poor rushing offense. In accord with this rationale, we formed the following variables:

PY = PO + PD = passing yards

RY = RO + RD = rushing yards.

For example, suppose team "A" is averaging 325 yards (that's high) per game in passing offense and is playing team "B" which is giving up 330 yards (also, of course, high) per game in passing defense. The total of 655 yards, comprised of the excellent passing offense for one team and relatively poor passing defense for the opponent, should indicate an advantage for team "A". Such an advantage is likely to lead to more scoring, at least in that phase of the game.

The dome variable will be a check to see if teams score more (or fewer) points if the game is played indoors, which eliminates weather as a factor.

The variable for points scored in the prior game (PP) is intended to check for streakiness in scoring. That is, if a team scores many (or few) points in a given game, are they likely to have a similar performance in the ensuing game?

We also test to ascertain whether or not scoring is contagious. That is, if a given team scores many (or few) points, is the other team likely to score many (or few) points as well? We test for this by two-stage least squares regressions in which the predicted points scored by each team serve as explanatory variables in the companion equation.

2. General Regression Equations

The general sets of regressions are of the form:

$$TP_{hi} = \beta_0 + \beta_1(PY_{hi}) + \beta_2(RY_{hi}) + \beta_3(D_i) + \beta_4(PP_{hi}) + \varepsilon_{hi} \quad (1)$$

and

$$TP_{vi} = \alpha_0 + \alpha_1(PY_{vi}) + \alpha_2(RY_{vi}) + \alpha_3(D_i) + \alpha_4(PP_{vi}) + \varepsilon_{vi}\ , \quad (2)$$

where the subscripts h and v refer to the home and visiting teams respectively, and the i subscript indicates a particular game.

Equations such as 1 and 2 are estimated using data for weeks 5 through 17 of the 2005-06 season. We choose to wait until week five to begin the estimations so that statistics on offense and defense are more reliable than would be the case for earlier weeks. For example, a team might have two exceptional games against poor opponents that would bias early season statistics in a positive way. Such aberrations will average out as more games are played.

V. Descriptive Statistics and Regression Results

1. Descriptive Statistics

Table I contains some summary statistics for the data set. Teams averaged approximately 211 yards passing per game (offense or defense, of course) for the season, and they averaged approximately 109 yards rushing. The statistics reported on the rushing and passing standard deviations without parentheses are for the offenses and the defensive standard deviations are in parentheses. Interestingly, both passing defense and rushing defense are less variable across teams than are the offensive counterparts. We hypothesize that teams must be more balanced on defense to keep other teams from exploiting an obvious defensive weakness, but teams may be relatively unbalanced offensively and still be successful. (The 2007 Patriots would be an example.) Home teams scored approximately 23 points on average for the season and outscored the visitors by about three points. Total points averaged 41.8 in 2005-2006 and the over/under line averaged 40.8. The difference between these means is not statistically significant; the calculated value for the t-test of paired samples is approximately 1. Not surprisingly, the standard deviation was much smaller for the line than for total points.

Table I: Summary Statistics

Variable	Mean	Standard Deviation
Passing Yards	211.4	53.9 (44.3)
Rushing Yards	109.3	29.3 (23.2)
Visitor Points	19.4	10.1
Home Points	22.5	9.7
Total Points	41.8	13.1
Line	40.8	5.0

2. Regression Results

In the estimations of equations 1 and 2, we find no role played by points scored in the prior week and thus we do not report regressions with that variable included. These estimations are available from the authors upon request. The estimated equations (at the end of the 16th week) are given in Table II.

Table II: Regression Results for Total Points Scored

Explanatory Variable	Dependent Variable = TP_{hi}	Dependent Variable = TP_{vi}
Intercept	-9.13 (-1.20)	-22.87 (-2.93)
PY_{hi}	0.0248** (2.00)	
RY_{hi}	0.0948* (4.55)	
PY_{vi}		0.038* (3.18)
RY_{vi}		0.115* (5.17)
D_i	1.66 (0.88)	3.76** (1.98)
$\overline{R}^2$	0.102	0.115
SEE	9.22	9.34
Observations	178	178
F-statistic	7.74*	11.85*

(The numbers in parentheses are t-statistics)
** represents significance at the 95 percent level of confidence or better and * represents significance at the 99 percent level of confidence or better for one-tailed tests

For the home points equation, the dome effect is not statistically significant, but the passing yardage and the rushing yardage are significant at the 95 and 99 percent confidence levels, respectively. The equation explains a modest 10.2 percent of the variance in home points scored. On the other hand, the F-statistic indicates that the overall equation meets the test of significance at the 99 percent level of confidence. The estimated coefficients for all of the variables have the anticipated signs. To interpret those coefficients, an additional 100 yards passing (recall that this is the sum of the home team's passing offense and the visitor's passing defense) implies approximately 2.5 additional points for the home team, whereas an additional 100 yards rushing implies approximately 9.5 additional points.

The visiting team estimation yields a very slightly better fitting equation. All explanatory variables are statistically significant—the yardage variables are each significant at the 99 percent level of confidence, and the dome dummy variable is significant at the 95 percent level. The equation explains almost 12 percent of the variance in visiting team points, and the F-statistic implies overall significance far greater than the 99 percent level of confidence. The coefficients for passing and rushing suggest a greater effect for the visiting team than the home team. The coefficients imply that an additional 100 yards passing yields approximately 3.8 points for the visiting team, and an additional 100 yards rushing is worth 11.5 points. The dome effect implies that the visiting team scores almost 4 additional points in indoor games. The combined dome effect suggests that (ceteris paribus) approximately 5.4 additional points are scored per game in domed stadiums. In fact, for the 32 games in our sample played in domes, the mean number of points was 46.34, whereas the mean was 40.91 for the 162 outdoor games. That difference, 5.43 points, is nearly identical to that predicted by our two equations. A t-test for different mean points scored between domed stadiums and outdoor stadiums is statistically significant at greater than the 95 percent level of confidence.

3. Other Hypotheses

Another hypothesis we wished to test is whether scoring is contagious. A priori, we surmised that points scored in given games for visiting and home teams would be positively related. This conjecture does not look promising. The estimated simple correlation coefficient between home team and visiting team points is -0.106, which is not statistically different from zero and has the "wrong" sign according to our intuition. Our initial thinking was that if team "A" scores and perhaps takes a lead, team "B" has a greater incentive to score. An obviating factor to this line of reasoning is that a given team may dominate time of possession, thus denying the opposing team opportunities to score. We also experimented with two-stage least squares to test the hypothesis that scoring was contagious. In this formulation we developed a "predicted points" variable for the home team, entered that variable as an independent variable in the visiting team equation, and reversed the procedure for the home team equation. Neither of the predicted points variables were statistically significant. The variable was positively signed for the home team equation, and negatively signed for the away team equation.

As indicated above, we find no evidence that teams are "streaky" with respect to points scored. In short, we find that points scored in the preceding week do not contribute to the explanation of points scored in the current week. That conclusion holds up for the regressions in section VI as well.

VI. Wagering on the Over/Under Line

In this simulated wagering project we use the estimated equations to predict scores of the home and away teams for all of the games played over weeks 8 through week 17 (end of the regular season). The points predicted in this manner are then compared to the over/under line for each game. We then simulate betting strategies on those games.

1. Out-of-Sample Method

Since it is widely known that betting strategies that yield profitable results "in sample," are often failures in "out-of-sample" simulations, we use a sequentially updating regression technique for each week of games. That is, we estimate equations TP_{hi} and TP_{vi} with the data from weeks 5, 6, and 7, then "feed" those equations with the known data for each game through the end of week 7, generating predicted points for the visiting and home team for all individual games in week 8. The predicted points are then totaled and compared to the over/under line for each game. Next we add the data from week 8, re-estimate equations TP_{hi} and TP_{vi}, and make predictions for week 9. The same updating procedure is then used to generate predictions for weeks 10 through 17. This method ensures that our results are not tainted with in-sample bias.

2. Betting Strategies

We entertain four betting strategies for the predicted points versus the over/under line on the games. These strategies are:

1. Bet only games for which our predicted total points differ from the line by more than 10 points. (For example, if the line is 40 and the model predicts more than 50, a simulated bet would be placed on the over. With the same line, if the model predicted less than 30, the bet would be on the under.)
2. Bet only games for which our predicted total points differ from the line by more than 7 points.
3. Bet only games for which our predicted total points differ from the line by more than 5 points.
4. Bet all games for which our predicted total points differ from the line by any amount—in our case, all games.

As stated previously, a betting strategy on such games must predict correctly at least 52.4 percent of the time to be successful.

Table III contains a summary of the results for the four betting strategies. The first betting strategy yields only nine "plays" over weeks 8 to 17. That betting strategy would have produced five wins, three losses, and one tie. For this small sample this strategy is profitable, with a 62.5 winning percentage. The second strategy (a differential greater than 7 points) yields 21 plays and a record of 11-9-1—a winning percentage of 55. The 5 point strategy yields more action, 39 bets and a 60.5 percent success rate. Finally for every game played, the method produces a still profitable record of 80-68-4 (notice that two games were eliminated from the full sample, because there were no lines for those games), with the winning percentage at 54.

While we do not find the winning results surprising for the 10 and 7 point criteria, we were surprised to find a winning percentage at lesser differentials, especially the strong performance of the model at the 5 point differential.

Table III: Results of Different Betting Strategies

Betting Strategy (Differential)	*Games "Played"*	*W-L-T Record*	*Win Percentage*
> 10 points	9	5-3-1	62.5
> 7 points	21	11-9-1	55.0
> 5 points	39	23-15-1	60.5
All Games	152	80-68-4	54.0

An important question is whether results for a single season will be robust over future seasons. Indeed, certain biases can disappear over time as bettors and bookmakers incorporate the knowledge of historical biases into their behavior (e.g., bettors with knowledge of the home team underdog bias may choose to wager more often on those teams). We suggest that the information used in this paper is less easily incorporated into market behavior, making the likelihood greater that the model will perform well in future seasons. Simulating the results of this paper requires weekly data collection as well as statistical modeling. Most bettors and bookmakers are unlikely to engage in such work.

Note also that we make no adjustment for injuries, weather, and the like that would be considered by those who make other than simulated wagers. We offer these methods only as a guide, not as a final strategy.

VII. Another Method of Predicting the Line and Total Points

Since we have collected and created variables that may be relevant to determining the betting line (and total points), in this section we investigate the relevance of our variables in this context. For purposes of comparison, we estimate an equation for the over/under line and, separately, for the actual points scored. These equations may be useful in confirming (or contradicting) the results of the previous sections, and may provide useful information applicable to wagering strategies.

The results of those regressions are contained in Table IV. We estimated a regression equation with the line as the dependent variable and all of the right-hand side variables specified in equations 1 and 2. Every coefficient estimate is correctly signed, statistically significant, and $\overline{R}^2 = .671$. As a comparison, we also estimated (far less successfully) an equation for total points with the same set of explanatory variables.

Perhaps the most striking result of these regressions is that the regression for the line explains fully two-thirds of the variance in that dependent variable and the equation for the actual points explains only 5.2 percent of the variance in total points, with only four of the seven explanatory variables meeting the test for statistical significance at traditional levels. The F-test for overall significance of the equation for total points does indicate, however, that a significant portion of the variance in the dependent variable is explained by the regression equation.

Table IV: Regression Results for the Line and Total Points

Explanatory Variable	Dependent Variable = Line	Dependent Variable = Total Points
Intercept	-21.03 (-5.29)	-10.58 (-0.59)
PY_h	0.0476* (12.10)	0.0166 (0.94)
RY_h	0.0507* (6.87)	0.0559** (1.69)
PY_v	0.0442* (11.52)	0.0376** (2.18)
RY_v	0.0450* (5.93)	0.0576** (1.69)
PP_v	0.0669* (2.86)	0.0766 (0.73)
PP_h	0.0343 (1.53)	0.1100 (1.09)
D	2.21* (4.01)	5.17** (2.09)
$\overline{R}^2$	0.671	0.052
SEE	2.84	12.76
Observations	194	194
F-statistic	57.2*	2.51**

(The numbers in parentheses are t-statistics)
** represents significance at the 95 percent level of confidence or better and * represents significance at the 99 percent level of confidence or better for one-tailed tests

In short, the line is, as expected, much easier to predict than actual points scored. That is, the outcomes of the games and points scored are not easily predicted, which is "why they play the games." At least two further observations are in order. First, consider the coefficients for points scored in the previous game. Those variables matter in determining the line for the game. However, they seem to play an insignificant (statistical or practical) role in determining the actual points scored. This particular result may mean that bettors place too much emphasis on recent information, as other authors have suggested. Finally note that the dome effect (based on the magnitude of the coefficient estimates) is weaker for the line equation than in the equation for total points. In fact, the effect for total points is approximately equal to the sum of those effects for equations TP_{hi} and TP_{vi} in section IV. Based on these results, a tentative conclusion might be that bettors underestimate the dome effect.

VIII. Summary and Conclusions

The regression results in this paper identify promising estimating equations for points scored by the home and away teams in individual games based on information known prior to the games. In a regression framework, we apply the model to four simulated betting procedures for NFL games during weeks 8 through 17 of the 2005-2006 season. Betting strategies based on four differentials between our predictions and the over/under line each produced winning results for the season. The relevant question is, of course, whether these results will hold up in future seasons.

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The Effects of Dropping a Grade in Intermediate Macroeconomics

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ABSTRACT

When preparing a course at the start of a semester, instructors must consider how students will be assessed. One commonly used approach is to allow students to drop their lowest grade on an assignment or test. However, the effect of this policy is debatable.

This study adapts the model used by Sewell (2004) to investigate student performance in Intermediate Macroeconomics over six semesters at a public Midwestern university. Allowing students to drop their lowest test score does not appear to artificially inflate their final grade in class. Performance in previous economics courses, overall GPA and class status are strong predictors of the final grade.

This grading approach does lead to strategic test-taking on the part of students. Some choose not to take an optional end-of-semester exam that can potentially raise their final grade. Probit analysis shows this decision is positively related to the student's score going in to the exam, their concurrent course load and the variance in their prior test performance. Surprisingly, it is not related to the minimum score needed to raise their final grade.

INTRODUCTION

Faculty members face a question at the beginning of each semester as to how they will assess students. One issue is how to deal with students who miss an exam. There are several common approaches, each with its challenges.[1]

One approach is to allow a make-up exam. However, it is difficult to ensure that a new exam is comparable to the original in construction or grading. At the same time, if the student is allowed to take the original test, there is no way to be sure how much the student learned from those who took it at the scheduled time. A second approach is to reweight the exams that the student did take. However, this is problematic since the student is now being assessed differently than his or her peers. A third approach, and the focus of this study, is to allow all students to drop their lowest score.[2] This may relieve the instructor of the responsibility of make-up exams. In addition, this policy allows for a nice response to the dreaded question: "Can you curve the grades?" A concern with this approach is the potential impact on the student's grade. Many are concerned that such a policy may artificially inflate grades. However, is this

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the case? Does allowing students to drop an exam lead to higher final grades when compared with other students who are not permitted to drop their lowest exam score? This approach as well as the re-weighting approach also suffers from the potential for a student to miss out on material completely if there is no comprehensive final examination.

Sewell (2004) offers up the only research related to this question.[3] In a single semester of Introductory Microeconomics, two sections were allowed to drop their lowest score while two were not.[4] The author finds the student's performance on a comprehensive final exam is positively related to SAT score, class status (upperclassmen fare better), gender (male) and major (business majors perform better). Of particular interest, allowing students to drop their lowest test score had a negative impact on the comprehensive final.

The current study follows the approach implemented in Sewell (2004). However, there are key differences between the studies. This study investigates the impact of dropping the lowest exam on the course grade using six semesters of Intermediate Macroeconomics with a single section in each semester. In contrast, Sewell (2004) investigated the impact of dropping the lowest exam score on the comprehensive final exam using four sections in a single semester of Introductory Microeconomics.

This study does not find that grade dropping has a significant effect on the final course grade nor does it appear to lead to artificially higher grades. The final grade is affected by overall GPA, the grade in Principles of Macroeconomics and class year. This grading approach leads to strategic test taking where students who have an opportunity to raise their grade through an optional fourth exam may decide not to take it. The likelihood of taking this fourth exam is positively related to the student's course load, grade going in to the final and variance in their prior test performance. Surprisingly, it is not related to the improvement required to raise their final grade.

DATA

This study uses data collected from six classes over four years.[5] The data on performance in Intermediate Macro were gathered by the instructor. The remaining data on demographics and overall academic performance were provided by the Registrar. A summary of student data is available in Table I.

There were 180 students in the six classes. Eighty-one percent of the students were white while eight percent were black, three percent Asian and two percent Hispanic. This matches closely with the demographics of the university (white – eighty percent; African-American – six percent, Hispanic – four percent, Asian – one percent). Women accounted for only twenty-three percent of the students. This is significantly less than the university (forty-nine percent). However, women are commonly underrepresented in an upper level economics course.[6] These demographics are consistent across the two assessment styles (Drop vs. No-Drop). The majority of students in the class were seniors (sixty-one percent). However, some juniors (thirty-one percent) and graduate students (eight percent) as well as a single sophomore enrolled in the course over the four years. There were no freshmen.

Table I. Descriptive Statistics - Students

	Full Sample	No Drop	Drop
Number of Students	180	46	134
Female	42	10	32
Male	138	36	102
White	145	35	110
African-American	14	5	9
Asian	6	2	4
LatinAmerican	3	1	2
Other	12	3	9
Freshman	0	0	0
Sophomore	1	0	1
Junior	55	12	43
Senior	110	28	82
Graduate Student	14	6	8
Introductory Macro grade (4 point scale)	3.14	3.05	3.17
	(0.83)	(0.93)	(0.80)
Concurrent course load	14.78	14.46	14.89
	(2.55)	(2.55)	(2.55)
GPA (4 point scale)	2.96	3.04	2.93
	(0.68)	(0.60)	(0.71)
Econ majors	75	23	52
Other business major	66	14	52
Non-business major	39	9	30

Standard deviations are included below in parenthese, where appropriate.

Since this class was required for Economics majors, it is not surprising that they made up a large portion of the class (forty-two percent). Other Business majors such as Accounting, Marketing, Management, Finance, Human Resource Management and Information Management took the class (thirty-six percent). In addition, non-Business majors such as Political Science, Journalism, Math, History and Computer Science were enrolled (twenty-one percent).

Each semester, a single section was offered. Since there was a single section per semester, the sample is truly randomized. However, the sample may vary over time. Class size ranged from a low of twenty-four to a high of thirty-seven students. All six sections were taught by the same instructor.

Over the six semesters, the grading scheme varied. For two of the classes, the final grade was the average of four exams. For the remaining four classes, the final grade was the average of the three best of four exams (seventy-five percent) in addition to homework assignments (twenty percent) and a class participation grade (five percent). In both cases the final exam was simply the fourth exam and was not cumulative.[7] In the classes where a grade was dropped, no make-ups were offered. In the other sections, make-ups were possible.

A summary of the test performance is found in Table II. The average final grade across all sections was a 74.9 out of 100 total points. Surprisingly, it is slightly higher for the classes where all exams counted, 76.2, than for those allowed to drop the lowest exam, 74.5.[8]

REGRESSION EQUATION AND RESULTS

To test the impact of the different grading schemes on final grades, the following regression equation is applied.[9]

$$\text{FinalGrade} = f\begin{pmatrix} \text{Drop, GPA, Grade in Principles, Course Load,} \\ \text{Business Major, Non-Business Major} \\ \text{Female, Black, Asian, Hispanic} \\ \text{Sophomore, Junior, Grad} \end{pmatrix}$$

Table II. Descriptive Statistics - Test Scores

	Full Sample	No Drop	Drop
Final grade	74.9	76.2	74.5
	(20.9)	(14.8)	(22.7)
Final grade (4 point scale)	2.56	2.57	2.55
	(1.27)	(1.22)	(1.28)
Test 1 results	77.2	72.7	78.7
	(15.2)	(14.1)	(15.3)
Test 2 results	73.4	71.5	74.1
	(19.1)	(17.0)	(19.8)
Test 3 results	70.3	67.3	71.4
	(19.1)	(19.4)	(19.0)
Test 4 results	70.4	74.4	67.5
	(17.4)	(16.4)	(17.7)

All scores are in percent except Final grade (4 point scale)
Standard deviations are included below.

Variable	Definition
Drop	= 1 if enrolled in section where the lowest grade is dropped.
GPA	the student's overall grade point average on a 4.0 scale.
Grade in Principles	the student's grade in Principles of Macroeconomics on a 4.0 scale.
Course Load	the number of credits the student was taking concurrently with Intermediate Macroeconomics.
Business Major	= 1 if student is majoring in business (other than economics).
Non-Business Major	= 1 if student is a non-business major
Female	= 1 if student is female.
Black	= 1 if student is black.
Asian	= 1 if student is of Asian origin.
Hispanic	= 1 if student is of Hispanic.
Sophomore	= 1 if student is a sophomore.
Junior	= 1 if student is a junior.
Grad	= 1 if student is a graduate student.

It should be noted that, since there are dummy variables for Business Majors and non-Business Majors, the default is an Economics major. Similarly, the default ethnic group and class are white and senior, respectively.

The results of this regression are found in Table III. Though positive, the coefficient on Grade Dropping is statistically insignificant, offering no evidence that allowing students to drop their lowest test score will artificially inflate the overall grade in the course.

Table III. Regression Results

	Coefficient	t-statistic
Drop	3.719	1.448
GPA	14.671**	5.951
Grade in Principles	3.545*	1.993
Course Load	-0.222	-0.397
Business Major	-3.646	-1.334
Non-Business Major	-3.725	-1.188
Dummy-Gender	-0.764	-0.28
Dummy-African-American	8.162	1.692
Dummy-Asian	3.556	0.586
Dummy-Latin American	-0.703	-0.073
Dummy-Sophomore	-62.012**	-4.564
Dummy-Junior	-7.444**	-2.848
Dummy-Grad	-2.262	-0.334
Adjusted R-squared	0.504	
F-stat	10.79**	
N	137	

** and * indicate significance at the 1% and 5% levels, respectively.

On the other hand, the student's grade in Principles of Macroeconomics and overall grade point average were each found to be strong indicators of performance in Intermediate Macroeconomics with each positively affecting the final grade in the course. Class status is also a significant contributor to performance where seniors and graduate students fare best while juniors and sophomores struggled.[10] This is likely due to the experience these students have, both in economics and college in general.

No significant effect was found for gender or ethnicity. Men and women performed similarly as did white, black, Asian and Hispanic students. Further, no significant difference was found between Economics, Business and non-Business majors. Finally, the number of credit hours taken was not found to significantly influence the final grade.

STRATEGIC BEHAVIOR

While allowing students to drop their lowest grade does not seem to affect the final grade students receive for the course, it is possible it affected their behavior. Of the 134 students who took the course with the grade dropping option, seventy-nine missed an exam. A disproportionate number, fifty-six, missed the fourth exam.

Table IV shows the decision making process students faced. It shows, by grade going in to the final, the student's options, the decisions they made and the resulting impact on their final grade. For instance, under this grading scheme, twenty-four students had an A going in to the final exam. Not surprisingly, they all opted out of the elective fourth exam. Meanwhile, of the thirty-nine students who had a B going in to the final, twenty-five had the potential to raise their grade.[11] Twenty-two chose to take the exam, and six raised their grades to an A. It should be noted that plusses and minuses were not grading options and that, given the grading structure; it is not possible for the fourth exam to lower a student's grade.

Table IV. Test Taking Strategies

Grade Going In to the Final	Number of Students	Was a higher grade possible?	Took the final	Improved their grade with the fourth exam	Final Grade
A	24	n/a	0		32
B	39	25	22	6	40
C	34	27	21	7 [1]	37
D	16	16	15	10 [2]	8
F	14 [4]	9	5	4 [3]	10
Total	127	77	63	27	127

[1] 2 students with C's going in to the final improved their grade to an A.
[2] 2 students with D's going in to the final improved their grade to an B.
[3] 2 students with F's going in to the final improved their grade to an C.
[4] 7 students withdrew from the course before final grades were submitted.

Overall, of the seventy-seven students who could have raised their grade only sixty-three made the attempt. Twenty-seven of the students did manage to raise their grade by at least one letter-grade, and six raised their grade by two letter grades.[12] One question that comes to mind while reviewing this data is 'Why would a student skip the final if a higher grade is possible?' There are two likely explanations for this. First, some students may be satisfied with their grades. This is certainly the case for the A-student but it is a possibility for other students as well. Second, while a higher grade is possible, it may require a Herculean effort. Both of these explanations should be considered since the fourth exam takes place during final exam week – a most harrowing time.[13]

In addition, many students opted out, not just of the fourth exam, but of the last portion of the course. Table V presents, by grade going in to Test 4, the number of students who skipped the fourth homework assignment and, of those, how many skipped the fourth exam. It is clear that a substantial number simply wrote off the final segment of the class.

Table V. More Test Taking Strategies

	Number of Students	Zero on Homework 4	Zero on Test 4
A	24	10	10
B	39	6	6
C	34	5	5
D	16	5	1
F	14 [4]	8	7
Total	127	34	29

[4] 7 students withdrew from the course before final grades were submitted.

The strategic behavior of students can be analyzed more fully using probit analysis. This shows the effect of various factors on the likelihood of taking an exam. The probit equation is:

$$\text{Prob}\left(\text{take final} = 1\right) = g\begin{pmatrix} \text{GPA, Course Load, Current Grade,} \\ \text{Test Variance, Minimum Score} \end{pmatrix}$$

GPA and Course Load were defined earlier. Current Grade is the students' grade going in to the last exam. This will be their grade if they either skip the last exam or take the last exam but fail to improve on their lowest score. Test Variance is the variance in the results of the first three exams in the semester. Minimum Score is the lowest score the student can earn on the last exam and achieve the next highest grade. That is, it is the score the student must get to bump his/her grade up by one letter grade. Those students who were assured of an A were excluded from this analysis.

The results, found in Table VI, indicate students were influenced by a number of factors. As Test Variance rises, students are more likely to sit for the fourth exam. This is likely due to students with high variance in performance seeing the opportunity to replace a grade substantially below their average with a grade significantly above it. Students with lower variance would not expect to raise their grade substantially with the optional exam.[14] It would also appear students were more likely to sit for the exam the higher their grade was going in to the exam. Remember, this analysis excludes those students that had already earned an A.

Table VI. Probit analysis of students completing the final exam.

	Coefficient	z-statistic
GPA	-0.107	-0.369
Course Load	0.199**	2.944
Current Grade	0.027*	2.151
Min. Score	0.036	0.032
Test Variance	11.661*	2.556
Log Likelihood	-45.253	
N	84	

** and * indicate significance at the 1% and 5% levels, respectively.
The coefficient on the constant term was not reported.

Two results were surprising. One would expect increased credit load to raise the opportunity cost of sitting for this final, making it less likely the student would take the fourth test. However, the greater the student course load, the more likely they were to sit for the exam. This is certainly a surprise. In addition, Minimum Score was not found to be significant. Contrary to expectations, the level of the threshold for improving their grade does not appear to influence their decision.

CONCLUSION

The purpose of this study was to evaluate the effects of allowing students to drop an exam. This study found no evidence that this grading policy artificially inflates the final grade of students. In fact, the average final grade was lower in the sections allowed to drop a score than in sections where no such option was provided. Those who have done well in earlier economics courses (Principles of Macro) and other courses (overall GPA) do better in Intermediate Macroeconomics as do more ‘mature’ students (seniors, graduate students). Ethnicity, gender, and major do not appear to affect the final grade in this course.

While the policy does not seem to affect the student's grade, it does appear to alter his/her behavior. A substantial portion of the class, when given the option, chose to skip the last exam. Several students passed up the opportunity to raise their grade when faced with the rigors of final exam week. This decision was based, in part, on their grade going in to the fourth exam and their concurrent course load. Surprisingly, the greater the course load, the more likely the student would sit for the exam. On the other hand, students with a higher grade going in were more likely to take the test. While the variance in their prior test performance was a factor, the effort required, as seen in the minimum score needed to raise their grade, was not.

A closing comment on this grading policy is in order. There are many reasons to choose a grading policy that allows students to drop their lowest score. However, in light of the results presented above, I believe that allowing students to drop one of a handful of equally weighted exams in the absence of a comprehensive final exam is faulty. The grading structure (A, B, C, D, F – no + or -) likely had an impact. Due to these large gaps in the grading scale, many students found a higher grade beyond reach. At other institutions, with different grading hierarchies, these results may vary.

While dropping a grade does not appear to artificially inflate grades, it would appear several students simply wrote off the final portion of the course.[15] In the absence of a comprehensive final exam, students appear to have little incentive to continue their studies. This, of course, is easily remedied by incorporating a required comprehensive final.

ENDNOTES

1. See Davis (1993) and McKechie (1999) for discussions of these approaches as well as other useful teaching tips.
2. Other less common approaches are scheduling an optional replacement test, usually the last week of class, which can be used to replace the lowest score for the semester or an oral exam as a substitute. In addition, some instructors allow students to choose their grading structure from a list of alternatives where one of these alternatives is dropping an exam.
3. Several studies investigate the various factors contributing to student performance in undergraduate economics classes. See Borg and Stranahan (2002a), Borg and Stranahan (2002b), and Kontolaimou, Pseiridis and Psallidas (2005) are recent examples.
4. As the author acknowledges, by assigning entire classes to one group or the other, the experiment is not randomized. For instance, athletes and student workers may have more rigid schedules and end up in a morning class.
5. Data on two other classes were available but not included due to substantial differences in assessment style.

6. See Dynan and Rouse (1997) for some explanation of the causes of this disparity.
7. Consequently, this study differs somewhat from Sewell (2004) since that study focused on the impact of the grading policy on performance on a cumulative final exam.
8. A t-test reveals that there is no statistically significant difference between these two measures.
9. This is quite similar to the equation used in Sewell (2004). The main difference is the absence of a measure of risk attitude derived from survey data which was insignificant in the Sewell study.
10. Recall that there was only one sophomore in the entire sample.
11. Given the grading approach, it is possible for a students whose average is just above the minimum threshold for a B to find an A is impossible, regardless of how well she does on the last exam while a C-student could find an A achievable. For a simple example imagine a B-student who scored an 80 on each of the first 3 exams would find an A is impossible even if she aced the last test. At the same time, a C student who scored 100, 90 and a 47 could earn an A in the class with an 80 on the last test. It was impossible for their grade to drop as a result of the final exam.
12. Four of those six had missed an exam earlier in the semester.
13. It is not possible to do with the data as structured, but it would be interesting to determine the timing of this decision. That is, of the students who decided to forego the fourth exam when a higher grade is possible, how many made that decision the night before the fourth exam, once the stresses of finals week took effect and how many made the decision immediately upon receiving the graded third exam.
14. Consider two students, each with an average score of 70. The first has earned a score of 70 on each of the three exams. The second scored 40, 80 and 90 on the three exams. The first student would likely forego the opportunity to take the optional exam. After all, based on previous performance he would likely expect his grade to be in the neighborhood of 70. There is little upside potential. The second student, on the other hand knows that she can do well. Even if her performance is just ‘average,’ it will replace the outlier and significantly improve her grade. Thus we see students with higher variance in test scores may be more likely to sit for an optional fourth exam.
15. It should be noted the third exam was offered very late in the semester. There was only a handful of new material presented in the remaining classes. In addition, given the nature of Intermediate Macroeconomics and the structure of the class, the material and the fourth exam were cumulative if not necessarily comprehensive.

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Performance Appraisal and Football Point Spreads: A Note

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Football point spreads were designed to divide the betting public in half and turn transaction costs into risk-free returns for gambling operators. Invariably, one team will be better than its opponent and, in the absence of a point spread, would naturally be the bettors' choice. By awarding a skillfully devised number of points to the inferior team (or underdog), the superior team (or favorite) becomes less attractive to bettors and, in theory, is then picked to "win" by roughly half of the bettors. Academic writers subsequently used point spreads to test the efficiency of people's average economic judgments, arguing that since point spreads—like security prices—adjust for all available information, regular betting profits would be a bona fide exception to the *efficient market hypothesis*.

Another use of point spreads was proposed by Kochman and Goodwin (2007), who argued that the performance of football coaches could be appraised by their success against the point spread. They reasoned that if the football betting market is as efficient as researchers have generally reported, no team should experience abnormal success or failure against the spread (ATS). To do otherwise, Kochman and Goodwin hypothesized that point spreads may only capture the observable factors such as talent, game site and injuries and exclude the intangibles such as mental and physical preparedness, game strategy and game-day decisions. To the extent that those overlooked variables fall largely within the purview of the coach, K&G concluded that success ATS would serve as a legitimate performance standard[1].

While Kochman and Goodwin's performance measure may be more intuitive than empirical, it at least attempted to quantify the effectiveness of coaches. No such effort was made by FoxSports.com when it recently named the 25 most effective college football coaches for the 2008 season. Only the opinions of sports observers were used to produce the rankings seen on its web site on August 24. Persuasive as the brief narratives were for each coach, they contained only anecdotal evidence. And since nearly all the "winners" pilot big-time schools in powerhouse conferences, it seems fair to say that their success is hardly coincidental given the talent that those institutions routinely attract.

The purpose of this study is to appraise the performance of college football coaches in the absence of any advantages which their programs may afford them. To accomplish that goal, we investigated the performance of college football coaches with minimum tenures of seven years at their respective schools against the point spread for the 2001-2007 seasons. (We are assuming that length of tenure prior to the data period will have no input on the rankings.) By contrast, Kochman and Goodwin used a five-year

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minimum and an observation period that differed for each coach. That meant, for example, that the straight-up[2] (SU) and point-wise[3] (PW) records of Joe Paterno over 39 years at Penn State and those of Bobby Bowden over 29 years at Florida State were compared with the SU and PW accomplishments of Randy Walker over six years at Northwestern and the same for Chuck Amato over five years at North Carolina State. Since different years produce different levels of competition, our decision to hold the observation period constant would seem to be a prudent one.

Once our eligible coaches were identified, their respective records with and without reference to the point spread were screened for non-randomness per Equation (1).

$$Z_R = \frac{(G - W)}{\{[(0.50)(1 - 0.50)]/G\}^{1/2}} \qquad (1)$$

where: Z_R = statistic for testing the null hypothesis of randomness
G = total number of games
W = winning games.

Wins-to-games ratios were then compiled and compared to a hurdle mark of 50 percent. A popular newsstand magazine (Steele, 2008) was the source of both coaching profiles and point spread histories. A total of 31 college football coaches satisfied our requirement of seven unbroken years at the same school through the 2007 season. Admittedly, the seven-year constraint is somewhat arbitrary. However, the alternatives were less compelling. Seeking to balance the numbers of years and coaches, we found that an eight-year minimum tenure would have shrunk the number of coaches to 18 while a minimum of six years would have added only five.

Table 1 exhibits the straight-up and point-wise performances of our 31 coaches over the 2001-2007 college football seasons. Inasmuch as they have lasted at least seven years in their respective positions, it is not surprising that the coaches' average winning percentage was an impressive 64.1 percent. And given the alleged efficiency of the point spread market, it was perhaps equally predictable that their average mark against the spread (53.6 percent) hovered around the 50-percent rate.

No fewer than 17 individual coaches surpassed the 53.6-percent mean—topped by Connecticut's Randy Edsall (61.3 percent) and Pete Carroll (60.0 percent). Statistically, both Edsall's record ATS (46 out of 75) and Carroll's mark ATS (54/90) were significantly nonrandom at $p < .10$. While Carroll's straight-up success (84.4 percent) might suggest a strong, positive correlation between straight-up and point-wise success, it is clear from Edsall's 51.8-percent SU record that such is not necessarily the case. We could even imagine a negative relationship between SU and PW records since four of the most

Table 1

Performance logs for college football coaches (2001-2007)

Coach	***School***	***SU***	***SU%***	***PW***	***PW%***
Amstutz	Toledo	55/87	63.2**	40/79	50.6
Ault	Nevada	42/85	49.4	46/81	56.8
Beamer	Virginia Tech	68/92	73.9***	51/87	58.6
Bellotti	Oregon	57/87	65.5***	49/84	58.3
Blakeney	Troy	44/83	53.0	38/69	55.1
Bowden, T.	Clemson	54/87	62.1**	41/82	50.0
Bowden, B.	Florida State	58/90	64.4***	42/89	47.2
Brown	Texas	76/90	84.4***	47/87	54.0
Carroll	So. California	76/90	84.4***	54/90	60.0*
Edsall	Connecticut	43/83	51.8	46/75	61.3*
Ferentz	Iowa	57/87	65.5***	48/83	57.8
Friedgen	Maryland	56/87	64.4***	45/83	54.2
Fulmer	Tennessee	63/90	70.0***	43/88	48.9
Grobe	Wake Forest	46/85	54.1	41/79	51.9
Groh	Virginia	51/88	58.0	47/87	54.0
Hill	Fresno State	59/92	64.1***	43/86	50.0
Leach	Texas Tech	58/89	65.2***	44/83	53.0
Leavitt	South Florida	52/82	63.4**	41/71	57.8
Long	New Mexico	49/87	56.3	43/81	53.1
Paterno	Penn State	50/85	58.8	43/83	51.8
Patterson	Texas Christian	62/86	72.1***	42/80	52.5
Pinkel	Missouri	49/86	57.0	45/81	55.6
Richt	Georgia	72/91	79.1***	47/85	55.3
Riley	Oregon State	52/87	60.0*	42/80	52.5
Schiano	Rutgers	38/84	45.2	44/78	56.4
Schnellenberger	Florida Atlantic	41/83	49.4	19/42	45.2
Stoops	Oklahoma	77/94	79.4***	46/91	50.5
Tiller	Purdue	50/88	56.8	40/81	49.4
Tressel	Ohio State	73/89	82.0***	50/87	57.5
Tuberville	Auburn	66/89	74.2***	47/84	56.0
West	Memphis	41/85	48.2	37/87	42.5
Totals		***1735/2709***	***64.1****	***1351/2523***	***53.6****

*significantly nonrandom at $p < 0.10$
**significantly nonrandom at $p < 0.05$
***significantly nonrandom at $p < 0.01$

prominent coaches in our study (Joe Paterno, Bobby Bowden, Phillip Fulmer and Tommy Bowden) all failed to guide their respective schools to above-average, or even average, performances ATS. *(Since this paper was written, Phillip Fulmer and Tommy Bowden have been relieved of their coaching duties.)* In fact, the combined record of the four coaches against the spread is 169 out of 342—or 49.4 percent.

In sum, all the coaches in our study are effective. Tenures of 7+ years are convincing proof of satisfactory performance. So what have we accomplished? We could perhaps take credit for identifying the ***most*** effective coaches—not unlike the FoxSports.com feature. Our top-five standings would include …

Rank	Coach	School	FoxSports.com Rank
#1	Randy Edsall	Connecticut	Unranked
#2	Pete Carroll	So. California	#2
#3	Frank Beamer	Virginia Tech	#7
#4	Mike Bellotti	Oregon	#15
#5	Kirk Ferentz	Iowa	Unranked

Too, we applauded and extended the proposition that managerial performance can be appraised through the use of point spreads. While the alleged Las Vegas contribution to the field of management may be curious—even dubious—it seems clear that point spreads provide a level playing field on which coaches can be impartially evaluated.

ENDNOTES

1. Kochman and Goodwin ruled out turnovers and penalties as possible explanations of anomalous success ATS since they tend to even out in the long run.
2. actual outcomes
3. betting outcomes

REFERENCES

Kochman, Ladd, and Randy Goodwin. "Performance Evaluation: Another Use of Point Spreads?" Journal of Business and Economic Perspectives, Fall/Winter 2007.

Steele, Phil. College Football Preview, Cleveland: Phil Steele Publications, 2008.

REFEREES

1. Richard Deitz
2. Lester Hadsell
3. Elia Kacapyr
4. Thomas Kopp
5. Ordean Olson
6. Manimoy Paul
7. David Ring
8. Craig Rogers
9. Donald Vitaliano

The New York State Economics Association
61st Annual Conference
Ithaca College
October 10 and 11, 2008

Friday, October 10
6:00-7:30 p.m. Reception: Country Inns and Suites of Ithaca

Saturday, October 11
7:30-8:00 a.m. Registration and Breakfast: Emerson Suites

8:00-8:15 a.m. Welcome: Emerson Suite B
Dr. Leslie Lewis
Dean, School of Humanities and Sciences
Ithaca College

8:15-9:35 a.m. Concurrent Sessions: Group 1

9:50-11:10 a.m. Concurrent Sessions: Group 2

11:25-12:40 a.m. Luncheon and Keynote Address: Emerson Suite B
Dr. Jon M. Conrad
Applied Economics and Management
Cornell University

12:50-2:10 p.m. Concurrent Sessions: Group 3

2:25-3:45 p.m. Concurrent Sessions: Group 4

4:00-5:00 p.m. Business Meeting: Ithaca Falls Room
(All are Welcome)

Conference Sessions

Friday October 10
6:00-7:30 p.m. Reception Country Inns and Suites of Ithaca

Saturday, October 11
7:30-8:00 Registration and Continental Breakfast
8:00-8:15 Welcome

8:15-9:35 Concurrent Sessions: Group 1

Session 1-A: Macroeconomic Issues

Chair Peter Pasqualino
Fulton-Montgomery Community College
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Monetary Policy and Bond Yields
Chin-Wen Huang
City University of New York
chuang@gc.cuny.edu

Discussant: Goswald Hughes
State University of New York, Oswego
goswaldh@gmail.com

Avalanches, Extinctions, Crashes, and the Sub-Prime Crisis in the Minsky Model
Goswald Hughes
State University of New York, Oswego
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Discussant: Ramond MacDermott
Virginia Military Institute
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Imigration and Foreign Direct Investment
Ramond MacDermott and James Bang
Virginia Military Institute
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Discussant: Chin-Wen Huang
City University of New York
chuang@gc.cuny.edu

Session 1-B: Undergraduate Research Session

Chair Florence Shu
State University of New York, Potsdam
shufp@potsdam.edu

Stochastic Analysis of Alternate Ways of Scheduling Transport for Tourists
Muhammad H. Chaudhary — Sponsor
Rochester Institute of Technology — Amitrajeet A. Batabyal
mhc7974@rit.edu — Rochester Institute of Technology

Measuring the Social Cost of a Chemical Spill in Ithaca, New York:
Some Preliminary Findings
Sara Holmes — Sponsor:
Ithaca College — Elia Kacapyr
sholmes1@ithaca.edu — Ithaca College

"An Assessment of U.S. Air Quality Policy: Past, Present and Future".
Casey Wichman
Ithaca College — Sponsor:
cwichma1@ithaca.edu — H. Spencer Banzhaf
Georgia State University

Minimum Wage Increases and Their Impact on the Restaurant Industry
Benjamin O'Neil — Sponsor:
Ithaca College — Darius Conger
boneil1@ithaca.edu — Ithaca College

Who Is Responsible for Sewage Backups/Failures in Your Own Yard?
Alexander Brehm — Sponsor:
State University of New York, Potsdam — Florence Shu
brehm13@potsdam.edu — State University of New York, Potsdam

Session 1-C: International/Development Economics

Chair: Michael MacAvoy
State University of New York, Oneonta
MCAVOYM@oneonta.edu

How Falling Exchange Rates Have Affected the US Economy and Trade Deficit
John Heim
Rensselaer Polytechnic Institute
heimj@rpi.edu

Discussant: Chunpin Hsu
City University of New York
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Modeling the Dependence of Asian Stock Markets and Currancy Exchange with Copula-Based Semi-Par. Approaches
Chunpin Hsu
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Discussant: John Heim
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ICT and Education in Developing Countries: A study of India and China
Richard Vogel and Bala Veeramacheneni
State University of New York, Farmingdale
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Discussant: Shaianne Osterreich
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Session 1-D: Economics and Education

Chair: Jeffrey Wagner
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What a Student Wants: Factors that Affect the Course Selection Process
Della Lee Sue and Gregory Tully
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Discussant Joanna Mitchell
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PEP: Progressive Economic Perspectives, Grades 3-6
Joanna Mitchell
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Discussant: Manimoy Paul
Siena College
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Tuition Increase: Exploring Factors Causing it
Manimoy Paul
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Discussant: John Piccione
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9:50-11:10 Concurrent Sessions: Group 2

Session 2-A: History of Economic Thought

Chair: Shaianne Osterreich
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Ideology and Voluntary Economic Standards
Jeanette Mitchell
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Discussant: James Booker
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Institutional Economics in the Age of J.P. Morgan
William T. Ganley
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Discussant: Kent Klitgaard
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Inovation, Evolution, and the Business Cycle in the Neo-Schumpterian Framework
Goswald Hughes
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Discussant Michael MacAvoy
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Session 2-B: Environmental Economics I

Chair Robert Culp
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The Role of Environmental Attributes in the Home Improvement Decision
Robert Culp
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Discussant: Open

The Social Discount Rate: An Explanation for "Throw Away" Societies?
John McCollough
Penn State-Lehigh

Discussant: Open

Sewage Failures/Backups: Still a US Evnironmental Issue?
Florence Shu
State University of New York, Potsdam
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Discussant: Open

11:25-12:40 a.m. Luncheon and Keynote Address

12:50-2:10 Concurrent Sessions: Group 3

Session 3-A: Panel Discussion: Teaching Undergraduate Econometrics

Chair: Elia Kacypur
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Panelists:

Cynthia Bansak,
St. Lawrence University.
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Jo Beth Mertens
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Jeffrey Pliskin
Hamilton College
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Bridget Gleeson
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Session 3-B: Economic Theory

Chair: James Booker
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Is it Time for a Change in Economic Theory?
Kent Klitgaard
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Discussant: Natsuko Iwasaki
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Business Week and the Coming of Keynesianism to America
Ranjit Dighe
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Discussant Roger Hindereliter
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A Peak into a Pandora's Box: Comparing Demand Under Price Competition with Inverse Demand Under Quantity Competition
William Kolberg
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Discussant: Joseph Eisenhauer
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Session 3-C: Public Finance

Chair: David Ring
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The Impact of Sarbanes-Oxley and Fraud Experience on Governance and Internal Control in Local Government: The Case of Towns and Villages in New York
Ronald J. Huefner
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Discussant: Richard Vogel
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OffTrack Betting and the New York Economy
Richard Vogel
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Discussant: A. Schiller Casimir
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Lawyers, The Rule of Law and Income Inequality:
Russell Harrison
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Discussant: Lester Hadsell
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Session 3-D: Labor Economics

Chair: William O'Dea
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Government as Facilitator of Anti-Poverty Programs
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Disussant: Jaison Abel
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Is there a Human Capital "Flypaper Effect" from Local Colleges and Universities?
Richard Deitz and Jason R. Abel
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Discussant: Frank Musgrave
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Human Capital and Economic Activity in Urban America
Jaison Abel
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Todd M. Gabe
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Discussant: William O'Dea
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2:25-3:45 Concurrent Sessions: Group 4

Session 4:A: Teaching Economics

Chair: Christopher Tom
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International Economics Teaching
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Surveying Student Learning in an Active and Participatory Economics Teaching
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Discussant: Della Lee Sue
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Teaching Economics to Non-Economics/Non-Business Majors
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Discussant: Edwin Portugal
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Session 4-B: Environmental and Agricultural Economics II

Chair: Jim Booker
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Exposure of Demographic Groups to Toxic Pollutants in NY State
Bridget Gleeson-Hanna, Dan Hatch, and Christopher Lominac
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Discussant Tom Kopp
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The Law and Economics of Municipal Solid Waste Restrictions
Christine Longo and Jeffrey Wagner
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Discussant: Edouard B. Mafoua-Koukebene
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Pricing Weather-Based Irrigation Cost Insurance
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Discussant: Jeffrey Wagner
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Session 4-C: Applied Microeconomics

Chair: Wade Thomas
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Experimental Estimates of Taxpayer Ethics

Joe Eisenhauer	Doris Geide-Stevenson	David Ferro
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Discussant: Ronald J. Huefner
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An Iterative Monotone Interval Condensing Method for the Uniform Approximation of External Fixed Points of Isotone Operators with Applications to Markov Equilibrium for Stochastic OLG Models with NeoClassical Production

Jaime McGovern
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Discussant Russell Harrison
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Tax Incentives and Capital Spending Revisited

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Discussant: Jaime McGovern
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Deceptive Advertising in a Market for Experience Goods

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Discussant: Bharat Bhole
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4:00-5:00 NYSEA Business Meeting

Participants

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62ND ANNUAL CONFERENCE

FRIDAY AND SATURDAY

OCTOBER 16-17, 2009

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