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# By the Numbers

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Volume 14, Number 4

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Summary

## Academic Research: Even More On Competitive Balance

Charlie Pavitt

*The author reviews two more academic studies on competitive balance in baseball.*

*This is one of a series of 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 its new location [www.udel.edu/communication/pavitt/biblioexplain.htm](http://www.udel.edu/communication/pavitt/biblioexplain.htm). Use it for your research, and let me know what is missing.*

**Gary Koop, Modelling the Evolution of Distributions: An Application to Major league Baseball, Journal of the Royal Statistical Society, Series A, 2004, Volume 167 Part 4, pp. 639-655**

**Martin B. Schmidt and David J. Berri, Convergence and Clustering in Major League Baseball: The Haves and Have Nots? Applied Economics, 2004, Volume 36, pp. 2007-2014**

The ever-growing scholarly literature on competitive balance appears, on its face, to resemble that on ethnic diversity in baseball; academics padding their vitas through reinventing the wheel using slightly different data sets and methods. However, there is a critical difference between these two topic areas, for in the case of competitive balance, the recent explosion of research is in response to the Blue Ribbon Panel's claim that competitive balance is declining due to the widening disparity in income between a wealthy subset of teams and the others. Most respondents have disagreed, demonstrating conclusively through

the use of a number of different statistical indices that the variation among teams within seasons has been declining steadily decade-by-decade since 1900. I have reviewed relevant research twice previously in BTN (Volume 9, Number 3, and Volume 13, Number 4). But there is a completely different way to think about competitive balance, through examining the extent to which each team's performance varies over time. There has been some work using this interpretation (see my review of a study by E. Woodrow Eckard in BTN, Volume 11, Number 4, which also

found evidence for increased balance). Both of these studies are this in this second camp.

Koop develops a Markov model in which the probability of

teams moving up and down in the standings can be described and the extent to which resulting transition probabilities change over time can be tested. Using the entire 20th century, Koop found all but one team to move up and down the standings fairly readily. The exception was, not surprisingly, the Yankees, who, when on top, are unusually likely to stay on top and, when on the bottom, are unusually unlikely to stay there for long.

While Koop is a newcomer to this issue, Schmidt is anything but; this is his fourth published article on competitive balance, all but one with Berri as second author. These authors examined the extent to which teams tend to congregate into "clusters" of teams whose winning percentages converge over time. Using the entire

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20th century but limiting the analysis to the 16 pre-expansion franchises, Schmidt and Berri found the number of clusters to have decreased across time, such that five are discernible when based on the years 1901 to 1960 whereas only three remain when using the entire data set. Interestingly, one “cluster” consists of only one team, the Giants. The two “real” clusters do look like the Haves (the Yankees, Dodgers, Cardinals, Red Sox, Cubs, and Reds) and Have Nots (the Pirates, Indians, Tigers, White Sox, A’s, Braves, Browns/Orioles, Senators/Twins, and Phillies), providing evidence that appears to be consistent with the Blue Ribbon Panel’s contention. However, the authors also show that, at least during the 1990-1997 period, the “Have Not” cluster outperformed the “Have” cluster on the field.

All of this adds more evidence in support of the only conclusion I can read from all of this; if competitive balance is worsening, it is not yet detectable, and could only have begun in the last decade or so.

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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 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.

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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## Well, Excuuuuse Me

Bill James

*In the November, 2003 issue of BTN, Herm Krabbenhoft criticized how leadoff hitters were treated in Bill James' recent book. Here, Bill James responds.*

In *The New Bill James Historical Baseball Abstract* (2001, [The Free Press](#)), I spent a page or two introducing a method to evaluate the performance of a leadoff man. There was, at that time, no published data that I am aware of about who was and was not a leadoff man for how much of his career, and so I had to—or chose to, if you prefer—make guesses or assumptions about who batted leadoff.

It turns out that I got a few of these wrong. I described Pee Wee Reese, for example, as “the best career leadoff man among the shortstops,” and I also identified as leadoff men, in the same article, Johnny Pesky and Woody English, and, in a different article, Elbie Fletcher, Rod Carew, Max Carey, and Augie Galan. These players were included in a list of 65 leadoff and leadoff type hitters.

Subsequent research, published by Herm Krabbenhoft, shows that Johnny Pesky led off in only 25 games in his career, Elbie Fletcher only 7 games, and Ray Chapman only 44 games, meaning that those three clearly were not leadoff hitters, and that Max Carey, Woody English, Augie Galan and Pee Wee Reese batted leadoff in only 22 to 28% of their career games played, making the identification of them as “career leadoff men” questionable. Rod Carew's percentage of games led off was even lower, closing off the question in his case, and there are other players involved in this issue, who I will discuss later.

By no means do I intend to minimize my mistakes, at least right now; maybe I'll minimize them later in the article. But for now ... certainly I should have been more careful in double-checking box scores to see who was batting leadoff and who was batting second or in some other place, and certainly it is not proper for a writer to identify players who are not leadoff men as leadoff men or people who are not serial murderers as serial murderers, although the one offense is somewhat more regrettable than the other.

In the November, 2003 issue of *By the Numbers*, Mr. Krabbenhoft was kind enough to make me aware of these errors, and also to exaggerate them enormously, so as to make it appear that I was indifferent to the facts.

*I don't know how or where Mr. James obtained his information to write the above statements. His statements are not consistent with the actual baseball record. Here are the facts.*

*--Herm Krabbenhoft*

Here are the facts, indeed, liberally sprinkled with exclamation marks and interpreted with such value-neutral words as “incredibly” and “unbelievably.” “With regard to the statements about Reese, Hemus, Pesky, Lary, and Chapman,” wrote Mr. Krabbenhoft, “here are the facts about their leadoff batter activity: ... Hemus was a principal leadoff batter for just three seasons (1951-1953).”

Yes, he was. He was also a *regular* for only three seasons (1951-1953). Why, then, does Krabbenhoft object to my characterizing him as a leadoff hitter?

Mr. Krabbenhoft has his standards ... something about “5 principal leadoff batter seasons and/or less than 3 '>75%' principal leadoff batter seasons.” What he is saying, implicitly, is that it is an error on my part to list as a leadoff batter anyone who fails to meet his standards. But his standards are obviously inappropriate as applied to a player who is only a regular for a few seasons. So is that my error, or his?

This problem accounts for many of the discrepancies between my list and Mr. Krabbenhoft's, which he characterizes as my errors. He objects to the classification of Gary Redus as a leadoff man, although, according to his data, Redus led off in over half of his career games, which was probably two-thirds of his career starts. He objects to the classification of Dave Collins as a leadoff man, although (by his data) Collins led off in 665 games, and, by my calculation, probably started less than 1200 games. He objects to Gary Pettis as a leadoff man, although Pettis led off in 586 games out of, I would guess, a thousand or fewer career starts.

But even if a player bats leadoff in 25 or 30% of his career starts, might it not still be reasonable to describe him as a leadoff man? Joe Torre caught in only 41% of his career games, but I still rated him as a catcher. Since players move between batting positions much, much more than they move between fielding positions, it seems to me likely that a player who leads off in 25% of his games will have more games as a leadoff man than he has batting in any other slot. Lonnie Smith may have led off in only one-third of his career games, in part because

he spent several years as a teammate of Willie Wilson, but he is still a leadoff man in my book. He may not meet Mr. Krabbenhoft's standards of a leadoff hitter, but neither does this disagreement meet the standards of an "error." It is a difference of opinion.

In the book I had written that almost exactly one-fourth of good major league shortstops were essentially leadoff men. In fact, says Mr. Krabbenhoft, "only 17% of these shortstops can realistically be considered 'essentially leadoff men.'"

Well, first, the difference between "almost exactly one-fourth" and "only 17%" doesn't seem to me to be worthy of very much comment. Second, some portion of that difference is accounted for by players, like Solly Hemus, about whom Mr. Krabbenhoft's classification system is clearly in error. And third, almost all the rest of it is just a difference of opinion. You may not regard Lyn Lary as a leadoff man; I do. I would predict that 80% of baseball fans, apprised of all the facts, would agree with me. You may not choose to regard Phil Rizzuto and Pee Wee Reese as "essentially leadoff men." I do, in large part because I evaluate players not merely by what they do most often, but also by what they do best. In their best seasons, Pee Wee and the Scooter were leadoff men, or were often leadoff men. To my way of thinking, those seasons obviously should count more than the years at the end of their careers when they were batting eighth.

Yes, I did identify as leadoff men, by my count, five players who clearly should not have been so identified—Galan, Fletcher, Carew, Pesky and Chapman. Perhaps I should have used the term "leadoff-type hitters" instead, or perhaps I should simply not have mentioned these hitters. But, throwing myself on the mercy of the court, the data which shows these assumptions to have been in error had, to the best of my knowledge, never been published at the time my book was written. Mr. Krabbenhoft's article does not say that this data had been published; perhaps this detail was omitted to spare my feelings, I don't know. I did make some assumptions about the missing data that turned out to be in error, but is it a valid principle that people who are missing some of the facts should not discuss an issue if this requires them to make assumptions about some of the missing facts?

It does not seem to me that it would be reasonable to make a rule that any analysis must wait on all of the facts, and this—at least it seems to me—is the rule that Krabbenhoft is effectively propounding. Tell me if I am wrong here, but it seems to me that the clear implication of Krabbenhoft's complaint is that, the facts not having been published, I should not have speculated about who was and was not a leadoff man, that it was careless and irresponsible for me to do this. But is that in fact a valid principle of sabermetrics, or a valid principle of any science?

Well, no, it isn't. Let us suppose that you were curious about the role of the first baseman's throwing in leading to an increase in 3-6-3 and 3-6-1 double plays. Counts of the exact number of 3-6-1 and 3-6-3 double plays started by each first baseman do not exist at the present time. You *could*, if you wanted to devote enough time and energy to the project, start documenting actual 3-6-1 and 3-6-3 double plays, leading eventually to the point at which you would know that Keith Hernandez started 147 3-6-3 double plays in his career, or 282, or 419, or whatever the actual number might be.

You *could* do this, and it would be a worthwhile thing for you to do, and eventually some researcher will do this, just as Mr. Krabbenhoft has compiled his amazing lists of leadoff games, which was a very worthwhile thing to do. But the question I am asking is, would it be irresponsible for you, as a writer, to discuss the *implications* of the first baseman's throwing arm, in advance of the data? Would it be irresponsible of you to suggest that Keith Hernandez and Gil Hodges saved their teams ten or fifteen runs a year by making this play, when subsequent research may show that you were operating on false assumptions about the data, that the best first basemen of their time at making this play were not Keith Hernandez and Gil Hodges, but Andre Thornton and Earl Torgeson? And after all, you *could* have known this, if you had spent a year or so collecting the relevant data. Should you then be called on the carpet and publicly scolded for your ignorant and wrong-headed comments on the subject?

Well, of course not. The world of knowledge doesn't operate that way, and it *shouldn't* operate that way. I didn't do anything immoral here. I wasn't sloppy or careless with the known facts. I didn't misstate the known facts in order to make a point, as Mr. Krabbenhoft did when he stated, in an effort to make me look bad, that Solly Hemus was not a leadoff man, and as he did repeatedly throughout his article.

I regret making mistakes in print, but frankly, not all that much. Writers make mistakes, just as school teachers do and doctors do and judges do, and for the same reason: the good Lord did not choose to make any of us perfect. Mr. Krabbenhoft, in his article, consistently refers to Woody English as "Woodie" English, although I know of no source for that spelling of the name, and I checked a dozen sources dating back to 1930 before I wrote that. Mr. Krabbenhoft, in footnote number one to his article, cites pages 649-651 of my book, although pages 649-651 of the book have nothing whatsoever to do with this issue (he had apparently intended to reference pages 684-685).

"Another incredible aspect of the James list of 'the greatest leadoff men who were actually leadoff men', Krabbenhoft writes, "is that several bona fide leadoff men were omitted. For example—"and here he goes on through Max Flack and Nemo Leibold, among many others. But in fact the list was *never* intended to be a comprehensive list of leadoff men. It was introduced with the phrase, "Other noted leadoff men, colon" which I think any alert reader would interpret to mean that this was a selective sample.

Mr. Krabbenhoft takes from my book the three-word phrase “greatest leadoff men” and the nine-word phrase “the greatest leadoff men who were actually leadoff men”, puts them in quotation marks, and uses them repeatedly to refer to a long list of players who, it is very clear in the book, were *never* referred to in this way. Incredibly! He takes the selective list which is introduced simply as “other noted leadoff men”, applies the term “the greatest leadoff men who were actually leadoff men” to it, and carves that list apart for two pages upon the false premise that it has been presented as a comprehensive list of the greatest leadoff men ever.

*Chapman, as pointed out above, was never a principal leadoff batter ... Incredibly, James ranked him as the 36<sup>th</sup> greatest leadoff batter ever!*

I did nothing of the sort—nor is this a reasonable misinterpretation of the text.

That’s a mistake, too, but that’s a mistake of a yet another kind. It is a mistake of pettiness and distortion. Mr. Krabbenhoft has done some good and valuable work here, which I would have been happy to praise and admire, had he not chosen to make this ill-informed and frankly deceptive attack on a couple of light-reading comments that don’t really have anything to do with his area of interest.

Bill James, [BiJames@aol.com](mailto:BiJames@aol.com) ♦

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If you would like more information, send an e-mail (preferably with your snail mail address for our records) to Neal Traven, at [beisbol@alumni.pitt.edu](mailto:beisbol@alumni.pitt.edu). If you don’t have internet access, we will send you BTN by mail; write to Neal at 4317 Dayton Ave. N. #201, Seattle, WA, 98103-7154.

## Informal Peer Review

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.

Expertise in "Statistics" below means "real" statistics, as opposed to baseball statistics - confidence intervals, testing, sampling, and so on.

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# Catcher ERA – Once More With Feeling

Tom Hanrahan

*In a study several years ago, the author found that when a catcher is in his prime, his team's pitchers tend to have a lower ERA than they did when the catcher was a rookie. The implication: catchers learn to handle pitchers better as they gain experience. Here, the author revisits his study, looking closely at specific pitcher/catcher pairs, in an attempt to confirm the effect.*

## Introduction

Do catchers in general do a better job of handling the pitching staff as they gain major league experience? In a previous study, published in the August, 1999 issue of BTN, I concluded the answer to this question was a resounding “Yes!” The data used were the team ERAs (adjusted for league average) of all clubs where the same primary catcher was used in consecutive seasons, from 1946 through 1987, and the number of previous major league games the catcher had caught prior to each year. In this study, I found that the team ERA dropped significantly as a catcher went from being a rookie to having spent 4 to 7 years with the same club. The effect was particularly strong when dealing with rookie catchers with very little experience (fewer than 50 MLB games caught); virtually every team who kept these catchers (16 of 17) had a better ERA by the time they gained more experience.

Some students of the game pointed out a potential flaw in my methodology. I attempted to “hold all other things equal” by comparing catchers in rookie seasons to their later years while with the same team, assuming that changes in the pitching staffs, while not inconsequential, would be random enough. However, it is possible that teams who employ a rookie catcher might be in a “rebuilding” year more often than is normal, and thus employing an untried or sub-par pitching staff at the same time. To make a quick check of this, I recorded team records for the year prior to a rookie catcher being used. These teams (for the rookie catcher in the database) averaged only about 76 wins (per 162), which does suggest a tendency to often be rebuilding when a rookie catcher is brought in. But correcting for the team records of the catchers’ rookie years wouldn’t appear to be of much analytical value, since those records would themselves be influenced by the catcher (or so the theory goes). Well, now what? I decided what I needed to do was to compare specifically pitcher-catcher pairs on the same team as the catcher matured. This ought to show any influence the catcher has on the pitcher over time. And that is the study of this article.

## The Update

First, I updated my original study. Actually, a gentleman named Jerry Swenson did all of the research (read: grunt work) for me. The database now includes the years 1988 through 2003, adding 47% more players to the file; from the original 560 catcher-years up to 835. The additional years used did not change the original conclusions: team ERA improved significantly from the catchers’ rookie years to while they were in their prime. The graph on the next page shows two curves, the original and the new (combined) relative change in team ERA versus how many games a catcher had spent with a team. The one noticeable difference found when adding the more recent data is a lessening of the poorer ERAs when a catcher was well past his prime (>1000 games caught). However, there isn’t as much data in this area (many catchers are not playing full-time by this point), so it could be just random noise.

## Pitcher-Catcher Pairs: The Data

In the original study, I identified 16 instances of a catcher who had a full-time rookie year after only having caught fewer than 50 games in the major leagues previously, and who then stayed with that team through his prime years. This was a set of catchers who “burst on the scene” as it were, and were good enough to continue playing. Updated for Jerry Swenson’s research of recent years, this set of catchers now numbers 26.

I found all of the pitchers who tossed at least 100 innings in these catchers’ rookie seasons. I then looked for any other seasons where the same pitcher threw at least 100 innings in a year when the catcher was in his prime with the team. (I defined “prime” largely by the findings of the previous study, as 4 to 7 years with the team.) I could then compare the individual pitchers’ ERA in different years with the same catcher. I also entered data for all of the pitchers who threw at least 100 innings in the year prior to the catcher’s rookie season (when they were obviously throwing to a different receiver). All of these data were entered into a file that contained 26 catchers, 90 catcher-pitcher

pairs, and a total of 233 paired seasons. (A list of names, teams, and years appears in the appendix at the end of this article.) Of course, there are many, many more cases of pitchers throwing to rookie or veteran catchers; but again, this data is only for those pitcher-catcher pairs who played for the same teams.

## From Rookie To Prime Backstops

First, I will compare the seasons when the catchers were raw rookies to their time as 4-to-7-year vets.

Example: Ramon Hernandez had his first full year in MLB in 2000, catching 142 games for the Oakland A's. He had only caught 40 games previously. In 2000, there were five pitchers who threw at least 100 innings for the A's. Only two of these also threw 100 IP in 2003 (and 2004; while our research stopped initially at 2003, I since updated any numbers with data from the 2004 campaign), which would be "rookie year plus 3 (and plus 4)" --

Mark Mulder and Tim Hudson. Hudson's ERA in 2000 was 4.14, and in 2003-04 combined it was 3.07. Mulder put up a 5.44 ERA in 2000, and a 3.84 ERA in 2003-04. So, Hudson and Mulder were both more effective when Hernandez was a veteran receiver with the A's.

The overall results were:

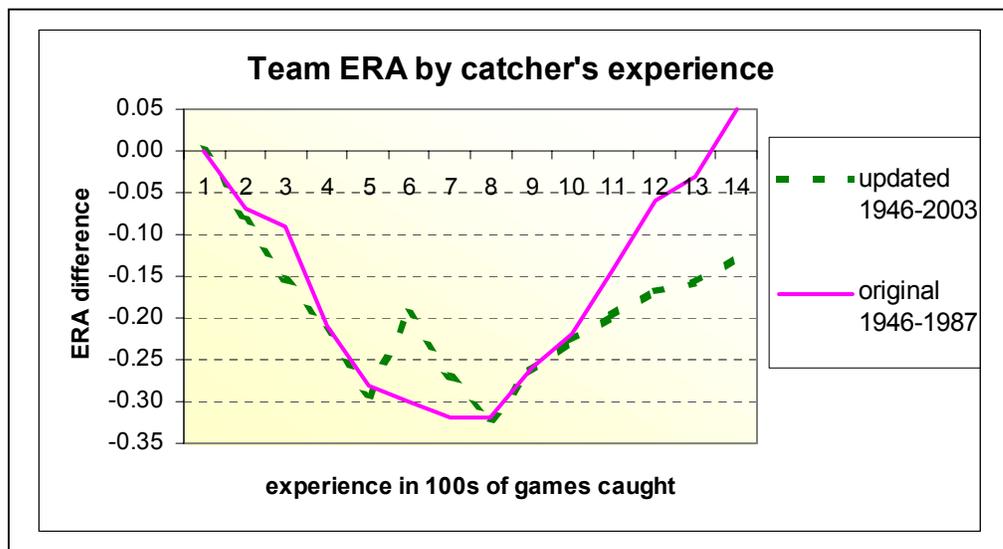
- There were 39 qualifying pitcher/catcher pairs. Of those, 22 had the lower ERA with the veteran catcher.
- Pitchers throwing to the catchers as veterans had a composite ERA that was 0.40 lower than when the catchers were rookies.
- Assuming a normal distribution, this 0.40 difference is well beyond the bounds of chance (greater than 2 sigma).

There was a significant amount of variation in the data. A few points stand out: in 1961, after a decent first season in the bigs, Chris Short put up a disastrous 5.94 ERA for the Phillies in Clay Dalrymple's initial season as Philadelphia's primary catcher. Short would later post ERAs consistently in the 2s with Dalrymple in 1964 through 67. Another large jump was exhibited by Sandy Koufax, who only managed a 4.48 ERA in 1958 throwing to a young Johnny Roseboro. Of course, a few years later, Roseboro was privileged to catch numerous gems from Sandy's amazing arm; certainly the move to Dodger stadium helped as well.

Going the other way, the largest increase in ERA from rookie to veteran catcher was the combo of Luis Tiant and Carlton Fisk. Luis put up a brilliant 1.91 ERA in Fisk's rookie year (1972), but was much less effective in the later 70s.

One problem area I saw with this data set is that Steve Carlton was famous for having his own personal catcher (Tim McCarver) late in his career with the Phillies, so it is questionable whether he can be used in this data set. There may be other similar examples (Greg Maddux?) that I am not aware of.

Some readers might, at this point, already be thinking that the first two pitchers mentioned here were also very young in the catcher's initial year, and of course Tiant was who-knows-how-old by 1978. This is true, and could be the topic of a future study, although there certainly were counterexamples in this database. And this leads to my next line of analysis.



## Rookies Compared to the Year Prior

For the next comparison, I compared the ERAs of pitchers who had 100 IP in both the catcher's rookie year, and in the previous year with the same team.

The overall results were:

- There were 73 qualifying P/C pairs; 22 of these were the same used in the previous data set.
- 47 of the 73 pitchers had a higher ERA with the rookie catcher than his predecessor.
- The composite ERA of these 73 pitchers was 0.37 higher with the rookie catcher.
- Assuming a normal distribution, this 0.37 difference is well beyond the bounds of chance (greater than 3 sigma).

This extra comparison, while not quite apples-to-apples, does at least answer the objection made previously: if it were true that the pitchers in the first study were improving as they naturally matured along with the catchers, then we would also expect them to have been even worse when they were a year younger. Instead, the opposite occurred. There are more data for this second comparison, since it is easier to find pitchers who threw 100 innings in consecutive years than for 4 or more years apart.

## Conclusions

How else can I say it? This is big. This is huge. This is practically irrefutable. Catchers, as a whole, somehow are "worse" at helping their pitchers when they are major league rookies. Pitchers, having worked previously with who-knows-what catcher, have their ERAs go up when throwing to the new guy. And after throwing to the new guy for a few years, their ERA goes way down. Maybe 40 or 50 or 60 runs per year, if you add up the effects for a whole pitching staff. No defensive player saves 50 runs a year. No catcher prevents stolen bases or passed balls, or blocks the plate, to anywhere near the tune of 50 runs a year.

The last team to win a pennant with a rookie catcher, even bending the definition of 'rookie' a bit, was the 1990 Reds, for whom Joe Oliver has caught 49 games the previous season. This is 26 pennants ago. Matthew Namee recently (web) published an note on *thehardballtimes.com* which stated that only two teams using catchers age 22 or younger had won a World Series: Tim McCarver's Cardinals in 1964, and Mike Scioscia's Dodgers in 1981. McCarver had already spent parts of 3 years in St. Louis, and Scioscia wasn't a rookie in '81 either.

You want an advantage for your fantasy league next year? Stay away from pitchers throwing to rookie catchers.

Do I know *why* this happens? No. But handling pitchers in the majors sure seems to be a learned skill.

## Appendix: List of Players Used

Rookie Year	Catcher	Team-LG	Pitchers
52	White	BOS-A	McDermottM, ParnellM, NissonW
56	Triandos	BAL-A	MooreR, PalicaE, WightB, BrownH
58	Roseboro	LAD-N	KoufaxS, DrysdaleD, LabineC, PodresJ, WilliamsS
61	Dalrymple	PHI-N	MahaffeyA, RobertsR, BuzhardtJ, GreenD, ShortC, BaldschunJ
62	Haller	SFG-N	SanfordJ, MarichalJ, MillerS
63	McCarver	STL-N	GibsonB, BroglioE, SimmonsC, SadeckiR
66	Casanova	WAS-A	RichertP, McCormickM, OrtegaP, HannanJ, CoxC
66	Hundley	CHI-N	EllsworthD
68	Bench	CIN-N	NolanG, AbernathyT, MaloneyJ
69	Herrmann	CHI-A	WoodW, HorlenJ, JohnT, PetersG
69	Sanguillen	PIT-N	VealeB, BlassS, MooseB, EllisD, WalkerL
70	Munson	NYA-A	StottlemyreM, BahnsenS, PetersenR
72	Fisk	BOS-A	SiebertS, CulpR, TiantL
72	Rader	SFG-N	BarrJ, BryantJ, StoneS, MarichalJ
73	Boone	PHI-N	CarltonS, LonborgJ, RuthenD, Twitchell
74	Sundberg	TEX-A	BibbyJ
76	Wynegar	MIN-A	GoltzD, HughesJ, CampbellB, RedfernP
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90	Oliver	CIN-A	BrowningT, RijoJ, JacksonD, MahlerR

91	Hoiles	BAL-A	MilackiB, BallardJ, MacDonaldB
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93	Piazza	LAD-N	HershiserO, CandiottiT, GrossK, MartinezR, AstacioP
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