By the Numbers

Volume 11, Number 2 The Newsletter of the SABR Statistical Analysis Committee May, 2001

The Committee Chair

Committee News

Neal Traven, Committee Co-Chair

I'd like to take a moment to greet the numerous Statistical Analysis Committee members reading their first *BTN*. We've had an appreciable influx of new members in recent months, nearly three dozen since I last performed an alphabetical sort of the roster. In some (too many) cases, you might have received one or more previous issues had I been better at keeping track of the committee roster in the middle of my job search and cross-country move. If you feel like that describes you, I'd be happy to forward you a back issue or two … just drop me an email.

When Clem Comly and I became committee co-chairs in 1996, our membership roster contained something like 180 names. One of my first acts as custodian of the roster was to cull out the deadwood of SABR dropouts and other such disappearances. I'm sure there are still some of those sorts sprinkled through the membership list, but I'm pleased to report that my roster spreadsheet currently contains 321 name/address lines!

In future reports, we'll look at increasing the SAC's web presence (Daniel Levine, someday soon I'll get to that message from DMLCo), at potential responses to Rob Neyer's challenging concept of creating a sabermetric research repository (see Rob's January 26, 2001 column at espn.com), and more.

In the meantime, I'm cc:ing all of you on my letter to Publication Director Mark Alvarez concerning the continuing saga of statistical articles in SABR publications.

Dear Mark,

After the release of *Baseball Research Journal 29*, a number of SABR members began asking pointed questions about the Society's editorial policy with regard to "statistical" articles. The emails flew across a number of SABR discussion lists, official and unofficial. The principal, but not the only, subject of these queries is the article by the Heatons, which is (to be blunt) methodologically unsound and in no way reflective of the sabermetric state of the art. Several postings recalled that you and I have often expressed the desire to encourage SAC peer review, and thus they wondered how the Heaton article was handled by that review process. In a February 15, 2001 message to the brsp discussion list I reported that the Heaton paper had, to my knowledge, undergone no SAC review whatsoever. Needless to say, this information did not sit well with those who questioned whether the Heaton paper belongs in *BRJ*.

In this issue

Frankly, it doesn't sit well with me either. Clearly, we have not established anything close to a workable peer review process for statistical papers. I'll take some share of the blame for our failure – my turnaround time for reviewing the articles you sent in the past couple of years was less than speedy – but neither I nor anyone else in the SAC can review articles we don't see. I am unaware of any manuscripts subjected to SAC peer review for *BRJ 29*.

The non-system we talked about building over the last couple of years is an abject failure. Therefore, I suggest starting afresh. Acknowledging that statistical papers remain the most problematic, I believe that what is needed is a peer review structure to be applied to *all* papers submitted to you by SABR researchers. It is time (some would say *past* time) to raise the bar for research throughout SABR's subject matter, not just in statistical analysis, and peer review is a vital means to that end. I propose introducing a more systematic and standardized peer-review procedure for the Society, using a model based on the peer-review process used in scholarly journals. A few salient points:

Overview -- your role as Publications Director (PD) becomes one of executive editor or publisher rather than day-to-day direct editor of manuscripts. You marshal SABR resources to establish standards for review, oversee the distribution of manuscripts to reviewers, and act as the go-between linking authors and reviewers in the iterative process of editorial review. Importantly, you remain the final decision-maker directing and shaping the content of SABR's publications. As I envision this approach, your workload will certainly change (more administration/organization, more high-level focus on research directions, far less wielding of the red pencil) and perhaps become somewhat lighter on a day-to-day plane.

Review bodies (RB) – these are the entities within SABR which review the research of their peers. By definition, all research committees are RBs. In addition, many regional chapters may want to be resources for reviewing works directly related to their locales, and other SABR entities may also wish to play a role in peer review. Each RB develops its own approach to selecting its pool of reviewers … by area of expertise, perhaps, as well as level of interest in participating.

Manuscript review – you carry out the initial assessment, briefly scanning the submission immediately on receipt. This step weeds out materials patently inappropriate for the Society's general publications; I'm sure you already do something of this sort. You might redirect some highly technical or extremely narrow papers to a specific committee for consideration by its newsletter editor rather than *BRJ*, but with a real peer-review process in place I don't think there will be a large number of those.

To guide you in directing a submitted manuscript to appropriate reviewers, the author completes a form, rank-ordering the RBs that in his/her opinion best match the paper's topic(s). The author can also suggest individual reviewers, or point out topic areas not covered by any existing research committee (Japanese baseball, for instance). Examples: (a) the author of "Park effects in the Pacific Coast League" checks off the Ballparks, Statistical Analysis, and Minor Leagues committees (in that order), along with PCL expert Dick Beverage; (b) the author of "Barney Dreyfuss on the Ohio River" chooses Biographical and Business. Based on the author's selections, as well as your own judgment, you refer the manuscript to RBs for their assessment; I can't imagine skipping over the author's top-ranked RB, though you might choose not to involve some secondary RBs. In the second example above, you might ask the liaison from the Pittsburgh and/or Louisville chapter to assign a reviewer for the paper in addition to (or instead of) the author's suggestions. Note that each RB's liaison, rather than you, is the "associate editor" assigning individual reviewers, and that the reviewers will know the manuscripts only by the code numbers you've assigned.

Blinding, openness, disclosure – the review process in respected scholarly journals is nearly always "double blind" … reviewers are not informed of the identity of authors, and authors do not know who has reviewed their work. As managers and organizers of the flow of reviews and reports, you and the RB liaisons *would* know who was who, if only to assure that author and reviewer aren't one and the same! To maintain this blindness, it would be highly inappropriate for you or an RB liaison to act as a peer reviewer. Even without specific identifiers, a qualified reviewer may have more than an inkling about who wrote a manuscript under review. In such cases, it's up to the reviewer to decide whether to recuse him/herself.

Another vital attribute of this process is that all reviews and critiques are returned to the author. Again, you and the RB liaison are the links between the two sides of the double-blind equation. Just as the reviewer's criticisms are not colored by knowledge of who penned the manuscript, the author has no information about who may have criticized (or lauded) the work. In scientific journals, the peer review process may go through several iterations as the author revises to address the reviewers' criticisms. The goal is to obtain a forthright evaluation of the quality of the research, irrespective of personal relationships or conflicts. In the end, what emerges is a stronger product, tested and tightened … a contribution to the research community.

We haven't yet put together any sort for workable process for the submission of quality statistical papers to *BRJ*. That deficiency must be rectified. I can assure you that, despite the frustrations many have voiced, the SAC wants to improve and expand its standing in SABR's publication universe. Thanks in no small measure to Phil Birnbaum's editorial skills with *BTN*, many of our members are eager to offer reviews of research manuscripts. When Phil recently emailed a request for reviewers of a submission to *BTN*, over a dozen committee members offered their assistance, most of them on the same day as Phil's request.

To be honest, I do not know how much peer review already exists in the current editorial process. Perhaps a streamlined version of the above – cutting out the RB/liaison layer, not double-blind – is already in place for fields where you feel more comfortable as an editor. Even if review and evaluation are qualitative rather than quantitative, the peer review principle is the same. I'm not asking for special treatment of statistical papers. On the contrary, I'd like all SABR research to be held to the highest of standards. That standard calls for a real system of double-blind peer review of all materials sent to *BRJ*.

The [brsp] discussion of *BRJ29* played a large role in working out this proposal. In particular, I appreciate the thoughtful words of Mark Armour, Mike Emeigh, Ted Turocy, and Paul Wendt. Any and all of us would be happy to discuss this issue further, whether face-to-face in Milwaukee or via email or on the phone.

Neal Traven, 4317 Dayton Ave. N. #201, Seattle, WA, 98103-7154, beisbol@alumni.pitt.edu ♦

Academic Research: Home Field Advantage, Manager/Player Fit

Charlie Pavitt

The author summarizes academic research on what might cause home-field advantage, and on what extent the fit between the manager and his players affect the team's performance

These are two in a series of occasional reviews of sabermetric articles published in academic journals. It is part of a project of mine to collect and catalog sabermetric research, and I would appreciate learning of and receiving copies of any studies of which I am unaware. Please visit the Statistical Baseball Research Bibliography at *www.udel.edu/johnc/faculty/pavitt.html*, use it for your research, and let me know what I'm missing.

D. Randall Smith, Anthony Ciacciarelli, Jennifer Serzan, and Danielle Lambert, Travel and the Home Advantage in Professional Sports, Sociology of Sport Journal, Vol. 17, 2000, pages 364-385.

One factor that all team sports share is the home court advantage. Baseball seems to be the least affected, with the home team winning about 54 percent of the time, compared to the 67 percent found in some analyses of other sports. Disproportionate home team success has been attributed to several factors, including crowd support and knowledge of the idiosyncrasies of the home field, but players also believe that the visiting team is at a marked disadvantage due to the rigors of travel. Nonetheless, past studies in various sports (baseball examples include Schwartz and Barsky, *Social Forces*, Vol. 55, 1977, pp. 641-661, and Courneya and Carron, *Journal of Sport and Exercise Psychology*, Vol. 13, 1991, 42-49) have found little support for the belief, with visiting team travel miles accounting for less than 1 percent of the variance in team winning percentage. Keep in mind, however, that if home team advantage in baseball is only 54 percent, then all the relevant factors together only account for 4 percent of variance in winning percentage, so travel might yet be found to be critical.

The authors (a sociology professor and three undergraduates) believe that further analysis is warranted for two reasons. First, previous studies have ignored team quality; perhaps poor teams are particularly susceptible to disadvantages from travel. Second, the home team may also have started a series after travel, negating the impact of the factor for at least that first game. In their study, they included the 1996 and 1997 seasons of NBA, NHL, and MLB games. These years are particularly well chosen in the case of baseball, because 1997 marked the beginning of interleague play, which has forced short series and thus more travel on teams. The result is a "natural experiment" in the authors' terminology, in which any travel impact should be greater in the second year than the first.

Limiting my discussion to baseball, the results showed the usual home team advantage of 54 percent for both seasons. Travel mileage was indeed greater in 1997. Once again, the impact of travel accounted for almost no variance in winning percentage. Interestingly enough, the number of days off before the first game in a series actually helped the traveling team despite the fact that the home team was less likely to be coming in off the road. More intuitively, in 1997 the length of the road trip did hurt the traveling team.

The moral of the story seems to be that, with this one additional study added to the literature, we still have no good idea what causes home team advantage.

Richard Prisinzano, Investigation of the Matching Hypothesis: The Case of Major League Baseball, Journal of Sports Economics, Vol. 1, 2000, 273-298.

I was unaware of the newly-minted *Journal of Sports Economics*, as it is not yet listed in the journal index on which I rely. Therefore, I was pleased when Richard Prisinzano sent me a copy of his article from that journal for inclusion in the Bibliography. The matching hypothesis is the supposition that organizational performance is a function not only of worker characteristics and manager characteristics but also of the specific match between the two, which of course will be idiosyncratic to the situation. To use the author's own example, the performance of the 1970s Orioles was partly a function of the players, partly a function of Earl Weaver, and partly a function of the unique fit between the players and Weaver. Earlier studies have led to inconsistent conclusions, with Chapman and Southwick (American Economic Review, Vol. 81, 1991, pp. 1352-1360) finding evidence in support from American baseball, Ohkusa and Ohtake failing to find evidence in support from Japanese baseball (*Japan and the World Economy*, vol. 8, 1996, pp. 475-488 and *Journal of the Japanese and International Economies*, Vol. 8, 1994, pages 204-219). The previous researchers, however, made some questionable methodological decisions; for example, Chapman and Southwick used team strikeout-to-walk ratio as their measure of pitching effectiveness under the presumption that, in contrast with other possible measures, it is least contaminated by factors not under the pitcher's control.

Prisinzano used MLB data from 1901 to 1992, representing offensive performance by team SA + OBA, defensive performance by opponent's SA + OBA, and managerial effectiveness by team actual wins minus expected wins (A-E). It strikes me that A-E is a particularly interesting choice for this context, as long-term tendencies toward a plus versus minus A-E have been interpreted as representing in-game managerial skill. Therefore, if Prisinzano is correct, then A-E must be taken as measuring both managerial skill and manager/player match, and conclusions solely about managerial skill based A-E are problematic. The study results are consistent with Chapman and Southwick in showing clear support for the player/manager match as significant beyond the impact of players and managers alone.

This is a potentially important finding, as it suggests that we must take this match much more seriously in evaluating managers. It inspired me to take a look at a couple of obvious past examples using data from the sixth Total Baseball edition. Casey Stengel's A-E was -2.7 in three years managing the Dodgers, a surprising high +7.4 in six years with the Braves, a surprisingly (for me) low +5.9 in 12 years with the Yankees, and a predictable -15.4 in four years with the Mets. Gene Mauch was +4.3 in 9 years with the Phillies, +3.7 in 7 years with the Expos, an atrocious -18.8 in 5 years with the Twins, and -2.1 in 5 years with the Angels. Are these differences across teams random happenings, or is something significant going on?

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If you're not a member of the Statistical Analysis Committee, you're probably reading a friend's copy of this issue of BTN, or perhaps you paid for a copy through the SABR office.

If that's the case, you might want to consider joining the Committee, which will get you an automatic subscription to BTN. There are no extra charges (besides the regular SABR membership fee) or obligations – just an interest in the statistical analysis of baseball.

To join, or for more information, send an e-mail (preferably with your snail mail address for our records) to Neal Traven, at beisbol@alumni.pitt.edu. Or write to him at 4317 Dayton Ave. N. #201, Seattle, WA, 98103-7154.

Neal Traven Address Change – Correction

Neal Traven, co-chair of the Statistical Analysis Committee, has moved as of March 8, 2001. His new addresses were printed incorrectly last issue. Here they are again:

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Trends in Career Pitcher Ball-in-Play Average

Duke Rankin

Following the Voros McCracken study that suggested that batting average allowed on balls in play is not a true skill of a pitcher, the author revisits the question in examining how this statistic varies throughout pitchers' careers.

Introduction

One attribute often ascribed to the art of pitching is inducing batters to hit bad pitches, creating routine chances for fielders. According to this hypothesis, good pitchers should have a greater success rate turning balls-in-play into outs than poor pitchers. Recently, Voros McCracken, on www.baseballprospectus.com, has presented data suggesting that, for any given pitcher, seasonal ratios of hits to balls-in-play do not exhibit significant correlations. This suggests the pitcher's success in turning balls-in-play into outs more closely resembles a random process than a measure of the pitcher's ability. I seek to examine this hypothesis by analyzing career totals for several outstanding pitchers in the post-war era. My hypothesis is that a pitcher's seasonal hits-per-balls-in-play ratio (hereafter abbreviated HPBP) is a skill and should therefore follow a learning curve: pitchers should exhibit high HPBPs early in their careers, and low HPBPs later in their careers.

Methods

To maximize the time period available for changes in HPBP within a pitcher's career, I sampled only pitchers with 3600 or more career innings. To eliminate variation associated with wartime ball and the deadball era, I sampled only pitchers who began play after 1946. To minimize variation associated with park effects and defensive changes, I sampled only pitchers with long service with a single club -- for this study, arbitrarily set at 8 consecutive seasons, beginning with the first season a pitcher threw 140+ innings. I used only the seasons of long service; Don Sutton, for example, enters the study only for the 15 consecutive seasons he pitched for the Dodgers. To determine age classes, I calculated the age of the pitcher as of July 1.

Using the leaders for career innings listed in the eighth edition of the Macmillan Encyclopedia (1990), 15 pitchers met the criteria for the study: P. Niekro, G. Perry, Sutton, Spahn, Carlton, Ryan, Seaver, Roberts, Wynn, Kaat, Palmer, Gibson, Koosman, Lolich, and Friend. Ryan pitched 8+ consecutive seasons for two teams and therefore entered the study twice. All the pitchers retired before the 1990 season, allowing me to examine their entire careers.

For each pitcher, I estimated the number of outs per season by multiplying innings pitched by three. I estimated balls-in-play using the following formula:

```
[1]    Balls in Play = Estimated Outs + Hits - Strikeouts
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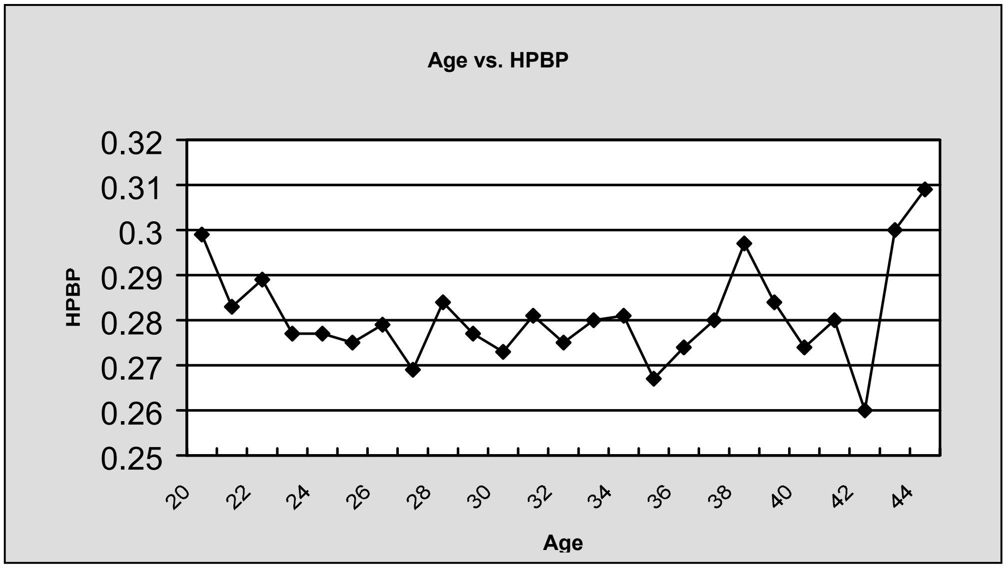
I summed the variables for each age class in the study, and calculated HPBP by dividing the sum total of hits by balls-in-play. I then regressed HPBP onto age class using the Statview statistical package.

Results

HPBP declined rapidly at the beginning of the sample -- from .291 in age class 20 to .277 in age class 23. HPBP did not, however, exhibit a linear correlation with age class ($p = 0.64$, $R^2 = 0.01$). Between age classes 23 and 36, HPBP remained fairly constant, typically between .275 and .280 (range = .267 to .284). After age 36, HPBP exhibited a general increase, reaching the highest levels in the sample in the oldest age classes -- .300 in age class 43 and .309 in age class 44. (See figure, next page.) Because HPBP was highest at the edges of the sample and lowest in the center, the data regressed significantly onto a quadratic model ($p = 0.023$, $R^2 = 0.289$).

Incidentally, the lowest HPBP in the sample (.260) occurred in the 42-year age class. The age class contained only two seasons. In 1963, Warren Spahn had one of the best years of his career, going 23-7 with an ERA of 2.60, 22 complete games, and 7 shutouts. In 1981, Phil Niekro also pitched well, although he pitched only 139 innings. Eliminating this age class from the analysis -- the data certainly appeared to be atypical -- substantially increased the strength of the regression ($p = 0.0002$; $R^2 = 0.548$).

Discussion



The data are consistent with qualitative notions of pitching careers. Young pitchers yield a high proportion of hits on balls in play. As they learn to pitch, the proportion of hits on balls in play decreases. Learning proceeds rapidly at first, then diminishes as the pitcher enters mid-career. Older pitchers eventually lose their stuff, and even well-placed pitches are increasingly hit safely.

In his article, McCracken found high levels of variation among seasonal HPBP totals. I have also found high variation in comparison to the mean responses. Although the quadratic regression is significant, the amount of variation captured by the model is modest; the predictive value of the analysis is poor except at the extreme ends of a pitcher's career.

Duke Rankin, 136 Indigo Lane, Calera, AL, 35040-4646, rankind@montevallo.edu ♦

Book Reviews Wanted

Every year, a number of books and magazines are published with a Sabermetric slant. Many of our members have never heard of them. Our committee members would like very much to hear when this kind of stuff comes out.

If you own a copy of any baseball book of interest, we'd welcome a summary or a full-length review. The only restriction, please: the book should have, or claim to have, some Sabermetric content.

For a sample of what we're looking for, check out David Shiner's review two issues ago, or Gabe Costa's review in the issue before that.

Send reviews to the usual place (see "Submissions" elsewhere in this issue). Drop me a line if you want to make sure no other member is reviewing the same publication, although multiple reviews of the same book are welcome, particularly for major works. Let me know which book you're doing, so I don't assign the same book twice.

And if you're an author, and you'd like to offer a review copy, let me know – I'll find you a willing reviewer.

Defense Independent Pitching Stats – An Update

Clifford Blau

The author summarizes various studies that followed the seminal Voros McCracken research on pitcher hits-allowed-on-balls-in-play.

A year ago, I wrote a brief review of Voros McCraken's article, "Defense Independent Pitching Statistics." In this article, Mr. McCracken concluded that the ratio of hits allowed on balls in play (hereafter referred to as HPBP) is outside the control of the pitcher. This finding has generated a lot of discussion and research in the interim. In this article, I will summarize some of this.

As mentioned in my review last year, some subsequent research by Mr. McCracken showed that pitchers' career HPBP is a better predictor of their following season's HPBP than their current season HPBP is. Subsequently, Craig Wright wrote in a letter to Rob Neyer that a group of knuckleball pitchers he selected had a collective career HPBP .020 lower than the league average. (I selected a larger group of knuckleballers, covering all years from 1952 to 1993 and nearly 20,000 innings and found a difference of .013, using a weighted league average.) Rob Neyer's response indicated that he selected a group of four hard-throwing closers, and they were even further below the average.

Keith Woolner also studied career records. He looked at all pitchers between 1979 and 1999 who allowed at least 3000 balls in play (pitchers). These pitchers's HPBP were compared to their team's HPBP, and their careers were divided into even and odd years. The correlation between the even and odd years was .53, meaning about 28% of the variation is explained by the pitcher's skill. 24% of the pitchers were more than two standard deviations from the mean.

Eric Van, in the internet Usenet discussion group rec.sport.baseball, found that pitchers with good control tend to have a higher HPBP, and wild pitchers have a lower one. Although the difference is statistically significant, it is very small, such that it makes a difference of only .003 or .004 in the most extreme cases.

Mr. Van, in a subsequent post, reported on a study he did of all 20th Century Major League pitchers who changed teams between seasons and pitched at least 110 innings each year. He found that nine percent of the variance in HPBP is explainable by the pitcher's skill, while twenty-eight percent is due to the park and league he pitches in. The balance is due to unknown factors or luck.

Finally, Mr. Van, again in rec.sport.baseball, determined the effect that managers and ballparks have on team HPBP. Using all 20th Century Major League teams, he found that those two factors explain about 34 percent of the variance in that statistic. Although he didn't determine how managers influence the HPBP, he did show that many of the biggest year-to-year changes in HPBP were associated with managerial changes, and that in most of those cases there wasn't a big alteration in the player personnel.

Based on the above, it appears that pitchers do have some influence on their HPBP, although other factors overwhelm their skill in this regard. Thus, for a given season, one cannot predict a pitcher's HPBP with any expectation of success.

Some interesting figures I noted in my knuckleball study:

	American League		National League	
	LgBA	LgHPBP	LgBA	LgHPBP
1962	.255	.263	.261	.274
1963	.247	.260	.245	.264
1964	.247	.263	.254	.272

The changes between 1968 and 1969 were almost the exact opposite as what was seen between 1962 and 1963. Despite the increase in the strike zone in 1963, the league batting averages dropped by .008 and .007 between 1962 and 1964, while the HPBP decreased by only .002 in the NL, and not at all in the AL. This implies that the size of the strike zone does not have a large effect on HPBP; much of the drop in batting average was due to an increase in strikeouts and a decrease in home runs.

Clifford Blau, 16 Lake St. #5D, White Plains, NY, 10603,brak@erols.com. ♦

Factors Affecting Pitcher Ball-in-Play Average

Phil Birnbaum

Building on the Voros McCracken study that suggested that pitcher hits allowed on balls in play (HPBP) is a weak indicator of pitcher quality or skill, the author studies what factors affect pitcher HPBP, and to what extent a pitcher's HPBP repeats from year-to-year.

Introduction

Recently, a gentleman named Voros McCracken published an article (http://www.baseballstuff.com/fraser/articles/dips.html) asserting that the ability to produce outs when a ball is put in play (and when it's not a home run) is not a skill. That is, once the ball is put in play, and it's not a home run, the ball has roughly the same chance of dropping in for a hit, whether the pitcher is Pedro Martinez or Phil Huffman. If this thesis is correct, then great pitchers are great pitchers only because they avoid walks and home runs, and because they strike out a lot of batters.

McCracken's article attracted some attention. In the May, 2000 "By the Numbers," Clifford Blau gave it a quick mention. In January 2001, Rob Neyer gave it a favorable review in his on-line column, and followed up that review a week later with comments by Craig Wright and Bill James. (Both Neyer columns are available at http://espn.go.com/mlb/s/2001/0115/1017090.html at time of writing.)

Both Wright and James disagreed with McCracken's conclusion. Wright suggested that batting average on balls in play (hereafter called HPBP for "hits per ball in play") "is a primary way for pitchers to distinguish themselves from others, but I do believe it is a more significant factor for some groups than for others." He pointed out that flyball pitchers have a higher HPBP than groundball pitchers, and that in a study that encompassed 1987 to 2000, he found that "Guys like Pedro Martinez and Nolan Ryan are roughly 50 points better than guys like Dave Weathers and Sean Bergman."

James suggested that we would find that HPBP "is not random, but that it 'follows' pitchers," and that we could gain understanding by finding what factors influence HPBP, such as, the park, or the opposition batting lineup, or the existence of an outstanding third baseman. (Actually, none of these causes would challenge the conclusion that HPBP is not a "skill," just the conclusion that it's random.)

I ran a few further tests to investigate some of the issues raised by these analysts.

Repeatability

If HPBP is a "skill" of the pitcher, we would expect that the pitcher's past HPBP performance would predict his future HPBP performance.

To test whether this was the case, I found every pitcher from 1979-1990 who gave up at least 300 balls in play, and matched up that season with the most recent first prior season where he also gave up 300 balls in play. I then ran a regression to see how well the prior season predicts the current season.

I'll give some of the raw regression results first, for those of you who are regression-literate, and then explain them later:[1]

```
Pairs of seasons in study: 1299
R-squared = 0.0239
F(1, 1297) = 31.78
Average current HPBP = 0.2859, average previous HPBP = 0.2836
Current HPBP = (.1562 * Previous HPBP) + 0.241575
Probability this would happen by chance = 0.0000
```

To answer the most important question first: HPBP is *not* random – the previous HPBP does help predict the current one, and in a positive direction. For each point of the previous HPBP, current HPBP would be expected to increase by 0.1562 of a point – or, put another way, every 7 points last year translates into one expected point this year. Roughly speaking, a pitcher who had a HPBP last year of .292 (roughly seven points above average) would be expected to have a HPBP this year of .286 (roughly one point above average).

[1] Full regression results (computer output) are available from the author.

The result is statistically very significant: the probability that this would happen by chance is zero. We can conclude that HPBP is *not* random, but does show something persistent. At this point, though, the cause could be anything: it could be the ballpark, it could be the defense, it could be anything that tends to remain fairly constant between one year and the next. And, of course, it could be the pitcher's skill or style of pitching.

Another thing we can conclude is that even though HPBP is not random, still, *most* of the fluctuation in year-to-year HPBP is the result of factors we haven't yet considered. Since it takes seven points of last year's HPBP to produce one expected point of this year's, six-sevenths of HPBP is still unexplained by last year's data. Talent is at most 15% of a pitchers' season differences from the mean HPBP.

Is this 85%/15% difference big or small? To help intuit this, I ran a quick simulation. I took 1500 pitchers, and spread each of their expected intrinsic (talent) HPBP in a normal distribution around an average of .2854, with standard deviation .075. Then, I ran two random seasons of 700 BIP for each pitcher.

The results:

```
Pairs of seasons in study: 1500
R-squared: 0.0217
F(1,1498) = 33.2962
Average current HPBP = 0.2854, average previous HPBP = 0.2858
Current HPBP = ( 0.1479 * Previous HPBP ) + 0.243154
Probability of this happening by chance = 0.0000
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The results for this random simulation are almost the same as for the results for the "real" study presented earlier. That's because I deliberately experimented with the standard deviation until I obtained similar results. A standard deviation of .05, or .10, would give results not at all close to the real results. Since it's the SD of .075 that gives similar results, it's reasonable to conclude that the distribution of the pitchers' intrinsic (skill) HPBP tendencey has a standard deviation of about 7.5 points.

And so, since 2.5% of a normal distribution falls more than 2 standard deviations above the mean, we can say that only 2.5% of pitchers have an intrinsic HPBP more than 15 points below the mean.

On this basis, HPBP does seem much less a part of a pitcher's skill set than (say) strikeout rates. The top 2% of pitchers might tend to strike out twice as many batters as average, but the top 2% of pitchers in HPBP might tend to only limit the opposition to a .270 average instead of .285. Clearly, the skill for giving up hits on balls-in-play is "smaller" than the skill for striking out batters.

What Influences HPBP?

Now that we know that there is some non-random component of HPBP, what causes it? Is it the park, is it the defense, or is it something about the pitcher's own style?

To help investigate this, I again took every pitcher-season with at least 300 BIP against from 1979-1990. Then, for each season, I ran a regression of HPBP on four factors:

- HR percentage allowed (HR/AB);
- BB percentage allowed (BB/AB);
- Strikeout percentage (K/AB); and
- Flyball percentage (FB / FB+GB).

The regression results showed that HR percentage and BB percentage were not significant, so I removed them from the regression and used only strikeout and flyball percentages.

Here are the regression results:

```
Seasons in study: 1810
R-squared: 0.0360
F(2,1807) = 33.7362
HPBP = (-0.0396*Kpct) – (0.0432 FLYpct) + .311876
Kpct was significant (p=.0021)
FLYpct was signficant (p=.0000)
Probability of this happening by chance: 0.0000
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We might have expected that the better the strikeout pitcher, the lower the HPBP should be; after all, if a pitch is hard to put in play, it should also be harder to hit it where you want it when you *do* manage to put it in play.

And that does turn out to be the case, but only to a very small degree: a pitcher whose strikeout ratio is .03 higher (that is, he strikes out one extra batter per 33 AB), will have a HPBP only 1.2 points lower. If Pedro Martinez declines in strikeouts, so that he strikes out only half as many batters next year (from 284 to 142), his expected HPBP will be only .007 higher.

We should also expect that the more flyballs a pitcher gives up, the lower the batting average should be when the ball remains in the park. That's because it's easier to turn a fly ball into an out than a ground ball.

And, indeed, our expectation is borne out. A pitcher who gives up 60 percent flyballs instead of 40 percent will see his HPBP improve by .009 points. This is more improvement than if the starter struck out an extra 140 men per season, and we can see that flyball tendency is a huge predictor of HPBP.

Finally, we should note that strikeout and flyball tendencies are a more reliable way to predict HPBP than even a pitcher's last year's HPBP. The R-squared rose from .0239 to .0360, meaning the K and FB predictors were more than one-and-a-half times as successful in explaining HPBP.

Combined

So there are at least two non-random factors that contribute to HPBP: strikeout rate, and flyball rate. But are there more? Or are these two pretty much it?

One way to try to find out is to combine the last two regressions, adding the pitcher's previous year's HPBP to the K/FB variables. If the r-squared doesn't move much, we can conclude that there's nothing in the pitcher's record other than K/FB that contributes to HPBP. But if there is a big improvement in r-squared, we'll know that there's extra information in the pitcher's record that we haven't isolated, not just K and FB.

Well, it turns out it's the latter case: the r-squared is up two-thirds, from .0377 to .0619:

```
Seasons in study: 1299
R-squared: 0.0619
F(3,1295)=28.4835
HPBP = (.127 prevHPBP) - (0.052*Kpct) – (0.0411 FLYpct) + .277426
Kpct was significant (p=.0005)
FLYpct was signficant (p=.0000)
PrevHPBP was significant (p=.0000)
Probability of this happening by chance: 0.0000
```

It's interesting that the effect of flyballs and strikeouts drops in both cases once we include previous HPBP. This means that some of the HPBP information contained in strikeout rate and flyball rate is also contained in previous HPBP. And so, given that the pitcher has a certain previous HPBP, strikeouts are less important in predicting this year's HPBP.[2]

[2] As an analogy, consider trying to predict a player's season batting average with his batting average this game. There will be a high correlation, because Tony Gwynns tend to do well, on average, in any given game. But, if we take into account how good the player was last year, this game becomes much less significant: because even if Tony goes 0-for-5 today, we know he's Tony Gwynn, and his 0-for-5 doesn't much affect what we think he'll do for the rest of the season. And so there will be little correlation between this Tony Gwynn game and this Tony Gwynn season.

In addition to the rise in r-squared, we can look at the coefficient of the previous HPBP. In the original regression, one point of previous HPBP translated to 0.1546 points of current HPBP. Now, even after taking all the other factors into account, one point of previous HPBP is still 0.127 points of current HPBP – 82% as much as before! By this measure, flyball rates and strikeout percentages only account for 18% of the information contained in the pitcher's historical record. Even after this study, we have still only isolated 18% of the causes of variations in HPBP.

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Submissions

Phil Birnbaum, Editor

Submissions to *By the Numbers* are, of course, encouraged. Articles should be concise (though not necessarily short), and pertain to statistical analysis of baseball. Letters to the Editor, original research, opinions, summaries of existing research, criticism, and reviews of other work (but no death threats, please) are all welcome.

Articles should be submitted in electronic form, either by e-mail or on PC-readable floppy disk. I can read most word processor formats. If you send charts, please send them in word processor form rather than in spreadsheet. Unless you specify otherwise, I may send your work to others for comment (i.e., informal peer review).

If your submission discusses a previous BTN article, the author of that article may be asked to reply briefly in the same issue in which your letter or article appears.

I usually edit for spelling and grammar. (But if you want to make my life a bit easier: please, use two spaces after the period in a sentence. Everything else is pretty easy to fix.)

If you can (and I understand it isn't always possible), try to format your article roughly the same way BTN does, and please include your byline at the end with your address (see the end of any article this issue).

Deadlines: January 24, April 24, July 24, and October 24, for issues of February, May, August, and November, respectively.

I will acknowledge all articles within three days of receipt, and will try, within a reasonable time, to let you know if your submission is accepted.

Send submissions to:
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The following committee members have volunteered to be contacted by other members for informal peer review of articles.

Please contact any of our volunteers on an as-needed basis - that is, if you want someone to look over your manuscript in advance, these people are willing. Of course, I'll be doing a bit of that too, but, as much as I'd like to, I don't have time to contact every contributor with detailed comments on their work. (I will get back to you on more serious issues, like if I don't understand part of your method or results.)

If you'd like to be added to the list, send your name, e-mail address, and areas of expertise (don't worry if you don't have any - I certainly don't), and you'll see your name in print next issue.

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