REJOINDER TO THE REPLY TO

WESTERN KENTUCKY UNIVERSITY'S ATHLETICS PROGRAM: FINANCIAL BURDEN OR BOON?

We appreciate the thoughtful REPLY made by the Fiscal Affairs Committee of the Faculty Senate to our study. Section One of this rejoinder was written by Bob Pulsinelli; Section Two, which deals with the statistical model, was written by Brian Goff.

SECTION ONE

I will consider the points made in the REPLY in the same order in which they appeared.

(1) Questionable Assumptions.

(a) Nothing written in the REPLY indicates that WKU is operating at full capacity. The fact that *residence halls* are at full capacity during the first two or three weeks of the semester says little about whether the *institution* is at full capacity. My paternal grandparents owned an Italian bakery during the Great Depression and they operated at near-full capacity. Because bread sold well during that period (for obvious economic reasons), should we assume that the *whole economy* was at near-full capacity? Moreover, we indicated in our original paper that WKU residence halls are at full capacity (early in the semester) because they are priced below market value. Finally, we *included* dorm rentals as a marginal cost in our study (to be conservative), so why even mention it now?

Enrollment caps may be "seriously discussed," but that does not mean that we are operating at or near full capacity; many reasons such caps may be desirable have little to do with physical capacity. Many faculty members doubtless prefer smaller class sizes or may wish to increase the "quality" of our students.

We do not maintain that empty classrooms indicate an "inadequate" number of students; they do, however, indicate excess physical capacity in the normal usage of that word. If we were really operating at or near-full capacity, we might well be forced to consider week-end classes.[1]

Interestingly enough, using a system developed by the Western Interstate Council on Higher Education (WICHE), WKU's Office of Institutional Research recently "ran a utilization study of Western's instructional space." The WICHE system, in effect, considers a school to be operating at "full capacity" when all classrooms are in use 4 hours per day, 5 days per week, and when class enrollment equals classroom capacity. (I believe that such a definition is obviously much too conservative, and the Director of Institutional Research does too.) Using that standard, nevertheless, it was estimated that at WKU (a) rooms were in use about 75 percent of the operating time, (b) when the rooms were in use, enrollment was about 57 percent of room capacity, and therefore,

[1]Noting that students "prefer" weekday to weekend classes doesn't say much. After all, I "prefer" a Mercedes to a Honda but purchased a Honda because it is so much cheaper. We might be able to entice students to take weekend (or night) classes by charging lower tuition fees at such "less preferred" times -- if ever we get near full capacity.

(c) the (weighted average) percent of *overall* room capacity in use was only about 49 percent.[2] Thus, using even such an unrealistic definition of "full capacity," WKU operates at less than 50 percent of full capacity.

(b) Our common sense tells us that if a student athlete *wants* to participate competitively in his or her sport then he or she will not come to WKU if the University drops the sport. It further tells us that if the student wants to play and *another* school offers him or her an athletic grant-in-aid but WKU does not, the student will choose the other university. The athletic coaches at WKU believe that if an athlete is good enough to be offered a WKU athletic grant-in-aid, that student surely will be offered one elsewhere. Their first-hand experience is consistent with our assumption.

While it is not *inconceivable* that student-athletes would attend WKU even if their sport were dropped, it is doubtful that such students exist in sufficient numbers to justify the assumption that student athletes would enroll at WKU whether or not "their" sport were dropped. After all, they can receive an education elsewhere, and at a lower cost (i.e., they can receive a grant-in-aid elsewhere). Surely the burden is on the authors of the REPLY to provide the relevant estimates; that something is "not inconceivable" hardly makes it eligible as a good working assumption for empirical research.

[2]For various reasons even that figure is overstated.

The statement "If student athletes chose not to attend WKU, they would most likely be replaced by other students" implies that somehow student-athletes occupy precious spaces that become available to non-student-athletes only when the former vacate the premises. It is obvious to me that all the non-student-athletes who want to be here (in the sense that they are willing and able to pay the cost of doing so) are already here. (Of course, this argument ultimately relates to the issue of whether or not WKU is at full capacity.)

The issue of causality, with respect to sports performance and enrollment, is considered below by Brian Goff. I pause here only to make three observations.

(i) While everyone has his or her own "correlation is not causality" anecdote,[3] the REPLY authors seem to think that such implicit advice is useful only when criticizing *others*.

There is something strange about an argument that (a) begins with the statement that correlation does not prove causation, and (b) proceeds to, by *statistical manipulation*, show that "aha" the correlations are too small to be significant. Suppose, after making *their* statistical manipulations, the REPLY authors had found that indeed, the correlations were very high. Would they then reject their results because they would remind themselves that

[3]Although I must confess, I think *their* anecdote is not a very good one. Surely at *some* (large) bee count the correlation between the number of bees and the number of people wearing shorts is *negative* (and non-spurious). I look forward to seeing the authors of the REPLY wearing shorts when the "Killer Bees" arrive in Bowling Green some warm summer day.

correlation does not necessarily imply causation? If it is not proper to infer causation from high correlations, is it proper to infer *non*-causation from low correlations? (Technically, it could be true that *their* model is misspecified -- and we think it is.) It seems that the REPLY authors want it both ways: statistics can't be used to prove what the authors dislike, but statistics can be used to disprove what the authors dislike.

(ii) The REPLY authors note that our "assumption" that athletic performance has an impact on enrollment " ... is glaring *petitio principii*: to the extent that the argument of PBG rests on this assumption, it is assuming its conclusion, and is circular." But we do not *assume* a relationship; we develop a *testable hypothesis* concerning such a relationship, based on acceptable economic theory, and we test that hypothesis empirically. That, in fact, is how science is conducted in economics.[4]

(iii) The REPLY authors write that our enrollment-caused-by-athletic-performance "assumption" (note again that we insist that it is not an *assumption*) "... is an essential component in an argument whose purpose is to demonstrate that the athletic program results in an increase in enrollment." Frankly, I'm getting a little tired of hearing and reading things that imply that we have some hidden agenda, or that we had a conclusion in mind and set out to demonstrate it. We undertook this study because it was obvious

[4]This point is so obvious it is hard to understand why the REPLY authors make such a big deal about this issue. Maybe claiming that another's work is illogical helps one's cause; or maybe it was just a good chance to use a little Latin.

to us (and to anyone with training in economics) that the Vos study overstated the costs and understated the benefits of WKU's athletic program.[5] We thought it would be interesting to apply economic analysis to the issue. If people cannot believe that we are capable of making an honest inquiry, then that says more about them than it does about us. And such charges are rather hypocritical, coming as they do from people (not necessarily the REPLY authors) whose real aim is to reallocate money from athletic programs to faculty salaries.

(2) Omitted Considerations.

While it is true that we did not identify *all* possible marginal costs, neither did we consider all possible marginal benefits. Some of each were omitted because we deemed them to be relatively small and difficult to quantify (and hence not worth the cost of estimating). We certainly are willing to read any exhaustive study which *does* attempt to measure with precision *all* potential costs and benefits. Perhaps the authors of the REPLY are willing to do so in the future.

Once again, I defer to Brian Goff those comments regarding the statistical model. I do, however, want to comment on the notion of allocating funds to academic (as opposed to athletic) scholarships.

[5]The most obvious (Vos) cost overstatement is the assignation to the athletic budget of tuition "costs" (pure accounting transfers) of student athletes' tuition grants-in-aid; the most glaring revenue (Vos) understatement is the failure to allocate to the athletic program state formula funding monies that accrue to WKU from the enrollment of student athletes.

I have no quarrel with that notion. It was I, in fact, who proposed such an idea to the Faculty Senate some four years ago -- based on the same economic model used in our study. Neither the faculty nor the administration rushed to put such a plan into action. It is amusing to note, nonetheless, that my model was not received with such hostility then. Could it be that it is the *conclusions*, and not the model, that rankle?

At any rate, we were only interested in discovering whether the athletic program as a whole (and each sport within that program) was self-financing. Nowhere in our study do we say that the same funds could not, at least theoretically, be used more efficiently in an alternative use. We are prepared to analyze any serious studies that address this important issue.[6]

(3) Inconsistent results. See Brian Goff's remarks.

(4) Faulty logic. (See ii in Part 1 above regarding our having "assumed our conclusions").

I keep reading about how we argue against a hypothetical opponent who "advocates eliminating WKU's athletic program altogether." First, I would like to point out that the Vos report does indeed at least hint at replacing WKU's athletic program with intramurals -- and that many townspeople interpret what has been written and said in precisely that manner. Second, in our report

[6]While it is certainly valid to consider alternative expenditures that potentially are more efficient than the athletic program, we haven't observed that the administration (or the Faculty Senate for that matter) is particularly concerned with economic efficiency in *other* areas.

we *do not* limit ourselves to that consideration. We do indicate what would happen to revenues and costs if the program were eliminated. And we indicate what would happen to costs and revenues if *each specific sport were eliminated*. (People continue to pretend that our analysis deals only with the program as a whole.) Third, we note that due to the fact that most costs are sunk (not marginal), reduction in the scope of specific programs (for example, a drop to Division II or III, or to intramurals) would cause revenues to fall faster than costs and weaken the financial condition of WKU.

In closing, I would like to say that our major conclusion (which seems to have gotten lost in the statistical argument concerning the effect of athletic performance on enrollment) is unscathed by the REPLY. Our main finding was that once the relevant costs and revenues are identified, then the annual athletic program incurs costs that exceed revenues by only $330,036; and that in order to be self-financing the athletic program need only attract about 80 non-athletes per year to enroll at WKU.

Now we turn to Brian Goff's comments regarding our statistical model.

SECTION TWO

The REPLY purports to show that the PBG/ARIMA model is inappropriate and flawed and then its authors estimate an alternative least squares (LS) model, which is claimed to be superior.

In the following paragraphs we address three main issues:

1) Criticisms of the PBG model;

2) Problems in the alternative analysis;

3) Comparisons of the two models.

1) Criticisms of the PBG/ARIMA Model

(a) The main criticism of the ARIMA model focuses on a supposed statistical bias in the predictions/residuals of both the "ARIMA-Only" and ARIMA-Athletic models. The REPLY calculates predicted and residual values based on simply adding the constant term [323.7 in "ARIMA-Only" and -837 in ARIMA-Athletic model] to the rest of the equation. This is incorrect.

Due to a misunderstanding of ARIMA modelling on the part of the REPLY authors, or to a lack of communication on our part, these values were treated in the REPLY as constant terms in typical regression equations. Instead, they are estimates of "MU" in ARIMA parlance. The full "constant term" is computed by

Constant = MU(1 - Autoregressive Coefficient)

or Constant = 323.7(1 - 0.621) in "ARIMA-Only" case

Constant = -837(1 - 0.67) in full ARIMA-Athletic case.

[See Pankratz, p. 241]

Because of this confusion, the REPLY generates overpredictions in the "ARIMA-Only" model and underpredictions for the "ARIMA-Base" part of the full PBG model solely from

miscalculation by the REPLY authors. The *residuals* in both of our models do, in fact, *sum to zero*; hence, the assertion in the REPLY that the "ARIMA-Only" model and the full PBG model generally overpredict is false, not simply in judgment but in fact. Thus, to the extent that the criticisms of our model rely on this misconception on the part of the REPLY authors, much of what is written in their Appendix is erroneous and irrelevant.

As additional checks of the PBG model's residuals for the desired property of independence of errors (i.e. non-systematically correlated), we performed the Box-Pierce Q test [Q(lag,24) = 20.5], the Durbin-Watson, and the Durbin-h test (D/W = 2.02; Durbin-h = 0.09]. All of these indicate a lack of residual correlation. Also, we overfitted the model with a second autoregressive term, which proved insignificant. This is a common means to check for correlation in the errors and also supports the conclusion of no residual correlation. (These are for the full PBG model; the same residual statistics for the ARIMA-Only yield identical results.)

(b) The claim that a more desirable number of observations for ARIMA analysis would be larger than the number available is correct. However, this holds true for all time series analysis techniques, including least squares. Estimates sometimes must be made under less than optimal conditions.

(c) It is true that the ARIMA method was designed primarily for forecasting applications. However, predicting or forecasting

the impact of dropping athletics is the question with which we were concerned.

(d) The REPLY implies that we pulled an underhanded trick by including other KY enrollment changes as an additional, rather than central variable. The REPLY also says that other KY enrollment is the primary causal variable.

Neither other KY enrollment nor the autoregressive term (lagged enrollment) is a primary causal variable. *Both* stand as proxies for the underlying causal variables such as income, tuition, graduation rates, and so on. We expected that the ARIMA-Only model would already filter out most of the impact of other KY enrollment. Inclusion of other KY enrollment reflected our desire to be up front and, at least, permit the possibility that the ARIMA-Only model did not capture most of the influence of the underlying causal variables.

2) <u>Problems in the REPLY (LS) analysis</u>

(a) When the REPLY LS model includes lagged football and basketball percentages along with dummy variables for post-season play, it is misspecified. Including the dummy variables with the continuous variables assumes the dummy variables have an additional impact on top of the continuous variables. We do not claim this in our paper. Statistically, inclusion of all four athletic variables creates the classic regression problem of multicollinearity (or strong correlation among regressors). This problem creates larger standard

errors of coefficients relative to the coefficients, which reduces t-statistics. The results of our estimations are not, therefore, inconsistent.

(b) The Durbin-Watson statistic on the REPLY LS model = 1.31, which falls in the ambiguous range for whether the model's residuals are systematically correlated.

3) Comparison of the Models

The REPLY LS model (R^2 = 0.45) outperforms the ARIMA-Only (R^2 = 0.38) in terms of predictive ability. The full PBG (ARIMA-Athletics) model (R^2 = 0.55) outperforms the REPLY LS model (the LS-Athletic model R^2 = 0.47). On predictive ability, the full PBG model is superior.

The PBG model generates unambiguously, uncorrelated residuals. The REPLY LS model is ambiguous as to residual correlation according to the Durbin-Watson test.

Conclusions

1. The REPLY does not show the PBG results to be inappropriate. The primary attempt to show that the PBG model is inappropriate is based on miscalculation by the REPLY authors.

2. The REPLY offers an alternative, though inferior, estimation technique. Using this alternative model, basketball winning has a smaller and less significant impact. Football has no impact.

3. If the REPLY LS model places a lower bound on the estimates and the PBG model an upper bound, we would still conclude that

basketball has substantial enrollment impacts. Football's impact would be smaller than basketball's and more open to question.