By the Numbers

The Newsletter of the Statistical Analysis Committee of the Society for American Baseball Research
Volume 3, Number 1 February, 1991

COMMITTEE NEWS

This issue is the first of what I hope will be four 1991 issues. One of the difficulties of trying to put out a newsletter like this is that it must be done in my spare time, working around the things that pay the mortgage.

Another difficulty is the need to assemble an inventory of articles to publish. Once again, I am running out of things and need to appeal to you to provide me with material. I'm rather pleased with what we have in this issue, but, as of now, I have nothing for the planned April issue.

If you are working on something which you think may be of interest, send it along. Also, if you are doing something fairly lengthy, which you think may be too long for the newsletter, think again. I am perfectly willing to devote the entire issue of a newsletter to a single piece, if it seems good enough.

If you do submit a piece, it works best for me if you can submit it on a 3.5", 720K floppy diskette, preferably in any version of Microsoft Word, or in an ASCII file (which I can read into Word). However, I am happy to get pieces in other formats as well.

Thus endeth my every issue plea for material.

I enclosed with the last issue of the newsletter a questionnaire. I intend to use the responses to this questionnaire to compile a current and accurate membership roster. (For new committee members, a copy of the questionnaire is enclosed with this issue.) However, a large number of you have not returned the questionnaire. If you do not return your questionnaire, I will assume that you are no longer interested in the Statistical Analysis Committee and I will remove your name from the roster and from the mailing list. **If you do not return the questionnaire, the last issue you will receive will be Vol. 3, No. 2, scheduled for mailing in April.**

A list of all of those from whom I have received completed questionnaires appears on the last page of this issue of the newsletter. If you have misplaced your questionnaire and wish to have a replacement copy, please let me know and I will send you one.

Donald A. Coffin
Indiana University Northwest
3400 Broadway
Gary, IN 46408
219-980-6646

****

SHORTS I

Bruce Stone (5054 Chowen Ave. South, Minneapolis, MN 55410) has sent me a compilation of games won and lost as a result of unearned runs through August in the 1990 season. The team totals are below.

AL:	W	L	NL:	W	L
BAL	10	4	ATL	11	12
BOS	6	7	CIN	7	5
CAL	5	14	CHI	10	8
CHI	12	9	HOU	4	8
CLE	7	4	LA	12	9
DET	5	7	MTL	10	7
KC	6	9	NY	5	6
MIL	8	11	PHI	4	6
MIN	8	8	PIT	8	3
NY	11	9	STL	4	6
OAK	8	4	SF	4	6
SEA	8	9	SD	5	9
TEX	11	7			
TOR	7	10			

EFFECTS OF ADJACENT PLAYERS ON DEFENSIVE AVERAGE

Robert B. Davis
1504 Hunters Lake East
Cuyahoga Falls, OH 44221

Introduction. Defensive average (or DA) is a recent breakthrough in evaluating defensive performance. In simplest terms, a fielder's DA is the fraction of balls hit to or through his area that he successfully converts into outs. Defensive average seeks to perform roughly the same task which range factor was designed for; however, DA has an advantage because it takes into account the actual opportunities that a fielder has to make a play. Thus, the defensive averages of fielders who play behind a pitching staff which is predominantly right- (or left-) handed or compiles extremely high (or low) numbers of strikeouts will not be affected by the same biases which are inherent in range factor. For a complete description of defensive average, see DeCoursey (1990a). All DA data in this paper are from DeCoursey (1990a).

However, there remain other biases which could possibly influence a player's DA. The first question which might be asked is: How is a player's DA affected by the fielders who play next to him? For example, there is an area between the third baseman and shortstop such that balls going through this area count against both fielders. As a result, a third baseman with outstanding range might save several balls over the course of the season which would have been counted against a shortstop; a third baseman with the range of a toaster oven will wave meekly at these balls, thus artificially lowering his defensive neighbor's DA. Obviously, this relationship works both ways, and could be extended to all adjacent infield positions. Is Jose Lind's DA going to be inflated because of the range of Sid Bream? Will Ken Caminiti's DA inaccurately reflect his performance merely because he plays next to Rafael Ramirez? To state the problem in Pete DeCoursey's words, "When you examine the San Francisco infield for instance, are they all really the best at their individual positions? Or are they all above average, giving each other's stats a shot of numerical helium?" (DeCoursey (1990b).) We will be able to better answer questions such as these after examining the results presented in this paper.

Methodology. In this paper, we will examine the relationships between AGDA statistics for adjacent infielders. This results in three pairs to investigate: 1b-2b, 2b-SS, and SS-3b. AGDA (essentially ground ball defensive average) numbers will be used instead of ADA (overall defensive average) because it seems that ground balls are the most likely chances which could be counted against two adjacent infielders at the same time.

Team AGDA from the 1988 season will be used for each position. It was decided that this was better than using individual player data for a variety of reasons. First, some teams had positions where no infielder appeared in 100 games. Second, the study concerns the properties of the AGDA statistic, and not of individual players. Hence, it is not sufficient to study the interaction between Ozzie Smith and Terry Pendleton, for example; instead, we must consider all the chances handled (or not handled) by everyone who played those positions for the Cardinals in 1988. It may be true that a player with particularly outstanding range can affect the AGDA of adjacent fielders, but here we are just studying the properties of AGDA in general. Further studies may be done at a later date involving "player pairs;" i.e., does Shawon Dunston's AGDA vary based on who plays third base for the Cubs?

In order to determine if a linear relationship exists between "adjacent fielder" AGDA, the linear correlation coefficients will be calculated for all pairs of infield positions. Since the relationship between "adjacent fielder"

AGDA is not necessarily linear, scatter plots of the values will also be produced to attempt to identify alternative functional relationships. If a strong relationship is present, it should show up in either the correlation coefficientson or the plots.

Note that some positive correlation between teammate defensive averages is unavoidable due to the home field factor. For instance, Fulton County Stadium has a notoriously bad reputation among infielders; thus, one would expect that all of their infielders would have their defensive averages adversely affected. (Note that all four Brave infield positions had below average performance in 1988. However, data presented in Dewan and Zminda (1990) would seem to indicate that the low AGDA values are due to a dearth of talent instead of home park disadvantages.) Also, if several managers have preferences for "glove men" and several others for hitters, additional correlation will be present. For this reason, only relatively high values of the correlation coefficients would demonstrate flaws in AGDA as a method for evaluating individual infielders.

Numerical Results and Scatter Plots. The correlation matrix for the various positions is given in the following table.

	1B	2B	SS	3B
1B	1.00	0.48	0.16	0.09
2B	0.48	1.00	0.34	0.29
SS	0.16	0.34	1.00	0.45
3B	0.09	0.29	0.45	1.00

This presents evidence that, if a linear relationship is present between adjacent fielder AGDA values, it is not a very strong one. To consider other types of relationships, one must examine the scatter plots (for reasons of space, these are not reproduced but are available from By the Numbers on request, with a SASE).

The 1B-2B pair provides the strongest linear correlation coefficient (0.48). It is possible that the strongest relationship exists between these two positions because of the fact that a first baseman spends some of his time holding runners close to the bag; in such situations, he is almost wholly dependent on his sidekick to make the plays on the right side of the infield and salvage his AGDA. (One can imagine Wally Joyner attempting to blame a low AGDA on Johnny Ray at an arbitration hearing in a more enlightened future!)

Note that this is a theory, and no empirical evidence is present to support or refute it. The AGDA values for first and second basemen with a runner being held at first would be necessary to test such a theory.

Note that even though this is the strongest relationship among adjacent positions, it is not indicative of an overwhelming dependence of a second baseman's DA on his first baseman's range (and vice-versa). A correlation coefficient of 0.48 could reasonably result from pure chance. Indeed, if one were testing the hypotheses

H0: 1b-2b AGDA values are independent
H1: 1b-2b AGDA values are positively correlated

this result would not lead to a rejection of the null hypothesis at the 1% level of significance (although the null would be rejected at a 10% level of significance). (See, for example, Johnson (1988) or most similar elementary statistics textbooks.) When one considers the suspected additional factors (home field and managerial preference) listed above which could contribute to positive correlation for teammate AGDA, this result provides evidence in favor of AGDA as a tool for evaluating individual infielders.

The scatter plot also displays no clear relationship between AGDA for first and second basemen. This is a solid argument in favor of AGDA.

The 2B-SS pair has an even lower correlation coefficient (0.34). This seemed surprising, because it is easy to view second baseman-shortstop combinations as units who depend on teamwork to survive. However, much of this impression arises from watching double plays; AGDA values between second basemen and shortstops displayed the lowest correlation coefficient for all three pairs of adjacent infielders. One possible reason for this correlation being a bit lower than the other two (which are very close indeed) involves the mechanics of Project Scoresheet scoring techniques. While the "dividing line" between the other pairs of adjacent fielders is somewhat ill-defined, it is easy for a Project Scoresheet volunteer to tell on which side of second base a ball was hit. Thus, except for the occasional play in which a shortstop comes across the diamond to field a ball and throw out the runner, the efforts of one keystone player will not seriously alter the AGDA of his partner. Again, the scatter plot does not reveal any clear relationship between the AGDA values.

The SS-3B values had a correlation coefficient (0.45) similar to that for the 1B-2B pairs. Again, this (and the accompanying scatter plot) presents no evidence of a functional relationship present between AGDA values. It would appear that a third baseman can be judged fairly by his AGDA, whether he plays next to Mark Belanger or Rafael Santana. To look at a specific case: Yes, Chris Sabo compiled a remarkable AGDA while playing next to Barry Larkin. However, this evidence suggests that even with a weaker defensive shortstop beside him, Sabo would still have had a Gold Glove caliber AGDA.

The entire correlation matrix was provided to consider one other matter: The effects of home field, managerial preference, and any other relevant factors on defensive average. The first baseman-third basemen linear correlation coefficient of 0.09 may provide an estimate of the inherent correlation due to such factors. If a study were done in which only AGDA road statistics were analyzed, the drop in linear correlation from 0.09 would provide an indicator of how much impact the home park has on AGDA. (Note that methods other than correlation studies could also be used to estimate this effect.)

Conclusions and Future Research. It was found that while some correlation was present between defensive averages of adjacent infielders, no strong linear (or nonlinear) relationship exists which invalidates AGDA as a tool for evaluating individual infielders on their own merit. This is a convincing argument in favor of AGDA. Now it seems clear that Chris Sabo was really a terrific third baseman in 1988, and not merely Barry Larkin's beneficiary.

This study demonstrates that the AGDA statistics for "defensive neighbors" are not hopelessly entangled. If the correlation coefficients or graphs had provided some obvious relationship between AGDA values for such players, it would be difficult to decide which player was contributing how much to overall team defense. Since no such relationship was found, using AGDA in judging individual defensive players is reasonable.

Note that the positive correlation which was present in the data is well within the realm of possiblity for essentially independent data sets. Further, it can be explained in a number of ways. The two suspected primary causes of such correlation would be the quality of the home park infield and the managerial preference for glove men or hitters.

Further research needs to be done to determine the relationship between home park and infielder defensive average. A method of doing such a study has been suggested; the analysis in this paper could be repeated using road defensive average only. Such a study would remove the home field bias, and possibly lower the correlation coefficients a good deal. If the correlations determined in such a study were extremely low, even stronger evidence in favor of (park adjusted) defensive averages as a defensive measuring stick would be provided.

The relationship between defensive average for adjacent outfielders might also be investigated in future work. Such a study might shed new light on the extent to which an efficient center-fielder helps the overall outfield defense.

Acknowledgements: Thanks to Pete DeCoursey for his words of encouragement, and for developing DA to begin with. Thanks also to my wife, Karen, for proofreading and constructive criticism.

References:

DeCoursey, Pete (1990a). Philadelphia Baseball File, Volume 1, Number 5, pp. 41-74.

DeCoursey, Pete (1990b). Personal correspondence.

Dewan, John and Zminda, Don (1990). The Stats Baseball Scoreboard, pp. 62-63.

Johnson, Robert (1988). Elementary Statistics, pp. 481-482.

****

OAKLAND SWEEPS SEASON SERIES WITH YANKS!! How likely was it, really?

By Rob Wood

The 1990 Oakland Athletics swept the season series with the New York Yankees, beating them in all 12 games. The A's completely dominated the Yanks, out-scoring them 62-12 in aggregate. The local and national media made quite a bit out of the season sweep, citing that it was only the fourth such sweep in the entire 20th century.

It will prove instrumental to look at the three previous sweeps: The 1988 Royals (84-77) swept the Orioles (54-107) in 12 straight, the 1978 Orioles (90-71) swept the A's (69-93) in 11 straight, and the 1970 Orioles (108-54) swept the Royals (65-97) in 12 straight. Thus in each case there was a lopsided match-up, a strong team sweeping a weak team.

There are several reasons why such a sweep is more likely than is commonly believed. First, there are numerous match-ups that could result in a season series sweep, the A's-Yankees being but one of 91 pairs (14 teams choose 2). Thus, while any specified series sweep may be unlikely, the odds that we will observe any series sweep is significantly higher.

Second, in the 8-team 154-game schedule, each team faced each of the other 7 teams in the league 22 times. Compare that to the current AL schedule (13 games with the other 6 divisional opponents, and 12 games with the 7 non-divisional opponents), and to the current NL schedule (18 games with the other 5 divisional opponents, and 12 games with the 6 non-divisional opponents).

The following table presents the actual odds of a season series sweep in each of the four principal schedules employed by baseball in the 20th century. These odds were derived from a computer simulation of thousands of seasons based upon the historical relationship among teams' seasonal winning percentages. I have used the entire 20th century as the relevant distribution. [The effect on these odds of today's increased "parity" is minor.]

ODDS OF A SEASON SERIES SWEEP			
8-Team 154-Game Schedule	10-Team 162-Game Schedule	12-Team 162-Game Schedule	14-Team 162-Game Schedule
1 in 3,200 seasons	1 in 277 seasons	1 in 23 seasons	1 in 12 seasons

Teams underlying abilities were drawn at random at the beginning of each season. Then a full schedule of games was played, with the outcome of each game depending (via the standard formula) upon the abilities of each team. For example, a 0.600 team facing a 0.400 team has an underlying 0.692 winning percentage in these games.

To give the reader a better feel for these odds, in a league in which all teams are of equal ability, the odds of a season series sweep are 1 in 75,000 seasons in the 8-team league, 1 in 2,913 seasons in the 10-team league, 1 in 56 seasons in the 12-team league, and 1 in 29 seasons in the 14-team league. As a comparison, the true odds given in the table are roughly equal to the odds of a season series sweep in a hypothetical league in which a favorite has a 0.575 chance to win each game.

As the table demonstrates, every wave of expansion dramatically increased the odds of observing a season series sweep. There are a greater number of match-ups after expansion, and each team plays far fewer games in a season versus its opponents in the newer leagues--especially in the two-division leagues.

The 8-team 154-game schedule was used in the NL from 1904-1961 and in the AL from 1904-1960. No season series was ever swept in these 115 seasons.

The 10-team 162-game schedule was used in the NL from 1962-1968 and in the AL from 1961-1968. No season series was ever swept in these 15 seasons.

The 12-team (with two 6-team divisions) 162-game schedule was used in the NL from 1969-1990 and in the AL from 1969-1976. We have observed 1 season series sweep (1970 Orioles-Royals) in these 30 seasons.

The 14-team (with two 7-team divisions) 162-game schedule was used in the AL from 1977-1990. We have observed 3 season series sweeps (1978 Orioles-A's, 1988 Royals-Orioles, and 1990 A's-Yankees) in these 14 seasons.

Formal statistical tests confirm that these observed frequencies are well within the bounds of chance. I conclude that once again formal statistical analysis demonstrates that observed baseball occurrences are not as unlikely as they may appear to the general public.

Rob Wood is an economist living and working in the San Francisco area.

****

RELIEVER'S WIN CONTRIBUTION

By Bill Deane

The term "blown save" has been with us for some time, but the phenomenon had never really been quantified until 1988. That year, the arbitrary formula used in the Rolaids Relife Man Awards incorporated "blown saves" for the first time. The official definition was: "A pitcher is charged with a 'blown save' when he enters the game in a save situation, and leaves the game with the situation no longer in effect because he has given up the lead."

In conjunction with the Rolaids Awards, Wirz & Associates of Norwalk, connecticut, have been documenting every save opportunity and blown save in the major leagues over the past two seasons (1988 and 1989). Steve Luftschein and Toby Zwikel of W&A provided the data used in this essay.

In 1988, there were a total of 1,462 save opportunities in the majors, with 1,049 (71.8%) being convetted into saves, and the other 413 going down as blown saves. Thses data offer interesting analytical possibilities, and lead to what I call "Reliever's Win Contribution": The number of wins a relief ace contributes to his team above what an "average" reliever might have contributed.

The 1,462 save opportunities in 1988 encompassed 1,396 games. There were 62 games in which a team had two save opportunities, and two games in which a team had <u>three</u> save opportunities. The San Diego Padres had the dubious distinction of being the only team to blow three save opportunities ina single game.

In the 1,396 games, 1,002 saves were recorded without the lead being surrendered. Obviously, the teams' record in these games was 1,002-0. However, a blown save, which occurred in the other 394 contests, does not necessarily lead to a loss. Teams blowing the first save opportunity managed to win 159 times (producing 47 saves for succeeding relievers), lose 234, and tie ones, for a 0.405 percentage in these games. Overall, then, teams turning over a save opportunity to the bullpen posted a record of 1,161-234-1 in the 1396 games, an 0.832 percentage. Assuming the 1988 data to be typical, we can conclude the following:

1) A pitcher who posts a win has increased his team's win probability from 0.832 wins to 1.000 wins (a gain of 0.168 wins).

2) A pitcher who blows a save opportunity has decreased his team's win probability from 0.832 wins to 0.405 wins (a loss of 0.427 wins).

My formula for "Reliever's Win Contribution" (RWC) rewards a reliever with 0.168 "wins" for every save (S) converted, but penalizes him by 0.427 "wins" for every blows save (BS):

RWC = 0.168*S - 0.427*BS

Let's use Mark Davis to illustrate the formula in action. Davis was credited with a major-league-leading 44 saves for San Diego in 1989, while being charges with just four blown saves. His RWC works out to (0.168*44) - (0.427*4), or 7.39 - 1.71. Davis is thus calculated to have contributed 5.68 wins (in thsoe 48 save opportunities) above what an "average" reliever might have produced. This was, by far, the best total in the majors.[1]

The table in the next column presents the 11 pitchers who achieved an RWC of 2.0 or better in 1989, plus 11 other pitchers with high save totals.

Two problems with the RWC formula are that it assumes that all save situations are created equal, and it ignores appearances which are not save opportunities. Pete Palmer suggests an improvement: Situational analysis of win probability for each save opportunity. For example, is a reliever comes into a game in the bottom of the ninth, with the bases loaded, none out, and a one-run lead, the defensive team's win probability is only 26.9% (rather than the 83.2% of an "average" save opportunity. If the reliever converts the win in this situation, he should be credited with 0.731 wins (1.000 - 0.269). On the other hand, if a reliever enters in the bottom of the ninth with nobody on base and a three run lead, the defensive team's win probability is 96.5%; a converted save here would be worth only 0.035 wins. Obviously this method would be more accurate, but also far more complex. For those of us who have just saves and blown saves to work with, I offer RWC as an alternative to "Rolaids Points" or just plain save totals.

Pitcher, Team	S	BS	RWC
Mark Davis, SD	44	4	5.68
Jeff Russell, TEX	38	6	3.82
Bill Landrum, PIT	26	3	3.09
Jay Howell, LA	28	4	3.00
Dennis Eckersley, OAK	33	6	2.98
Dan Plesac, MIL	33	7	2.56
Mike Schooler, SEA	33	7	2.56
Dave Smith, HOU	25	4	2.49
John Franco, CIN	32	7	2.39
Lee Guetterman, NYAL	13	0	2.18
Todd Worrell, STL	20	3	2.08
Greg Olson, BAL	26	6	1.97
Randy Myers, NYNL	24	5	1.90
Bobby Thigpen, CHIAL	34	9	1.87
Lee Smith, BOS	25	6	1.64
Doug Jones, CLE	32	9	1.53
Mitch Williams, CHINL	36	11	1.35
Bryan Harvey, CAL	25	7	1.21
Jeff Reardon, MIN	31	11	0.51
S. Bedrosian, PHI-SF	23	8	0.45
Dave Righetti, NYAL	25	9	0.36
Tim Burke, MTL	28	11	0.01

Another potential problem with RWC is that it compares a pitcher with a mythical "average" receiver (with a save percentage of 0.718), rather than a replacement-level reliever, with a save percentage of, say, 0.500 (the lowest figure among the 44 pitchers with 10 or more save opportunities in 1989). This would raise Mark Davis up to something like 12 wins above a replacement-level reliever--a preposterous conclusion in my opinion. How can a player, in 93

1. (Ed. note) Willie Hernandez's record in 1984 of 33 saves and one blown save would give him a RWC of 5.12.

half-innings, be responsible for 12 wins all by himself? I think the problem here is that you'd be assuming that all of the best pitchers are relievers, when in reality almost all are starters. Don't you think that is all the best pitchers--Orel Hershiser, Roger Clemens, et al.--were put in the bullpen, the average save percentage would be more like 85% to 90%, and a "replacement-level" figure would be, or, maybe 71.8%?

RWC does not pretend to be a fool-proof statistic for evaluating relief pitchers. However, it does represent a step toward the better understanding of the contributions they make.

****

SHORTS II

Remember: if you have data you would like to share with other researchers, or if there is data you need for a project on which you are looking, we may be able to help. To share data, write up a brief (one paragraph) description of it, including the form in which it's available, and any charge for it, and send it along to By the Numbers. Make someone's day and advance the cause of understanding baseball.

****

BATTING EYE COMBINED WITH POWER

By Jorgen Rasmussen

In looking at Jonathan Katz'a batting eye tables (By the Numbers, Vol. 2, No. 3), I am struck by a name that doesn't appear--Joe DiMaggio. This is because Katz uses Gagnon's data and DiMaggio didn't quite make the cut--BEI of 0.243. But if the third column of Katz's Table 1 is to be complete, DiMaggio should appear just above Cullenbine at 0.363. This still puts him some distance behind Ted Williams' 0.480.

Perhaps Williams did have the better eye, but the comparison is more complicated than these figures might suggest. As David Halberstam writes in Summer of '49 (p. 188): "Williams would never, no matter what the situation, go for a pitch that was even a shade outside the strike zone. DiMaggio was different: He believed that, as a power hitter on the team, he sometimes had an obligation to swing at imperfect ptiches. On certain occasions a walk was not enough; it was a victory for the pitcher." Given this difference in approach, is BEI (of whatever formula) compelling evidence that Williams had a better eye than DiMaggio?

More generally, is batting eye simply a matter of whether a hitter can distinguish a fourth ball from a third strike? Most strikeouts are swinging, not called. Failure to make contact is not evidence of a bad eye; the pitch may indeed have been in the strike zone. As Katz acknowledges, a hitter with a good eye is more likely than not to have selected a pitch to hit into play before a fourth ball or third strike is thrown. While the ratio of walks to strikeouts is one element of a hitter's contribution to the offense, it is a questionable measure of batting eye.

I would suggest that as good a measure is disciplined power. Free-swinging power hitters are likely to strike out a lot. They are also likely to get a lot of walks, either intentional or because the pitcher has been told, "Don't give this guy anything good to hit. Thus Katz's finding that walks and strikeouts are correlated is not surprising. The trick, or course, is to select a pitch that is close enough to being hittable to offer some prospect of success if swung at. Swing, but not wildly at just anything.

The best success a batter can achieve is a home run. The greatest failure is a strikeout. (A double play is a matter of the base-running situation, the placement of the fielders, etc.) A ground ball or a fly ball has some chance of advancing runners (if some are on), while a strikeout contributes nothing. Comparing these two extremes of offensive success provides evidence of a hitter's ability to select pitches. If a hitter swings for the fences even when he has two strikes, he will miss a lot of third strikes. If he shortens up, especially when he has two strikes, just to get the ball in play, he will reduce his home run potential.

Dividing strikeouts by home runs produces a disciplined power ratio (DPR). While this statistic lacks the virtue of Katz's BEI3 (it has no upper range or lower limit), it produces more than just a relative ranking of players. An 1.00 DPR means that a hitter is just as likely to hit a home run as to strike out, virtually an absolute measure of excellence. While DPR may well involve skills in addition to batting eye, it certainly seems related to a hitter's ability to select "his" pitch from among the balls thrown him. Presumably batting eye knows meaning when to swing, and that is not just a matter of whether a pitch is a strike. A high ball hitter may take a low strike ,not because his eye is deficient, but because he hopes that one of the subsequent pitches will come closer to his best hitting spot.

So who has the best eye in terms of being able to get his pitch without swinging at bad balls or being called out on strikes? Who have been the disciplined power hitters? Presumably we should seek them among two groups--those most likely to hit home runs and those least likely to strike out. Using data in Total Baseball, I selected those hitters who averaged 5.8 home runs or more per 100 at-bats during their careers (18 players) and those hitters who averaged 27 or more at-bats per strikeout (15 players). Dividing career strikeouts by career home runs produced the following Disciplined Power Ratios:

Player	DPR
Ted Williams	1.36
Tommy Holmes	1.39
Frank McCormick	1.48
Lou Gehrig	1.60
Hank Aaron	1.83
Babe Ruth	1.86
Ralph Kiner	2.03
Don Mueller	2.25
Willie Mays	2.31
Joe Sewell	2.33
Jimmy Foxx	2.46
Hank Sauer	2.48
Hank Greenberg	2.55
Frankie Frisch	2.59
Frank Robinson	2.61
Billy Southworth	2.85
Dale Mitchell	2.90
Eddie Matthews	2.90
Andy High	2.95
Harmon Killebrew	2.97
Willie McCovey	2.98
Mickey Mantle	3.19
Rip Radcliff	3.36
Mike Schmidt	3.44
Frank Howard	3.82
Edd Roush	3.88
Willie Stargell	4.08
Dave Kingman	4.11
Pie Traynor	4.79
Nellie Fox	6.17
Lloyd Waner	6.41
Sam Rice	8.09
Johnny Cooney	53.50

Not surprisingly, Williams leads the list. Nor are the 4th through 7th names unexpected. But what are Tommy Holmes and Frank McCormick doing a close second and third? Neither of them struck out much and both had some power. Their slugging averages are respectable--0.432 and 0.434. McCormick hit between 17 and 20 home runs in four separate years. Holmes is a bit more questionable. Although he hit 28 home runs in 1945 (more than McCormick in any single year), in no other year did he hit even half that number. That one season, in fact, accounted for nearly a third of his career home run output.[2]

Note that Sewell, who headed Katz's BEI3 list, does fairly well in DPR also. Clearly, he should be remembered as more

2. (Ed. note.) If we assume that the rest of Homles's career is really typical, and take the three years in which he hit more home runs than in other years (except 1945), he homered in 1.9% of his at-bats, or a predicted 13 homers in 1945. This alters his DPR to 1.67, still good enough for 4th on the list.

than the man who replaced Ray Chapman. His lifetime slugging average was over 0.400 and he drove in over 1000 runs. Call it disciplined power, batting eye, or whatever, Sewell stacks up well with some fast company.

Contrast Sewell's DPR with that of the last few names on the list. These batters, like Sewel, struck out relatively infrequently, but, like Nellie Fox, have no reputation for power. Lending further validity to the ratio is the result for Dave Kingman, the classic free-swinging power hitter. No onw thought of him as having a good eye. yes, he hit home rums qrequently--more than 6.5 for every 100 at-bats, and only four players in the history of baseball hit them more frequently than that--but he also struck out with much greater frequency. As the DPR indicates, he was more than four times as likely to strike out as to hit a home run.[3]

As I noted, I examined only a manageable group, which, because of frequent home runs and/or few strikeouts, was likely to contain the players with the best DPRs. In addition, I checked a couple of other players who were too far down on the career home run and strikeout lists to be included--Ernie Lombardi and Yogi Berra. Neither of them struck out a great deal and both had some power. Lombardi's DPR is nearly as good as Williams--1.38. And Berra's is even better--1.16.

These DPRs, even more than those of McCormick and Holmes, may jeopardize any claim for the validity of this measure. Do I really think that Lombardi and Berra has as good or better batting eyes as Williams? No, I don't want to make that claim. But both were selective enough hitters to avoid striking out without sacrificing power. Yes, I know that Berra was always ridiculed as a bad-ball hitter. Yet the fact remains that he is 78th on the all-time list for low striekouts--better than Billy Herman, Luke Appling, and Jackie Robinson, to name only three. If you can make contact when you swing at "bad" balls and hit them out of the park nearly as often as you miss them for a third strike, isn;t that some discipline? Isn't that just as much a "good eye" as the hitter who simply stands at the plate waiting for a walk?

Finally, what about Joe DImaggio--the point where all this started. He also was too low on both lists to be included, but his DPR is the most remarkable of all. During his career, DiMaggio struck out only 8 times more than he homered! His DPR was 1.02. As I said to begin, maybe Williams had a better eye than DiMaggio. BEI says that he did. But maybe, as Halberstam says, they just had different batting philosophies. And anybody who can hit the ball out of the part as often as he swings and misses a third strike must be seeing the ball pretty well, regardless of whether you call it batting eye.

3. (Ed. note.) For those of you who are interested, Reggie Jackson has a DPR of 4.62, and missed this list because his home run frequency was only 5.7%.

Jorgen Rasmussen follows baseball and baseball analysis from Ames, Iowa.

****

A QUICK AND DIRTY STUDY OF THE RELATIONSHIP BETWEEN THE COUNT AND THE PLATOON ADVANTAGE

By Charlie Pavitt

Over the past few years, statistical baseball researchers have found fairly strong evidence for two claims concerning offensive performance. First, it has become clear that batting performance is strongly related to the count. Pete Palmer (see pp. 163-164 in The Hidden Game of Baseball) looking at League Championship and World Series games from 1974 to 1982, found consistent and significant increases for batting and on-base average and runs produced as the count became more favorable to the batter. Stanley M. Katz (Baseball Research Journal #15) discovered similar tendencies for batting and on-base average in a sample of games from 1986.

Second, the platoon advantage, or tendency for batters to perform better when facing opposite-handed pitchers than when facing same-handed pitchers, appears to be universal. John Thorn and Pete Palmer (The Hidden Game of Baseball, p. 165) describe a study by George Lindsey showing consistent platoon advantages for batting averages in games from 1951 and 1952, while Palmer found platoon advantages for batting, on-base, and slugging averages for the American League from 1974 to 1977 (Baseball Analyst, #24). Tom Hanrahan (Baseball Analyst, #24) found consistent platoon advantages in the 1984 games for several measures, whether a hitter was good or had, or tended toward home runs or singles. Bill James (1988 Baseball Abstract) had similar findings for 1984 to 1986 games, and in addition found the platoon advantage ot hold across hitters who strike out a lot or a little, and hitters of different ages.

Given these two findings, it seems reasonable to ask if they are. Perhaps hitters are more successful against opposite-handed pitchers because they are more likely to get the count in their facor. Looked at from the defensive standpoint, perhaps pitchers are more successful against same-handed batters because they are able to get ahead in the count. To test this relationship, I performed what scientists call a quick and dirty study. I looked at a few weeks of 1988 Project Scoresheet data for Phillies games (May 11 - June 14) and noted for each at-bat during those games whether the batter did or did not reach each of the 11 possible ball-strike counts (I did not include 0-0, which of course was reached in every at-bat).

I call the study "quick and dirty" for three reasons. First, the sample size is small (28 games) and only includes Phillies games; thus, any conclusions can only be generalized to baseball in general at some risk. Second, I did not note the progress of the count. There are many different ways to get to a 3-2 count, but I did not distinguisg among them. Second, I only noted a count once; thus, for example, if a batter at 2-2 fouled off some pitchers, I did not note it again. For these latter reasons, I cannot do a Markov analysis of the ball-strike count, as Katz did, which is really the best way to do this sort of study.

The findings for the National League versus Phillies' pitching (based on 725 plate appearances with platoon advantage and 316 with platoon disadvantage are shown in Table 1. The table shows, for each number of pitches in the count (disregarding "extra pitches" from two-strike fould), the proportion of times each count was reached or the at-bat ended. After one and two pitches, the batter could not have struck out, so those at-bats usually would have ended with the batter hitting the ball (with the occasional hit by pitch and the like other possibilities). This is not the case with three or more pitches, where strikeouts and walks began proliferating.

With one exception, the findings support the proposed relationship between ball-strike count and platoon advantage. After one pitch, batters with the platoon advantage were much more likely to have the count in their favor or, one could assume, to have found a pitch to their liking and have hit it. After two pitches, batters with the platoon advantage were more likely to have hit the ball and less likely to have the count in the pitcher's favor, but were not more likely to have the count strongly (2-0) in their favor. After three pitches, patters with a platoon advantage were more likely to have a 2-1 count and less likely to be behind, but were no more likely to be ahead 3-0. The platoon advantage still existed, although weakly, after four pitches and just about disappeared after five pitches. Overall, the findings are supportive, with the exception of the no-strike counts. Unless it is a fluke specific to these games, this exception requires an explanation.

Table 1: NL vs. Phillie Pitching		
Count	Advantage	Disadvantage
One Pitch		
1-0	0.412	0.383
0-1	0.386	0.481
AB Ended	0.202	0.136
Two Pitches		
2-0	0.159	0.152
1-1	0.326	0.354
0-2	0.143	0.206
AB Ended	0.372	0.288
Three Pitches		
3-0	0.055	0.057
2-1	0.190	0.177
1-2	0.199	0.247
AB Ended	0.556	0.519
Four Pitches		
3-1	0.076	0.063
2-2	0.171	0.190
AB Ended	0.753	0.747
Five Pitches		
3-2	0.097	0.089
AB Ended	0.903	0.911

The findings for the Phillies batters (with 433 plate appearnaces with platoon advantage and 496 with platoon disadvantage) are shown in Table 2.

In this case, the expected relationship did not occur. In fact, the only consistent finding was for Phillie batters to end an at-bat earlier in the count with a platoon disadvantage, findings opposite for those of other teams against Phillie pitching.[4]

4. (Ed. note.) The Phillies batters do seem to go deeped into the count that their opponents--note that the proportion of at-bats ending after x pitches is always lower for Phillie batters.

Table 2: Phillies vs. NL Pitching		
Count	Advantage	Disadvantage
One Pitch		
1-0	0.490	0.492
0-1	0.416	0.431
AB Ended	0.094	0.077
Two Pitches		
2-0	0.198	0.183
1-1	0.388	0.385
0-2	0.185	0.145
AB Ended	0.228	0.287
Three Pitches		
3-0	0.065	0.058
2-1	0.252	0.226
1-2	0.254	0.236
AB Ended	0.438	0.480
Four Pitches		
3-1	0.102	0.095
2-2	0.226	0.177
AB Ended	0.672	0.728
Five Pitches		
3-2	0.120	0.099
AB Ended	0.880	0.901

The differences in results for Phillies batters and for other teams' batters could be the result of one or both of the following two possibilities, which only future research can evaluate:

1) While consistent with expectation, the results for National League batters in this 28-game sample may in fact not be valid for baseball in general.

2) The results for national League batters is indeed valid for baseball in general, but the Phillies batters are unusual.[5] if this possibility turns out to be true, the resulting implications are interesting. First, it could be that the Phillies batters asa group are unable to take advantage of platoon differentials, or were unable to do so during this 28-game stretch. If this were

5. Or the sample is an insufficient sample to observe the true relationship for Phillies batters.

the case, we would then have a potential diagnostic tool for evaluating offensive performance. However, evidence does not support this claim. Based on data from the Philadelphia Baseball File (Vol. 1, No. 2), the Phillies as a team had a normal platoon advantage during the first half of 1988 [BA with (without) platoon advantage: 0.271 (0.222); SA with (without) platoon advantage: 0.384 (0.354); OBA with (without) platoon advantage: 0.342 (0.290)].

Second, the possibility exists that an individual player's platoon differential is connected with the ability to control the count. Similarly to the team in general, if this were trun it would provide a tool for evaluating a player's performance. Again, the available data are not consistent with this idea.

Table 3: Von Hayes in the Count		
Count	Advantage	Disadvantage
One Pitch		
1-0	0.473	0.500
0-1	0.396	0.500
AB Ended	0.131	0.000
Two Pitches		
2-0	0.275	0.273
1-1	0.396	0.318
0-2	0.132	0.227
AB Ended	0.197	0.182
Three Pitches		
3-0	0.099	0.136
2-1	0.341	0.227
1-2	0.231	0.273
AB Ended	0.329	0.364
Four Pitches		
3-1	0.132	0.227
2-2	0.308	0.182
AB Ended	0.560	0.591
Five Pitches		
3-2	0.077	0.182
AB Ended	0.923	0.818

I looked at the count and platoon differentials for the six players who were everyday players during the first half of the year (Bradley, Hayes, James, Parrish, Samuel Schmidt) and found no relationship. To illustrate the most extreme cases, as of June 30 (again based on data from the Philadelphia Baseball File), Von Hayes as usual had an extreme platoon differential [BA with (without) platoon advantage: 0.328 (0.113); SA with (without) platoon advantage: 0.498 (0.197); OBA with (without) platoon advantage: 0.405 (0.222)], while his count differential was consistent with expectation, and inconsistent with the Phillies as a team (but not abnormally so) (see Table 3 for Hayes count differentials).

Juan Samuel, on the other hand, often has platoon reversals (see data in The Great American Baseball Stat Book),

Table 4: Juan Samuel in the Count		
Count	Advantage	Disadvantage
One Pitch		
1-0	0.524	0.375
0-1	0.476	0.490
AB Ended	0.000	0.135
Two Pitches		
2-0	0.143	0.115
1-1	0.381	0.292
0-2	0.190	0.198
AB Ended	0.286	0.385
Three Pitches		
3-0	0.048	0.042
2-1	0.238	0.146
1-2	0.190	0.250
AB Ended	0.524	0.562
Four Pitches		
3-1	0.000	0.052
2-2	0.333	0.125
AB Ended	0.667	0.823
Five Pitches		
3-2	0.190	0.063
AB Ended	0.890	0.937

although up to June 30, it was unusually extreme [BA with (without) platoon advantage: 0.152 (0.251); SA with (without) platoon advantage: 0.261 (0.383); OBA with (without) platoon advantage: 0.212 (0.297)]. However, as with Hayes, his count differential was as expected and inconsistent with his team as a whole (see Table 4 above).

Given the nature of the data set used in this study, all of my findings are naturally quite preliminary. However, I believe that the relationship of the count to platoon differential and to batter preformance will be a major source of research questions for baseball researchers to ponder in the coming years.

****

Questionnaire Respondents

John Benson
Phil Birnbaum
Patrick Brown
Robert Brunell
David Buonanno
Randy Burnham
Ralph Caola
Rob Ciandella
Tom Conlon
Patrick Curtis
Bob Davis
Andy Finn
Roy Fleming
Stephen Grant
Brock Hanke
Ralph Horton
Kevin Huigens
Gordon Hylton
David Icenogle
Tom Jennings
Keith Karcher
Jonathan Katz
Herman Krabbenhoft
Stephen Krevisky
Mark Landsbaum
Dan Lindner
John Matthew IV
Barry Mednick
Evan Meyer
Jim O'Malley
Mark Pankin
Charlie Pavitt
W. T. Rankin
Daniel Rapport
Jorgen Rasmussen
Stuart Shea
Gary Skoog
Tal Smith
Judge Bruce Stone
John Stryker
Charles Weaver Jr.
Woody Wilson
Rob Wood