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Competitive Balance Conundrums: Response to Fort and Maxcy's Comment

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Reply

COMPETITIVE BALANCE CONUNDRUMS

Response to Fort and Maxcy's Comment

Fort and Maxcy misrepresent my argument at various points and misapprehend the nature of the competitive balance issue. The analysis of competitive balance needs to be informed by theory. Purely empirical exercises are unlikely to enhance our understanding of the importance of competitive balance to team sports leagues.

Keywords: *competitive balance*

In successive sentences at the outset of their comment, Fort and Maxcy (hereafter F&M) assert that I take “an unfortunate position concerning the measurement of competitive balance” and that I adopt “an unnecessary position concerning the measurement of competitive balance.” I agree with the latter. Of course, it is not necessary for me to believe what I believe. I disagree with the former. I believe my position is fortunate.

But before attempting to persuade the reader that my position is fortunate, let me clarify what my position is. F&M write that I insist “there is a single best measure of competitive balance.” This is misleading. My view is that if one were choosing among several measures, the best from the group would be the one to which fan behavior showed greatest sensitivity.¹ More on this in a moment.

F&M also quote at length from page 113 of my introductory article to the effect that fans do not experience a particular measurement of competitive balance and then assert that the quote applies to the standard deviation of win percentages. Yet it is clear in my piece that I am referring not to the standard deviation of win percentages but to the ratio of actual standard deviation to idealized standard deviation of win percentages. In my view, fans basically do experience the former but not the latter.

It is also clear from my piece that I believe that competitive balance is a complex phenomenon, that it has many dimensions, and that it is conditioned by the history and culture of a sport. I argue, for instance, fans care not only about the uncertainty

of outcome but also about the perceived fairness of the game. For this reason, I suggest that a team's ability to buy a championship and remain on top year after year—or that a team's inability to be competitive year after year—will affect a fan's response. Part of this problem may be captured by the correlation between win percentages and team payroll. In contrast, under certain circumstances fans may enjoy a dominant team because it adds an edge to a contest or a season. Thus, any single measurement of competitive balance is likely to be inadequate.

F&M divide the competitive balance literature into two parts: descriptive/empirical and analytical. I think that for purposes of characterizing the existing literature this dichotomy may be largely legitimate, but I also believe that it is unfortunate. If the empirical measurement of competitive balance is not informed by theory, then it runs the risk of data mining or mindless empiricism.

There are a plethora of ways to measure statistical dispersion. Some are commonly used in the empirical competitive balance literature: standard deviation across teams during a year, standard deviation over time, ratio of standard to idealized standard deviation, Gini coefficient of win percentages or championships, the adjusted Gini coefficient Herfindahl-Hirschman Index of championships, the ratio of top to bottom win percentages, the range of win percentages, and excess tail frequencies, among others.² There are also equally plausible but infrequently used measurements, such as: the ratio of the 90th to the 10th percentile of win percentages, the range of the 90th to the 10th percentile of win percentages; the interquartile range or ratio of win percentages; the range or ratio of any other two percentiles; the coefficient of variation; skewness; and so forth. The competitive balance literature should be more than an endless cataloging of these metrics.

Presumably, the reason sports economists are interested in competitive balance is that our theory and empirical research tell us that fans expect a certain degree of uncertainty in the outcome of games and seasons. Fans also expect fairness in the rules and conditions of competition. As a result, the economic success of a sports league will be affected by competitive balance. At the root of the connection between competitive balance and success is the fan. If fans were indifferent to the uncertainty of outcome and fairness of competition, competitive balance would be no more relevant to our study than the choice of typescript on the balls used in competition.

On this premise, I suggested using a filter among the multitudinous potential measurements of competitive balance: Those metrics to which fans evince greatest sensitivity are the best ones. I see nothing in F&M's comment to make me alter that position.

F&M's comments, however, help reaffirm my view. For instance, they write: "It is clear competitive balance declined in the period after the strike in the American League (AL) (38%) but not in the National League (NL). This result was fueled by especially dramatic changes in 1998-1999. A clear suggestion is that the revenue sharing scheme put in place in 1996 bears further analysis!" The evidence for changes or lack of changes in competitive balance cited by F&M is the yearly stan-

dard deviation of win percentages. In other words, without explaining why, F&M assume that this is the definitive measurement of competitive balance.

Why should that be so? Why isn't it the tight correlation between payrolls and win percentages that becomes highly significant in the mid-1990s and remains so, after not being consistently significant during the previous 15 years? Why isn't it the fact that between 1995 and 2001 only four teams from the bottom half of payrolls reached the playoffs, and combined they won only 5 out of 224 postseason games? Or why shouldn't we consider dozens of other measures?³

And what do F&M mean when they say the "dramatic changes" in 1998 and 1999 are connected to Major League Baseball's post-1996 revenue sharing system? Do they believe the system promoted balance or imbalance, and why? The new revenue sharing system began to apply in 1996. The new luxury tax plan applied in 1997, 1998, and 1999. It ceased to apply in 2000, precisely the year that F&M's standard deviation falls in both leagues. Did the end of the luxury tax bring more balance?

There are no easy answers to these questions. Indeed, if there is nothing to justify our empirical measurement of balance, it is not even clear that the questions are interesting in the first place.

In the end, it may be that the best measure of competitive balance is a multivariate index, that it is nonlinear or constrained, and/or that it differs league by league. Whatever the optimal measure, we need to know what we are seeking before we begin to quantify. I continue to believe that fan behavior is an appropriate place to begin.

NOTES

1. Brad Humphreys's (2002) article in the cited volume provides a cogent illustration of this point.
2. See Utt and Fort (2002).
3. Indeed, what is an owner's objective function? If owners seek to maximize profits, isn't it in their interest to win games and divisional races by narrow margins? If so, how would this affect expectations about dispersion measurements and the analysis of competitive balance?

REFERENCES

- Humphreys, B. R. (2002, May). Alternative measures of competitive balance in sports leagues. *Journal of Sports Economics*, 3, 133-148.
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