By the Numbers

The Newsletter of the Statistical Analysis Committee of the Society for American Baseball Research
June, 1991
Vol. 3, No. 3

Committee News

This Issue. Keith Karcher's article is, I think, an extremely important one, because it points out the dangers of relying too readily on statistical tests. If, as he concludes, we need more than a full season's data (for example, 900+ at-bats in "clutch" situations) before a test of the magnitude of a difference becomes "powerful" enough to rely on, then we need to examine data over periods substantially larger than a single season.

Rob Wood examines a potential benefit of having a runner on first with exceptional stolen base ability. His conclusion is that there may be more to the value of stolen bases than we might previously have concluded.

Bob Davis has developed a new index to measure the concentration of batting skills in exrtra-base power. It is interesting to observe his findings and also interesting to speculate on whether this i ndex may be analytically useful.

Finally, Jonathan Katz extends the discussion of batting selectivity, by attempting to find a relationship between Jorgen Rasmussen's Disciplined Power Ratio and the various Batting Eye Indexes that Katz had previously examined in this newsletter.

Future Issues. After the convention issue, I have NO MORE MATERIAL. If you are working on something, please let me know, or send me a copy. Remember, my preferred format for articles is on a 3.5", 720K disk (DOS formatted), either a Microsoft Word file of a plain text (ASCII) file. You should include a printout as well; if you do not have access to a computer, a type-written article will be fine.

I have had a number of committee members express an interest in reading something about Rotisserie (and similar) leagues. If you are doing something here, I think we can guarantee you an audience. If you are interested in doing something, please give me a call so we can talk aboout what may be the best approach to the issues. Also, if you're interested in reading about it, why not write about it?

Committee Roster. The committee membership roster will be purged following the convention (on or about July 1) of all current members who have not returned the membership questionnaire (new members, as listed in Vol. 3, No. 2 excepted). This will leave us with about 75 committee members, which is still, I think, pretty good for a committee which was completely non-funtioning two-and-a-half years ago.

Please use the membership roster to make contacts with other committee members, especially those in your vicinity. You may find that you can collaborate on some topic, or that you simply enjoy talking baseball.

See you next time (September?) with reports on convention presentations.

Donald A. Coffin
Indiana University Northwest
3400 Broadway
Gary, IN 46408
219-980-6867

The Power of Statistical Tests

By Keith Karcher
1F Reler Lane
Somerset, NJ 08873

I. Introduction. In the December 1989 issue of "By The Numbers", Rob Wood presented an article ("Clutch Ability: Myth or Reality?") which seemed to sound the death knell for the argument in favor of clutch hitting. That was until, in a subsequent issue, a brief retort (by Tom Conlon, in BTN, Vol. 2, No. 2) raised the question of the power of the statistical test Wood used in his article. The rejoinder claimed, correctly, that we cannot accept the null hypothesis that clutch hitting does not exist unless we have sufficient power to do so, we can only fail to reject this hypothesis. This article attempts to explain power a bit more fully and then perform power calculations for tests comparing various batting average splits.

II. Into the Wood's. First, let's go back to the seminal Wood article and see what test he is employing to arrive at his conclusions. In his article, he presents a table (reprinted below) which displays the differentials required to claim that the player's performance in the clutch was significantly different from his performance in non-clutch situations. (Wood assumes that 10% of the season's at-bats are during clutch situations as defined by Elias as "late-inning pressure situations with runners in scoring position".) This section will formalize the process that Wood must have carried out to obtain the presented differentials.

Table 1: Clutch Versus Non-CLutch Performance Differences Required for Performance Differentials to be Statistically Significant

At-Bats	Non-Clutch BA	Required Clutch BA	Required Differential
300	.200	.356	.156
	.250	.418	.168
	.300	.477	.177
400	.200	.335	.135
	.250	.395	.145
	.300	.453	.153
500	.200	.320	.120
	.250	.380	.130
	.300	.437	.137
600	.200	.310	.110
	.250	.368	.118
	.300	.425	.125

We will first need some notation. Define the following terms:

BA_c = batting average in clutch situations,
BA_n = batting average in non-clutch situations,
BA = overall batting average,
AB_c = at-bats in clutch situations,
AB_n = at-bats in non-clutch situations,
AB = total at-bats.

Wood's null hypothesis is:

H0: $BA_c = BA_n = BA$,

i.e., there is no difference between clutch batting average and non-clutch batting average, they are merely reflections of the player's overall batting average. To test this hypothesis Wood employed the following statistic:

$$(1) \qquad Z = \frac{|BA_c - BA_n|}{\sqrt{[BA(1-BA)]*[1/AB_c + 1/ABn]}}$$

This is the standard statistic used to compare two independent binomial proportions, which is what BA_c and BA_n are. Z, under the null hypothesis, is a standard normal variate which can be compared to tabled values to determine its significance. Now, we can verify Wood's significant differentials by plugging the batting averages and at-bats into (1) and calculating Z. Using the .300 non-clutch batter with 600 at-bats we get:

BAc = .425,
BAn = .300,
ABc = (.9)(600) = 540,
ABn = (.1)(600) = 60,
BAn = [(540)(.3) + (60)(.425)]/600 = .3125,

Therefore,

$Z = 1.98$

(The reader may verify this by making the appropriate substitutions in the formula above).

The value of Z required to declare the differential significant at the 5% level (5% of the time such a differential could occur by chance) is 1.96. 1.98 exceeds this, therefore this differential is significant. The other differentials in the table are similarly just above the magic value of 1.96.

It is important to note that the Z-statistic is two-sided. That is, taking the same .3125 overall hitter in our example, the batter could hit .125 worse in the clutch than in non-clutch situations and come up with the exact same value of Z [note the absolute value in the numerator of (1)].

Wood concludes that if we performed the test described above on every player we would find no more hitters classified as "clutch" as pure chance alone would provide. He states that, using the 5% level of significance, "roughly 5% of all batters" would "qualify as clutch hitters" as a result of chance. (Due to the two-sidedness of the test he should say that 2.5% would qualify as clutch hitters and 2.5% would qualify as choke hitters.) In essence, Wood's argument is this: Since we cannot reject the null hypothesis of equality of BAc and BA_n any more times than we would expect to by chance, we must conclude that BA_c and BA_n are equal. What is not taken into account is the power of the test that was used.

III. More Power To 'Ya. Suppose we have a .3125 hitter, and suppose we know for an absolute fact that this hitter hits .125 better in clutch situations than non-clutch situations. What is the probability that in a season of 600 at-bats (10% of which are in the clutch), the test of (1) will provide a significant result (at the 5% level)? The answer is 50.8%. That's right, the odds are 50/50 that this hitter in any season of 600 at-bats will not be classified as clutch, even though we know him to be so. This is what power is all about.

Power can tell us, given a player's overall batting average and number of at-bats in each split, the probability of finding a difference **d** (rejecting H0) in batting average between splits when that difference actually exists (H0 false). Why is this important? Well, Wood tells us that if we took the pool of all .3125 hitters with 600 at-bats we would find approximately 5% of them with the required .125 differential for significance. The power we have just mentioned tells us that among those 95% non-rejections there may be some clutch hitters we were unable to find this time. How, then, can we conclude that clutch hitters do not exist when we are unable to find them when they do? Scattered among the evidence we would like to give supporting the conclusion that clutch hitting does not exist is evidence which may contradict that conclusion; this makes the conclusion untenable.

How, you may ask, can it be that if we know that a player is a clutch hitter we cannot declare him so by using the test described in (1)? The reason is because the "true" differential is a theoretical one. It is not believable to think that this player will achieve this differential every year, sometimes he may exceed it, sometimes he may fall short or even hit worse in the clutch. Sixty at-bats a year in clutch situations leaves a lot of room for variability. Unfortunately, one season's worth of data is not enough to provide the kind of power we need to conclude that clutch hitting does not exist. There may be some real clutch hitters falling through a rather gaping hole.

IV. The Development. Hoping now that the importance of power is understood, this section will attempt to describe how a power calculation is made and present a general form for it. These power calculations pertain only to the test described in (1), and only to those tests with a level of significance of 5%. We will first generalize the notation:

BA_1 = batting average in situation 1.
BA_2 = batting average in situation 2.
BA = overall batting average.
AB = total at-bats.
p = percent of total at-bats spent in situation 1.
d = differential = $BA_1 - BA_2$.

BA_1, BA_2, BA, and hence, d, are all theoretical values. Let $\widehat{BA}_1$, $\widehat{BA}_2$, $\widehat{BA}$ be the observed batting averages over the period of at-bats, AB. To test the difference $BA_1 - BA_2$ for significance we calculate,

$$(2) \qquad Z = \frac{|\widehat{BA}_1 - \widehat{BA}_2|}{\sqrt{[\widehat{BA}(1-\widehat{BA})]*[1/pAB + 1/(1-p)AB)}}$$

or

$$(2A) \qquad Z = \frac{|\widehat{BA}_1 - \widehat{BA}_2|}{\sqrt{[\widehat{BA}(1-\widehat{BA})]/[p(1-p)AB]}}$$

if Z is greater than 1.96 then we declare the difference $\widehat{BA}_1 - \widehat{BA}_2$ significant at the 5% level.

Now we ask the question, what is the probability of getting a significant value from (2), when the true difference between $\widehat{BA}_1$ and $\widehat{BA}_2$ is d (0). Stated mathematically below, we are asking for P':

$$(3) \qquad P' = (Prob(Z > 1.96)$$

This probability is not so easy to find, since the null hypothesis (H0: $BA_1 = BA_2 = BA$) is no longer true, Z is no longer a standard normal variate. It turns out that the formula for P' is:

$$(4) \qquad P' = 1 - \Phi(1.96 - x) + \Phi(-1.96 - x),$$

where

$$x = d/\sqrt{[\widehat{BA}(1-\widehat{BA})]/[p(1-p)AB]}$$

and $\Phi(x)$ is the probability that a standard normal variate is less than x. (The mathematical details of moving from (3) to (4) are provided in Appendix 1.) Values of (x) are tabled in most statistics reference books.

V. Examples.

1. What is the probability of detecting a differential of .125 between situation 1's BA and situation 2's BA for a .3125 hitter with 600 at-bats, 10% of which come in situation 1?

BA = .3125
AB = 600
p = 0.1
d = .125

x = (.125)/√[.3125(1-.3125)]/[.1(.9)(600]

= 1.98

P' = 1 - Φ(-0.02) + Φ(-3.94)
= 1 - 0.492 + 0.000
= 0.508

This is the 50.8% probability quoted in Section III.

2. What is the probability of detecting a differential of .125 between situation 1's BA and situation 2's BA for a .3125 hitter with 600 at-bats, 50% of which came in situation 1?

BA = .3125
AB = 600
p = 0.5
d = .125

x = (.125)/√[.3125(1-.3125)]/[[.5(.5)(600)]
= 3.30

P' = 1 - Φ(-1.34) + Φ(-5.26)
= 1 - 0.091 + 0.000
= 0.91

This example shows what an important role p, the percentage of at-bats in situation 1, plays when determining power. The only difference between examples 1 and 2 is that p was increased from .1 to .5; as a result, the power increases dramatically.

VI Using Power and the "Real" Question. A table was developed which calculated powers in the clutch versus non-clutch split (p = .1) for situations covering a range of values of BA (.220 to .360 by .02), AB's (300 to 600 by 100, and 650), and differentials (.05,.1,.12,.14,.16). This five-page table is available on request from <u>BTN</u> [include a self-addressed, stamped ($0.29) envelope with your request.] As can be seen only three situations [(.220, 600, .16), (.220, 650, .16) and (.240, 650, .16) provide power in excess in 80%.

Statisticians usually like to see powers up around 90%, but sometimes relax their standards to 80%. Even using the relaxed standard it is evident that we cannot use a single season's worth of data to support a claim that clutch hitting does not exist.

Before moving on to show how (4) can be used to determine how many AB's are necessary to achieve a certain power, there is a question which needs to be addressed. Here and in the Wood article, much has been said about significant differentials. However, we are referring to "significant" in a statistical sense. What we have neglected to do is think about significant in the baseball sense. What kind of differential do you require, as a baseball fan, before you consider a particular batting average split important (as opposed to the dreaded "significant")? Does it depend on the split (p)? Discussions of power, at-bats required to achieve sufficient power, etc., should begin with the question of what an important differential is. We would then proceed to find out how many at-bats we would need to achieve sufficient power to detect this difference. Not knowing the consensus of the baseball world on the question of important differentials, this article will proceed using a range of differentials.

VII How Many At-Bats? The equation (4) can be used to determine the number of at-bats required to achieve a certain power given the differential (d), batting average (BA), and the percentage of situation 1 at-bats (p). By making one small assumption and some algebraic manipulation (the details are given in Appendix 2) a formula for total at-bats to achieve 90% power with a significance level of 5% is:

$$(5) \qquad AB = \frac{BA(1\text{-}BA)(10.5)}{p(1\text{-}p)d^2}$$

So for a .260 overall hitter, to detect a .05 difference between clutch and non-clutch batting (p=.1) the number of at-bats required is 8979 (by substitution in equation 5), or roughly 15 (600 at-bat) seasons. To detect the same difference when p=.5 (home versus away, for instance) requires 3232 at-bats, which is more than 5 (600 at-bat) seasons.

Increasing d by a factor of k reduces the AB's required by a factor of k^2, so that if, in the first example of this section, d=.05*2=.1, the number of at-bats to achieve 90% power is 8979/4=2245 (slightly less than 4 600 at-bat seasons). Now you can see how crucial it is to determine d beforehand. Otherwise, we can start with a certain number of at-bats, determine the d for which we can get 90% power, and proclaim that as the important difference. This is patently unfair.

Table 2 shows for a range of values of p, BA, and d how many at-bats are required to achieve 90% power (5% level of significance).

Table 2: At-Bats Neded for 90% Power

(p = 0.1)

BA	D	AB	BA	D	AB
.220	.05	8008	.300	.05	9800
	.10	2002		.10	2450
	.12	1391		.12	1702
	.14	1002		.14	1250
	.16	783		.16	958
.240	.05	8512	.320	.05	10155
	.10	2128		.10	2539
	.12	1478		.12	1763
	.14	1086		.14	1296
	.16	832		.16	992
.260	.05	8979	.340	.05	10472
	.10	2245		.10	2618
	.12	1559		.12	1819
	.14	1146		.14	1336
	.16	877		.16	1023
.280	.05	9408	.360	.05	10752
	.10	2352		.10	2688
	.12	1634		.12	1867
	.14	1200		.14	1372
	.16	919		.16	1050

VIII Conclusion. When attempting to prove that no difference exists between two batting average splits, the issue of power cannot be ignored. This article has shown that a single season's worth of data is incapable of having sufficient power to claim no difference between clutch and non-clutch batting averages. Formulas were presented which enable the reader to calculate the power of test (2) and to determine the at-bats required to achieve a power of 90% given the batting averages, percentage of situation 1 at-bats, and the differential, d (significance level of 5%). The determination of d, the differential one wishes to detect, is the most crucial element when it comes to power. Its determination must be made on a baseball - not necessarily a statistical - level.

[The mathematical appendices are available from BTN; send a self-addressed, stamped ($0.29) envelope if you are interested.]

****

The Threat of a Stolen Base

By Rob Wood
2101 California St, #24
Mountain View, CA 94040

In this essay I will present some very preliminary results regarding the value of a stolen base. I hope to show that, through its "hidden" influences, the running game can indeed be a viable offensive weapon.

Via the linear weights and runs created formulas, sabermetricians have estimated the value of a stolen base attempt to be far less than the general public imagines. The cost of a caught stealing is roughly 0.6 runs, whereas the benefit of a successful steal is roughly 0.3 runs. Using the rule of thumb that an additional win requires 10 additional runs, according to this line of reasoning, even the most successful and prolific base stealers do not contribute very much to their team's pennant chances.

In this essay I will not question that finding. Bill James and Pete Palmer have sufficiently made their case. However, that should not end all discussion on the issue. Many other factors can come into play in a potential steal situation.

The positives include: the pitcher's concentration is divided, more fastballs are thrown in order to increase the odds of throwing out the base stealer, the middle infielders cheat closer to second base in case they need to cover on a steal attempt, and the first baseman has to hold the runner on.

The downside is that the batter is asked to take a lot of pitches in order to give the runner a chance to steal, and thereby often falls behind in the count. Only recently have we discovered how important the count is to the outcome of every at bat.

It is unclear whether the positives outweigh the negatives. You hear both sides of the argument. Maury Wills and his 104 steals allegedly "cost" the 1962 Dodgers the pennant since the second place hitter (Jim Gilliam?) took too many fastballs right down the middle. On the other hand, Tony Gwynn is quoted in George Will's book <u>Men At Work</u> as saying that hitting behind Alan Wiggins in 1984 was a blessing. "Knowing that I'd get all fastballs outweighed the disadvantage of not being able to swing early in the count." [Gwynn hit .351 in 1984.]

It is possible to imagine a situation (e.g. Vince Coleman on first base) in which the probability of an event (e.g. the batter gets a hit) is significantly altered. Note that neither linear weights nor runs created are designed to capture this "situational" effect. Those formulas estimate the value of a single, say, across all possible situations.

Using Project Scoresheet data on the St. Louis Cardinals from 1985-1988 (the last year of data I have), I have conducted a mini-study of the value of having Vince Coleman perched on first base. Let me simply present the data in the following tables, commenting briefly on the salient points of each.

Table 1: Effect of Coleman on 1st Base* on #2 Hitter

	Coleman on 1st base	All other at bats
Batting Ave.	.318	.284
BB/Plate App.	13.4%	8.8%
K/Plate App.	7.7%	9.3%
Plate Appearances	596	2064

* No other runner on base

Sample: All at bats of Cardinals' #2 hitter in 1985-1988 games in which Coleman hit leadoff

In this small sample, we see that Cardinals #2 hitters had a 34 point increase in batting average and walked more often with Coleman on 1st base than in other at bats. Combining the two effects, on base average was a full 62 points higher with Coleman on 1st base. Not shown in the table, slugging percentage was also 24 points higher (so that isolated power was essentially unaltered). In addition, there is no evidence that strikeouts are increased by taking pitches to allow Coleman a chance to steal.

It is interesting to break out the at bats with Coleman on 1st base depending on whether or not Vince stole second base. Table II looks more closely at the 596 plate appearances of the 1st column of Table 1. I do not include the 55 plate appearances during which Coleman was caught stealing or picked off. That sample is too small to make any meaningful comparisons. I should also add that hit-and-run plays are not labelled in the dataset.

Table 2: Breakdown of Effect of Coleman on 1st Base* on #2 Hitter

	No Steal Attempt	Stolen Base
Batting Ave.	.359	.281
BB/Plate App.	8.6%	17.9%
K/Plate App.	5.2%	9.5%
Plate Appearances	268	273

* No other runner on base

Sample: All at bats of Cardinals' #2 hitter in 1985-1988 games in which Coleman hit leadoff

Perhaps not surprisingly, the entire batting average advantage of having Vince Coleman on 1st base at the beginning of an at bat resides in Coleman remaining on 1st base throughout the at bat. This is evidence of hitters receiving a predominance of fastballs and/or the pitcher distraction factor.

Also, the entire on base average advantage resides in Coleman successfully stealing second base. Is the pitcher shaken by the steal? I seriously doubt whether the pitcher is "pitching around" the #2 hitter in order to get to the #3 and #4 hitters.

It may be argued that I am comparing apples and oranges. Knowing that Vince Coleman has reached 1st base, it is likely that the pitcher is not Dwight Gooden or that he does not have his best stuff that day. In addition, having anyone on 1st base opens up the right-side hole. Thus, it is not surprising that the #2 hitter performs better than "average" when Coleman is on 1st base.

To handle this possible explanation of the above results, I tracked all Cardinal hitters who had more than 100 plate appearances with Coleman on 1st base with 2nd base unoccupied, and also more than 100 plate appearances with some other runner on 1st base with 2nd base unoccupied. It turns out that there are only two candidates, Willie McGee and Ozzie Smith.

For reasons which will become apparent, I have presented the data in two separate tables. Table 3 gives the individual splits when McGee batted in a steal situation, and Table 4 gives the same splits when Ozzie batted in a steal situation.

Table 3: Effect of Coleman on 1st Base Relative to Other Runners on Willie McGee

	Coleman	Other
Batting Ave.	.295	.296
BB/Plate App.	6.9%	4.3%
Plate Appearances	102	346

Sample: All at bats of McGee with a runner on 1st base with 2nd base unoccupied, 1985-1988

We see that Coleman created virtually no greater advantage for McGee than any other Cardinal base runner. McGee's batting average was virtually unchanged, and his on base average was only 16 points higher. Such is not the case for Ozzie Smith, below. In my small sample, Ozzie hit a phenomenal .369 with Coleman on 1st base, compared to "only" .316 with any other Cardinal on 1st base. Smith's added propensity to walk with Coleman on 1st base gave him a .421 on base average, compared to "only" .367 in the control sample.

Preliminary conclusions: To be perfectly honest, I chose to focus the mini-study on Vince Coleman, the most prolific base stealer in the major leagues (over the years for which I have data), hoping to find no effect whatso ever. I was hoping to reinforce the previous sabermetric finding that stolen bases are vastly overrated as an offensive strategy.

Table 4: Effect of Coleman on 1st Base Relative to Other Runners on Ozzie Smith

	Coleman	Other
Batting Ave.	.369	.316
BB/Plate App.	8.2%	7.5%
Plate Appearances	171	226

Sample: All at bats of Ozzie with a runner on 1st base with 2nd base unoccupied, 1985-1988

However, these preliminary data do not rule out the possibility that the threat of a stolen base actually is quite valuable since the batter is apt to see many fat fastballs. In fact, if the numbers appearing in my mini-study are at all representative, it may turn out that the threat of a steal is more valuable to the offense than a successful steal.

I have no plans to address the deeper issue of why pitchers would "allow" the threat of a stolen base to have greater value than a successful steal itself! Please send any comments or suggestions on ways to "extend" this research into a full-fledged study to the editor of this newsletter.

****

Muscular Mendoza Index: Separating the Keelers from the Kingmans

By Bob Davis
1504 Hunters Lake East
Cuyahoga Falls, OH 44221

Introduction. With the recent hitting statistics compiled by Mark McGwire, many casual fans may be wondering about past players with low batting averages but high home run production. Muscular Mendoza Index (or MMI) is an attempt at quantifying power relative to batting average. It is useful in separating players with high averages and little power from those with plenty of pop whose batting averages hover close to the Mendoza line (i.e., the "Muscular Mendozas").

One way to measure the power of a player without figuring in batting average is the isolated power statistic. Isolated power, first suggested by Bill James, is found by subtracting a player's batting average from his slugging percentage. Of course, isolated power is not sufficient for the purpose here. When one considers the fact that Billy Williams and Tony Armas have virtually identical career isolated power statistics, it is apparent that isolated power is only part of what is needed to separate the Muscular Mendozas from the more well-rounded hitters.

An early attempt was to look at isolated power divided by batting average. If a player has high isolated power and a low batting average, this quotient will be large; if he has little power and a high batting average, the quotient will be small. This figure was calculated for several players before the problem with it became apparent. If a player has a batting average of .200 and a slugging percentage of .400, he would be placed in the same category as a player with figures of .300 and .600 - even though the second player has an average nowhere near the Mendoza line. Specifically, consider Babe Ruth (.342, .690) and Dave Kingman (.236, .478). They would be rated as virtually equal (Ruth: 1.02, Kingman: 1.03) by such a measure.

In order to further emphasize batting average, the statistic

$$\text{(1)}\quad MMI = \frac{\text{Isolated Power}}{(\text{Batting Average})^2}$$

was used. With this measure, a low batting average will result in an extremely low denominator - thus, the MMI statistic will be large. If isolated power is low and batting average is high, then the statistic will turn out to be quite small. This statistic turned out to be much more efficient at separating the Muscular Mendozas from the rest of the pack.

All-Time MMI Leaders. Career MMI was calculated for all players with over 5000 major league at-bats. The following table contains a list of major leaguers with a career MMI of at least 3.0. These men may be thought of as the "Muscular Mendozas."

Note that it is not an insult to be a "Muscular Mendoza." Five of these players (Killebrew, Kiner, McCovey, Mathews, and Stargell) are in the Hall of Fame, and two more (Schmidt and Jackson) are certain to be future inductees. A sixth Hall of Fame member, Hank Greenberg, nearly made the list

Table 1: All-Time MMI Leaders

Player (years)	BA	SA	MMI
Kingman (71-86)	.236	.478	4.345
Killebrew (54-75)	.256	.509	3.860
Schmidt (72-89)	.267	.527	3.647
Kiner (46-55)	.279	.548	3.456
McCovey (59-80)	.270	.515	3.361
Allison (58-70)	.255	.471	3.322
Jackson (67-87)	.262	.490	3.321
Mathews (52-68)	.271	.509	3.241
Maris (57-68)	.260	.476	3.195
Armas (76-89)	.252	.453	3.165
Colavito (55-68)	.266	.489	3.152
Stargell (62-82)	.282	.529	3.106
Thornton (73-87)	.254	.452	3.069
Howard (58-73)	.273	.499	3.032

with an MMI of 2.981. MMI measures the extent to which a player's power production is "out of proportion" to his batting average. For Willie Stargell to make the list despite a batting average of .282 should be viewed as further evidence of his power rather than as an indictment of his skills. Perhaps the statistic should be given a more appealing name.

Also of note is the fact that all but one of these men (Kiner) played in the sixties and/or seventies, when power hitters and power pitchers were the dominant players on the scene.

Among active players with enough at-bats to qualify, Lance Parrish (2.99), Jack Clark (2.92), and Dale Murphy (2.90) could make this list with one or two more power-packed, low average seasons. Tom Brunansky checks in with an MMI of 3.138, and will join the Muscular Mendoza club upon his 57th at-bat of 1991.

Some other former players who did not collect enough at- bats but proved to be hitters with low averages and high power include former Padre first baseman Nate Colbert (3.522), Willie Kirkland (3.160, and hit 21 homers despite a .200 average in 1962), and the unofficial record holder, Gorman Thomas, who compliled a mammoth career MMI of 4.405. Thomas only collected 4677 at-bats, so until MMI's have been calculated for all players with more than 3000 at-bats this cannot be considered an official record.

All-Time "Judies". At the other end of the spectrum are the truly powerless hitters, affectionately referred to here as the "Judies." Here is a complete list of players with over 5000 at-bats and a Muscular Mendoza Index of 0.80 or less.Note that the majority of the all-time Judy leaders were active around the turn of the century. This should come as no surprise to those familiar with baseball history. Players such as Matty Alou, Glenn Beckert, and Maury Wills were unusual for their era but would have been perfectly at home in 1900.

Outstanding Single Seasons. Dave Kingman, the most prolific MMI player in history, dominates the single-season list with six of the top ten performances among those players listed above as career leaders. Kingman compiled MMI's of 4.5 or greater an incredible seven times (1972, 75, 76, 77, 81, 82, and 86) in seasons in which he qualified for the batting title. His best performance was in 1982. Playing for the New York Mets, Kingman hit .204 and slugged .432 (37 homers), resulting in a 5.48 MMI. Harmon Killebrew's 1962 cam

Table 2: All-Time MMI Tail-Enders			
Player (Years)	BA	SA	MMI
R. Thomas(99-11)	.290	.333	0.511
P. Donovan (90-07)	.300	.354	0.600
M. Wills (59-72)	.281	.331	0.633
W. Keeler (92-10)	.345	.420	0.637
M. Huggins (04-16)	.265	.314	0.698
F. Tenney (94-11)	.295	.359	0.740
B.Hamilton (88-01)	.344	.432	0.744
F. Jones (96-15)	.285	.347	0.769
S. McInnis (09-27)	.308	.381	0.770
G. Beckert (65-75)	.283	.345	0.774
R. Ashburn (48-62)	.308	.382	0.780
L. Waner (27-45)	.316	.394	0.781
M. Alou (60-74)	.307	.381	0.785
D. Bush (08-23)	.250	.300	0.800

paign (.243, .545, 48 homers) takes second place at 5.11. Gorman Thomas' 1985 season (.215, .450, 32) produced an MMI of 5.08. While not all single seasons have been checked, Kingman, Killebrew, and Thomas are the only former players known to have compiled single-season MMI's exceeding 5.

In the Judy category, Willie Keeler holds the single-season record among the players listed as all-time Judies. In 1898, Willie hit .385 for Baltimore. Among his 216 hits were seven doubles, two triples, and one home run. With a slugging percentage of .410, Willie's MMI for that season was an astounding 0.16! In today's game, it is hard to imagine a player threatening this mark. Evidently they didn't call him "Wee" for nothing. Honorable mention goes to career leader Roy Thomas. In 1900, Thomas hit .316 and slugged .335, resulting in an MMI of 0.19. Among his 168 hits were 161 singles. According to the Baseball Encyclopedia, Thomas weighed only 150 pounds. If he had only had a catchy nickname like Keeler ("Runty Roy?") maybe fans would still be talking about him today.

Results from 1990. Rob Deer joined the exclusive "MMI-5" club of Kingman, Killebrew, and Thomas in 1990. After threatening the hallowed 5 mark in both 1986 (4.87) and 1989 (4.88), Deer smashed the barrier with an astounding 5.11 MMI. Based on the research

Table 3: 1990 Leaders			
AL Top Ten, 1990		NL Top Ten, 1990	
R. Deer	5.11	J. Clark	3.77
M.McGwire	4.60	E. Davis	3.34
G. Vaughn	4.38	L. Walker	3.32
C. Fielder	4.11	H. Johnson	3.19
J. Canseco	3.58	D. Justice	3.18
J. Barfield	3.47	F. Stubbs	3.14
P. Incaviglia	3.45	D. Strawberry	3.14
B. Jackson	3.39	B. Bonilla	3.04
R. Milligan	3.23	K. Mitchell	3.02
M.Tettleton	3.18	J. Carter	2.95

currently completed, this ties him with Killebrew for the second-highest single-season MMI of all time. Like many other baseball records, history was made and no one was even aware of it at the time.

Much has been made of the fact that the Tigers have collected strikeout kings Deer, Fielder, Incaviglia, and Tettleton on the same offense. While I have not calculated team MMI records, the Tigers are a good bet to collect a team MMI of historical significance. Most of the team marks in this category are probably held by teams from the all-or-nothing period of the sixties and early seventies.

The top 1990 Judies are shown in Table 4. While Al Newman had the most punchless season (80 singles, 14 doubles, no triples or homers) of any regular in 1990, he still collected a higher MMI than Roy Thomas or Patsy Donovan did for their entire careers. Jerome Walton should receive a special award for ranking in the top ten despite playing his home games in Wrigley Field.

Conclusions. Muscular Mendoza Index is meant to be a "fun" statistic. It rates players not according to perceived value, but according to whether the player is an "All-or-nothing" hitter or a "Punch-and-Judy." While not meant to be a tool for any serious analysis, any quantity which places Dave Kingman and Gorman Thomas at one end of the spectrum and Willie Keeler and Matty Alou at the other has to have some merit. While the accomplishments of current sluggers Mark McGwire and Rob Deer are not unprecedented, both players could rank high on the all-time MMI list by the ends of their careers. No current player appears to have much chance of becoming an all-time Judy.

Table 4: 1990 Lagards

AL Judies, 1990		NL Judies, 1990	
A. Newman	0.61	E. Yelding	0.67
J. Huson	0.69	L. Harris	0.76
L. Polonia	0.69	B. Butler	0.79
C. Lansford	0.72	O. Smith	0.79
F. Fermin	0.73	Sharperson	0.86
O. Guillen	0.80	W. McGee	0.91
W. Weiss	0.80	J. Uribe	0.91
L. Johnson	0.89	C. Biggio	0.95
S. Sax	0.96	J. Walton	0.95
A. Espinoza	1.00	A. Griffin	1.00

****

Disciplined Power Ratio

By Jonathan L. Katz
3024 N. Calvert St.
Baltimore, MD 21218

In examining the issue of determining how accurate an eye a batter has, Jorgen Rasmussen (By the Numbers, Vol. 3, No. 1) suggested a statistic that divides strikeouts by homeruns, Disciplined Power Ratio (DPR). The inclusion of homeruns in the statistic is an attempt to develop a statistic that assesses disciplined power (power with a low likelihood of strikeouts), as the concept of accuracy of pitch judgement may be interpreted in many ways. As Rasmussen notes: "While DPR may well involve skills in addition to batting eye, it certainly seems related to a hitter's ability to select 'his' pitch..."

Rasmussen selected batters with career averages of at least 5.8 homeruns per 100 at-bats and those with career averages of at least 27 at-bats per strikeout. Of those individuals I eliminated Johnny Cooney (DPR = 53.5), since his DPR was an extreme value. I examined the relation between DPR and several measures of batting eye that were previously examined (By the Numbers, Vol. 2, No. 3). The statistics examined were: 1) Batting Eye Index (BEI; Gagnon; Baseball Research Journal, Vol. 17) which divides the difference between walks and strikeouts by games played; 2) BEI3, which divides the difference between walks and strikeouts by the sum of walks and strikeouts; BB proportion, which divides the number of walks by an approximation of the number of plate appearances (at-bats + walks); and, 4) K proportion, which divides the number of strikeouts by the same approximation of the number of plate appearances.

In short, none of the four measures of batting eye were correlated with DPR among the batters examined. In addition, the correlation of DPR with batting average was insignificant ($r2 = 0.01$). Obviously, the inclusion of some aspect of power hitting in the statistic introduces a factor that, by design, was not included in any of the four statistics previously examined. The utility of any new statistic is its ability to isolate a particular characteristic for measurement. Answers to questions about baseball can be best answered with statistics that vary with change across a single dimension.

One important limitation of the present comparison of these statistics is the pre-selection of batters on the basis of their homerun or strikeout ratios. The conclusion that there is no relation between DPR and the four statistics described above may be limited to those batters that hit homeruns with a high frequency or strikeout with a low frequency. A relationship between the statistics may in fact hold if all batters were examined. However, the exceedingly low correlations obtained are suggestive that no correlation would prove to be general.