Sunk Costs in the NBA: Why Draft Order Affects Playing Time and Survival in Professional Basketball

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This study represents one of the first quantitative field tests of the sunk-cost effect. We tested whether the amount teams spent for players in the National Basketball Association (NBA) influenced how much playing time players got and how long they stayed with NBA franchises. Sunk costs were operationalized by the order in which players were selected in the college draft. Draft order was then used to predict playing time, being traded, and survival in the NBA. Although one might logically expect that teams play and keep their most productive players, we found significant sunk-cost effects on each of these important personnel decisions. Results showed that teams granted more playing time to their most highly drafted players and retained them longer, even after controlling for players' on-court performance, injuries, trade status, and position played. These results are discussed in terms of their implications for both sunk-cost research and the broader literature on managerial decision making.•

Common sense tells us that people try to avoid losing courses of action. They move away from lines of behavior that have not been rewarded and hesitate to follow strategies that are not likely to yield future benefits. Yet some behavioral research has challenged this logic. Coming under the rubric of escalation of commitment, a number of studies have shown that people can become stuck in losing courses of action, sometimes to the point of "throwing good money after bad."

Evidence of this escalation effect was initially provided by three independent lines of research. Staw (1976) used a simulated business case to show that people responsible for a losing course of action will invest further than those not responsible for prior losses. Tegar (1980) took advantage of an unusual competitive bidding game (Shubik, 1971) to demonstrate that people can become so committed to a position that they will pay more for a monetary reward than it is worth. Finally, in several related studies, Brockner and Rubin (1985) showed that people are likely to expend substantial amounts of time and money in efforts to reach a receding or elusive goal. These initial investigations have been followed by a wide range of studies on conditions likely to foster persistence in a course of action, along with a set of theories accounting for these effects (see Staw and Ross, 1987, 1989; Brockner, 1992, for reviews).

Though the escalation literature has grown dramatically over the past two decades, it has continued to suffer from some serious problems. One issue is that escalation researchers have borrowed heavily from other research areas, such as cognitive and social psychology, without strict guidelines for selecting those variables most parallel to the conditions or events present in escalation situations. A second problem is that much of the escalation literature, despite its intent to explain nonrational sources of commitment, has not directly challenged the assumptions of economic decision making. By and large, the escalation literature has demonstrated that psychological and social factors *can* influence resource allocation decisions, not that the rational assumptions of

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decision making are in error. A third weakness is that almost all the escalation literature is laboratory based. Aside from a few recent qualitative case studies (e.g., Ross and Staw, 1986, 1993), escalation predictions have not been confirmed or falsified in real organizational settings, using data that are generated in their natural context. Therefore, despite the size of the escalation literature, it is still uncertain if escalation effects can be generalized from the laboratory to the field.

This paper presents one of the first quantitative field studies in the escalation literature. The study does not resolve all the problems of the escalation area, but it was designed with these deficiencies in mind. Because escalation situations involve the expenditure of resources over time, it is important to know whether the amount one initially spends on a course of action can affect subsequent commitment. Therefore, the study of sunk costs (past and irreversible expenditures) is central to the escalation question. Research on sunk costs is also a form of inquiry that confronts directly the assumptions of rational economic decision making. Economists universally caution against the use of sunk (rather than incremental) costs in decisions to invest further time, money, or energy in a course of action (Samuelson and Nordhaus, 1985; Frank, 1991). Therefore, any demonstration that sunk costs influence subsequent investment decisions calls into question the description of people as economically rational decision makers. Finally, and perhaps most importantly, by constructing a test of sunk costs using real organizational data, a large void in the escalation literature can be filled. If sunk-cost effects can be demonstrated in the field, then we may have greater confidence that escalation hypotheses can be generalized to situations devoid of the props, scenarios, and student samples generally used by laboratory researchers.

Research on Sunk Costs

Probably the most important set of sunk-cost studies is a series of ten experiments conducted by Arkes and Blumer (1985). Their most well-known study used a "radar-blank plane" scenario. Students were asked to imagine they were the president of an aircraft company deciding whether to invest $1 million in research on an airplane not detectable by conventional radar. These students were also told that the radar-blank plane was not an economically promising project because another firm already had a superior product. As one might expect, only 16.7 percent chose to commit funds to the project when funding was characterized as being used to start the unpromising venture. But, as predicted, over 85 percent chose to fund the venture when it was described as already 90 percent completed.

Follow-up studies by Garland and his colleagues replicated the sunk-cost effect yet posed questions about its interpretation. Using variations of Arkes and Blumer's (1985) radar-blank plane scenario, Garland (1990) demonstrated that sunk costs influenced investment decisions across several combinations of prior expenditures and degrees of project completion. When Conlon and Garland (1993) independently manipulated the level of prior expenditures and degree of

project completion, however, they found only effects for degree of completion. Garland, Sandefur, and Rogers (1990) found a similar absence of sunk-cost effects in an experiment using an oil-drilling scenario. Prior expenditures on dry wells were not associated with continued drilling, perhaps because dry wells were so clearly seen as reducing rather than increasing the likelihood of future oil production. Thus it appears that sunk costs may only be influential on project decisions when they are linked to the perception (if not the reality) of progress on a course of action.

Though sunk-cost effects have not been shown to be as simple as originally predicted for project decisions, a more robust sunk-cost effect has so far been demonstrated on resource utilization decisions. Again, several of Arkes and Blumer's (1985) scenario studies provide illustration. In one study, students had to decide which of two prepaid (but conflicting) ski trips to take: a trip likely to be the most enjoyable or a trip that cost the most. A second Arkes and Blumer study asked students which of two TV dinners they would eat: one for which they had previously paid full price or an identical dinner purchased at a discount. In a third study, Arkes and Blumer (1985) arranged to have theater tickets sold at different prices, with researchers monitoring subsequent theater attendance. The results from each of these resource utilization studies showed evidence of the sunk-cost effect. When people had to decide which of two similar resources to utilize, they used that resource for which they had paid the most.

Sunk Costs in the NBA

Using a design that parallels the resource utilization studies, this research tests the sunk-cost effect in the context of professional basketball. We use the National Basketball Association (NBA) draft to determine the initial cost of players. We then examine whether this cost influences the amount players are utilized by teams and the length of time they are retained by NBA franchises.

Probably the most important asset of any team in the NBA is its roster of players. Typically, players are selected from the college ranks via the NBA draft. Because each team is assigned only one draft selection for each round of the draft (barring any prior trades or deals for additional draft choices), the order in which players are taken in the draft represents an expenditure teams make to attain the services of a particular player. Salary contracts extended to players are roughly in line with their draft order, such that players drafted earlier expect to be paid substantially more than those taken later in the draft. Thus, the draft order of players represents an important and tangible cost to NBA teams. The draft order also represents a set of opportunities foregone, since choosing any particular player means passing over many other candidates who might also help the team.

Drafting high in the NBA draft does not guarantee teams having the best talent on the court, however. Teams may pass up players that turn out to be all-stars and may draft players that never reach their basketball potential. As one commentator noted on the eve of the 1994 draft:

Whether a team drafts in the Top 5 or not until the second round, nothing in pro basketball is quite as chancy as the draft. This is where Portland in 1984 gambled that it was much wiser to take Sam Bowie than the showboat Michael Jordan, and where, in the same year, Dallas decided they would go with the sure thing in Sam Perkins rather than chance a pick on that unusual kid from Auburn, Charles Barkley. One year later, the top pick was Patrick Ewing, followed by, in order, Wayman Tisdale, Benoit Benjamin, Xavier McDaniel, Jon Koncak and Joe Kleine, leaving for the latecomers such picks as Cris Mullin (No. 7), Joe Dumars (18th) and A. C. Green (23rd). (Shirk, 1994)

Because of the vagaries of forecasting talent, teams may have invested more in some players than is merited by their performance on the basketball floor. Therein lies the sunk-cost dilemma. Do teams use players they have expended the most resources to attract, even if their performance does not warrant it? Likewise, do teams retain high-cost players, beyond the level warranted by their performance on the court? These questions are analogous to those posed by Arkes and Blumer in their resource utilization studies, in which the use of an asset can depend more on its previous cost than its future utility.

Hypotheses

All things being equal, one would expect teams to play their most productive players. One might also expect that those who were selected high in the draft would, in general, constitute teams' most productive players. Thus, as a null hypothesis one might predict that, after controlling for productivity on the court, draft order will add little to the prediction of playing time in the NBA. If, however, sunk costs actually do influence utilization decisions, then draft order will be a significant predictor of playing time, even after the effects of on-court performance have been controlled.

Analogously, one can also investigate the role of sunk costs in decisions to retain the services of NBA players. Logically, one might expect on-court performance to be the primary determinant of decisions to cut or trade players in the NBA. If sunk costs are influential,however, then it can be hypothesized that draft order will constitute a significant predictor of keeping players, even after controlling for players' on-court performance.

We used three separate analyses to test the effects of sunk costs on personnel decisions in professional basketball. The first examined the role of sunk costs in the decision to use players on the court (minutes played); the second assessed whether sunk costs can predict the number of seasons players survive in the NBA; the third examined the effect of sunk costs on whether players are traded from the team that originally drafted them.

ANALYSIS OF PLAYING TIME

Method

The NBA draft. Conducted at the end of the season, the draft is the principal mechanism for teams to secure new talent or rebuild after a losing season. The rules of the draft dictate the order in which professional teams get to select

amateur college basketball players. Before 1985, the first pick in the draft was determined by a coin toss between the teams from the Western and Eastern Conference with the worst win-loss records. The rest of the teams in the league then selected players in the inverse order of their prior regular season records, with the best team picking last in each round. To reduce the incentive for teams to underachieve deliberately so as to get one of the best picks, the draft lottery was inaugurated in 1985. The lottery allowed all teams that did not make the play-offs to have an equal chance of getting the number one draft pick.[1]

Sample. The sample included all players selected in the first two rounds of the 1980–1986 drafts of the National Basketball Association. We restricted our sample to the first two rounds of these drafts because players selected beyond this point were rarely offered contracts. In 1989, the NBA itself narrowed the draft to two rounds. We also restricted our sample to players who received contracts and played at least two years in the NBA, so that we could track their performance over time. Of those who were drafted in the first two rounds, 53 players never received a contract and one player left to play in Europe and returned to play only one year in the NBA. Thus our sample included 241 players selected from the 1980–1986 drafts who eventually received contracts and played at least two years in the NBA.

Dependent variable. The number of minutes each basketball player plays per game is a carefully recorded statistic in the NBA. In these analyses we used readily available information on the number of minutes played during the entire regular season. The *Official NBA Encyclopedia* (Hollander and Sachare, 1989) and the *Sports Encyclopedia* (Naft and Cohen, 1991) were the principal sources of data on the amount of court time each player received yearly. These volumes were also the sources of data on regular season performance statistics, trade and injury information, and positions played by NBA personnel in the sample.

Development of a performance index. A variety of fine-grained statistics are maintained on each player's performance. To create an index for player performance, we used nine widely recorded player statistics: total number of points scored in a season, assists, steals, shots blocked, rebounds, personal fouls, free-throw percentage, field-goal percentage, and 3-point field-goal percentage. Because players with more minutes will naturally have a greater number of points, assists, steals, fouls, shot blocks, and rebounds, we controlled for playing time by dividing these measures by the total number of minutes played during the season. The free-throw, field-goal, and 3-point percentages were calculated by dividing the number of shots made by the total number of shots attempted for each category. Thus our measures of performance reflect the productivity of players while they are on the court rather than simply being a reflection of playing time or attempts at various shots.

We expected the underlying skills measured by the performance statistics to differ according to a player's position. Big players who typically occupy the forward and

1 In 1993, the NBA changed the draft lottery to a weighted system. The team with the worst record of the 11 nonplay-off teams has a 17 percent chance of getting the number-one draft pick, the next worst team has a 15 percent chance, and the probability descends to a 1 percent chance for the team with the best record among those eligible for the lottery.

center positions are more likely to have a high number of rebounds and blocked shots. Therefore one might expect that rebounds and blocked shots would emerge as a performance factor for forwards and centers, rather than guards. Conversely, one might expect steals and assists to constitute an important dimension of performance for guards, rather than forwards and centers, since the guard position is staffed by smaller, quicker players who are responsible for ball handling.

To reduce multicollinearity problems due to the intercorrelation of several performance measures, we factor analyzed the performance data to form broader, more independent performance indices. To avoid confounding a player's performance with his position, we conducted two separate factor analyses on the performance statistics: one for guards and another for those in the forward or center position. For each group of players the nine performance statistics were subjected to a principle components factor analysis with varimax rotation. Three factors with an eigenvalue greater than 1.0 emerged for both subsamples, explaining 58 percent of the variance in the correlation matrix of performance statistics for guards and a similar 58 percent of the variance for the sample of centers and forwards.

Somewhat surprisingly, the results of the factor analyses indicated that the same three factors underlie the performance statistics of both groups. As shown in Table 1, these three factors also appeared to form a logical structure for the components of performance. We labeled these performance components as "scoring," "toughness," and "quickness." The first factor consisted of points per minute, field-goal percentage, and free-throw percentage.[2] In the sample of forwards and centers, this factor (with an eigenvalue of 2.7) explained 30 percent of the variance, whereas it accounted for 23 percent of the variance (eigenvalue of 2.1) in the sample of guards. The second factor consisted of rebounds per minute and blocks per minute. This factor accounted for 16 percent (eigenvalue of 1.5) and 20 percent (eigenvalue of 1.7) of the variance in the samples of forwards/centers and guards, respectively. Finally, the third factor was composed of assists per minute and steals per minute. This factor accounted for 12 percent (eigenvalue of 1.1) and 16 percent (eigenvalue of 1.4) of the variance in the samples of forwards/centers and guards, respectively.

Based on the factor analyses we constructed three indices of player performance: *scoring, toughness,* and *quickness.* For each of the factors, we standardized the component measures, summed them, and then divided by the total number of items in the factor, thus creating an index with the mean of zero and standard deviation of one. To ensure that performance on any of these dimensions was not biased by the position of a player and to facilitate comparisons of players' performance, we standardized each of these performance indices by position, calculating the performance of each player relative to the performance of all other players in the sample at his particular position.

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Although personal fouls loaded on the *scoring* factor for guards, we excluded it, for two reasons. First, it was the only performance statistic that was not consistent in its loading across the two subsamples of guards and forwards/centers. Second, interpreting the meaning of fouls was more problematic than the other performance statistics, because they could be associated with either playing hard defense or being aggressive on offense.

Table 1

Factor Analysis of Performance Statistics by Player Position

	Factor Loadings for Forward/Centers			Factor Loadings for Guards		
Variable	Scoring	Toughness	Quickness	Scoring	Toughness	Quickness
Points/min.	.79	−.17	−.22	.63	.47	−.33
Field-goal percentage	.75	.39	.03	.66	.28	.05
Free-throw percentage	.62	−.33	−.09	.67	−.17	.08
Rebounds/min.	.02	.82	.05	−.15	.73	−.07
Blocks/min.	−.11	.66	−.14	−.05	.77	.10
Assists/min.	.33	−.50	.49	.15	−.29	.82
Steals/min.	−.05	−.06	.93	−.10	.36	.79
Personal fouls/min.	−.58	.40	.08	−.65	.30	−.13
3 Pt. field-goal percentage	.17	−.44	.13	.53	−.15	−.08

Additional control variables. Because a player's performance may be affected by injuries or illness, we included such information in our analyses. We coded whether players suffered from fourteen types of injuries or illness. We created a dummy variable, *injury,* as a broad indicator and coded for the presence of any type of injury or illness.

Another possible influence on playing time is whether an individual has been traded, but the effect of being traded is difficult to predict. A trade could increase playing time if the player moves to a team that has a greater need for his services. But being traded can also signal that a player is no longer at the top of his game, thus leading to a reduction in playing time at the new team. A dummy variable, *trade,* was created to control for these possible effects. It was coded 1 if the player was traded before or during a particular season and coded 0 otherwise.

A player's time on the court may also be determined by the overall performance of his team. Being drafted by a winning team, composed of other quality players, may make it more difficult for the incoming player to receive significant playing time. In contrast, being drafted by a weaker team may mean that the incoming player will be given more time on the court. Thus we coded the team record for each player (*win*) by taking the percentage of games won over the total number of games played during the season.

Since playing time may differ by position, we also included a dummy variable, *forward/center,* in the regression equation that was coded 0 if the player was a guard and coded 1 if he was a forward or center.

Results

The means, standard deviations, and zero-order correlations among the independent variables and dependent variable, minutes played, are shown in the Appendix.

We conducted four regression analyses to test for the effects of draft order on playing time in the NBA, controlling for each player's prior performance, injury, trade status, and position. To predict playing time in a given year, we entered the performance statistics (*scoring, toughness, quickness*) from the prior year, position (guard vs. forward/center), data on whether the player had been injured or traded during the

year, and finally, the player's draft number. This procedure was repeated for years 2, 3, 4, and 5 of the player's career in the NBA.

As shown in Table 2, a player's scoring was the primary performance variable associated with greater playing time over the five years of data. The occurrence of an injury or being traded were also consistent predictors of minutes played over the five years. In contrast, the measures of quickness, toughness, and the player's position were not (with the exception of quickness during the second and fourth years) significant predictors of playing time.

Table 2

The Effect of Draft Number on Minutes Played

	Minutes Played			
Variable	(1) Year 2	(2) Year 3	(3) Year 4	(4) Year 5
Forward/center	−11.68	−139.99	−206.81	−63.45
Prior year scoring	324.79•••	712.53•••	315.16•••	446.21•••
Prior year toughness	−23.35	80.09	120.26	112.48
Prior year quickness	133.25•	53.97	243.31••	76.51
Injury	−658.97••	−982.57•••	−649.53•••	−916.14•••
Trade	−466.66•••*	−547.89•••*	−437.95•••*	−362.65•*
Win	−1.37	−6.15	6.69	.93
Draft number	−22.77•••	−16.21•••	−16.46•••	−13.77••
Intercept	2100.74•••	2415.66•••	1955.06•••	2258.89•••
N	241	211	187	165
R-square	.34	.46	.34	.34

• $p < .05$; •• $p < .01$; ••• $p < .001$, one-tailed tests, except where noted.
* Two-tailed tests.

Table 2 also shows that draft order was a significant predictor of minutes played over the entire five-year period. This effect was above and beyond any effects of a player's performance, injury, or trade status. The regressions showed that every increment in the draft number decreased playing time by as much as 23 minutes in the second year ($\beta_1 = -22.77$, $p < .001$, one-tailed test). Likewise, being taken in the second rather than the first round of the draft meant 552 minutes less playing time during a player's second year in the NBA.[3] Though a player's draft number was determined before entering the league, it continued to be a significant predictor of playing time up to and including the fifth year of a player's NBA career ($\beta_2 = -16.21$, $p < .001$, one-tailed test; $\beta_3 = -16.46$, $p < .001$, one-tailed test; $\beta_4 = -13.77$, $p < .01$, one-tailed test).

Although the magnitude of the effect for draft order appeared to decline over time, this pattern should be interpreted with caution. Because players left the league over time, there were changes in our sample across the five periods. Since the departure of players is no doubt associated with a decrease in their skills (e.g., being cut or not making the final team roster), one could argue that our population became increasingly elite over time. Due to the restriction in range, demonstrating significant effects of draft order might have been more difficult in year 5 than in year 2,

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The number of players drafted in a round is equal to the number of teams in the NBA. Because the number of teams in the league changed during the time period of this study, we used an average of 24 teams to compute the difference between first- and second-round draft picks.

thus making any interpretation of the trend of effects problematic. Our second set of analyses specifically examined the effect of draft order on the exit of players from the NBA over time.

ANALYSIS OF CAREER LENGTH

We hypothesized that the decision to keep or cut players—like the decision to give players court time—is based on sunk costs as well as performance criteria. Therefore, we investigated whether survival in the league could be explained by a player's initial draft number, after controlling for his level of performance in the NBA.

Method

Examining survival in the NBA poses several challenges to standard regression techniques. First, ordinary regression analyses cannot easily incorporate changes in the value of explanatory variables over time. Creating performance variables for every year (up to twelve years) spent in the league would not only be very cumbersome but would also introduce problems of multicollinearity. Second, there is no satisfactory way of handling right–censored cases, i.e., the players for which the event of being cut from the league is not observed within the time period of the study. Conducting a logistic regression on a categorical dependent variable that distinguishes those who were cut from those who were not cut would retain information on both groups. But logistic regression cannot incorporate the effect of duration or time spent in the state prior to the occurrence of the event. The effect of duration, measured by length of tenure in the NBA, is particularly important because we would expect that a player's risk of being cut will increase the longer he remains in the league.

To address each of these challenges, we used event history analysis to examine how draft order influenced the risk of being cut from the NBA. A model of the survival process using this framework can explicitly include (1) explanatory variables that vary over time, such as on–court performance; (2) data on those who were and were not cut from the league; and (3) information on duration of time before leaving the league. Morita, Lee, and Mowday (1989) specifically recommended this technique for the analysis of turnover data and provided detailed information on its use.

Sample. For the event history analysis we again used a sample consisting of all players selected in the first two rounds of the 1980–1986 NBA drafts. We restricted the sample to those who played at least one year in the league after being drafted, yielding a final sample of 275 players. We followed all the players' careers until they were cut from the league or until the 1990–1991 season, the last year for which data were obtained. During the time frame of this study, we observed that 184 of the 275 players in this sample were eventually cut from the league.

Dependent variable. The dependent variable in event history analysis is the hazard rate. The hazard rate is interpreted roughly as the probability of the event of being cut occurring in a time interval t to $t + \Delta t$, given that the

individual was at risk for being cut at time t (Petersen, 1994). Dividing the probability $P(t, t + \Delta t)$ by Δt, and letting Δt go to zero, gives us the more precise formulation of the hazard rate expressed as an instantaneous rate of transition:

$$r(t) = \lim_{\Delta t \to 0} P(t, t + \Delta t)/\Delta t.$$

Model. We specified a model in which the hazard rate is a linear function of time, thereby allowing the risk of being cut from the league to increase or decrease over time.[4] The hazard rate function can be expressed as:

$$r(t) = \exp[\alpha + \beta_1 \mathbf{X} + \beta_2 \mathbf{X}(t) + \gamma(t)],$$

where $r(t)$ is the hazard rate or risk that a player is cut from the NBA, α is a constant, $\mathbf{X}$ is the vector of time–constant variables such as draft number, $\mathbf{X}(t)$ is the vector of time–dependent variables such as performance that are updated each year, and t, the time variable, is the length of tenure in the NBA. β_1, β_2, and γ are the coefficients to be estimated. The hazard rate is exponentiated to keep the hazard rate greater than zero.

Control variables. In the event history analysis we used the same performance indices as those in the study of playing time: *scoring* (points per minute, field–goal percentage, and free–throw percentage), *toughness* (rebounds and blocked shots per minute), and *quickness* (steals and assists per minute). Injuries, trades, and player position were also entered into the event history analysis as control variables. Being injured can obviously affect the length of a player's career. Being traded can also influence career length, although, again, we do not offer a prediction on the direction of the effect. The player's position was included as a control variable to account for the relative scarcity and greater difficulty in replacing larger players (i.e., forwards and centers).

We also included as a control variable the player's team record, measured as the percentage of games won during the season. Because poorly performing teams often make major roster changes in efforts to improve performance, players may be more likely to be cut when teams undergo such rebuilding. In addition, we controlled for tenure in the NBA, since the risk of being cut can be expected to increase with the number of years a player has already spent in the league. The tenure "clock" stopped, however, for some players who left to play for teams in Europe for one or more years and restarted again when they returned to the NBA.

Results

The results of the event history analysis appear in Table 3. The full model with control variables and draft number offered a significantly better fit than the null model (χ^2 = 123, 9 d.f., $p < .01$).

In this sample, 184 events were observed. The proportional hazard rate for dropping out of the NBA was therefore .13. The expected time until the event of being cut from the league occurred at 7.9 regular seasons (or years), without controlling for the independent effect of an increasing hazard rate over time.

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We also analyzed the data using the Weibull model, in which the log of the hazard rate could increase or decrease with the log of time. The results were not significantly different.

Table 3

The Effect of Draft Number on Survival in the NBA*

Variable	β
Forward/center	.84
	(21.34)
Scoring	−45.57•••
	(7.07)
Toughness	−18.46•
	(9.51)
Quickness	−3.17
	(8.37)
Injury	−14.55
	(27.96)
Trade	61.81•••†
	(15.57)
Win	−.74
	(.48)
Tenure	6.88•
	(3.03)
Draft number	3.30•••
	(.61)
Intercept	−299.90•••
	(33.01)
Number of events	184
Number of spells	1455
Chi-square	123

• $p < .05$; •• $p < .01$; ••• $p < .001$, one-tailed tests, except where noted.
* Standard errors are in parentheses. All coefficients and standard errors are multiplied by 100.
† Two-tailed tests.

As hypothesized, the effect of being chosen later in the draft had a significant positive effect on the hazard rate for career mortality ($\beta = .03$, $p < .001$, one–tailed test). To make the coefficient meaningful, it must be exponentiated and converted into a percentage, using $100[\exp(b) - 1]$. For continuous variables, this calculation gives us the percentage change in the hazard rate given a one–unit change in the explanatory variable (Allison, 1983). We can approximate the effect of the explanatory variable by subtracting the change in the hazard rate from the proportional hazard rate, $r(t)$, to get the new hazard rate $r(t)'$. (This calculation is an approximation because it is independent of the moderating effect of tenure on the hazard rate.) The time until the event of interest can then be computed by taking $1/r(t)'$. Using this method, every increment in the draft number raised the hazard rate function by 3 percent. A first–round draft pick would therefore stay in the league approximately 3.3 years longer than a player drafted in the second round.

The results also showed that two performance statistics, *scoring* and *toughness,* significantly affected the hazard rate for career mortality. The effect of a one–standard–deviation increase in the scoring index decreased the hazard rate function by 37 percent, or added approximately 4.6 years to a player's career ($\beta = -.46$, $p < .001$, one–tailed t–test). A similar increase in the toughness index, a measure of rebounding and blocked shots, decreased the hazard rate by 16 percent ($\beta = -.18$, $p < .05$, one–tailed t–test). The results also showed that players were more likely to be cut if they had been traded in the season prior to their release

from the NBA ($\beta = .62$, $p < .001$, two–tailed test).

The duration variable that counted the number of years in the NBA showed that, independent of the other covariates, the hazard function for being cut from the league increased with time ($\lambda = .07$, $p < .05$, one–tailed t–test). Each additional year in the NBA increased the hazard for career mortality by 7 percent. Therefore, one additional year in the NBA decreased the time to the event of being cut from the league by approximately half a year.

ANALYSIS OF BEING TRADED

By the time most players leave the NBA, they are no longer playing for the team that originally drafted them into the league. Therefore one might argue that the analysis of career length is only a rough test of the sunk–cost effect. Because of the frequency of trades, the effects of draft order on career length may largely represent sunk–cost effects that have been passed from team to team as players are traded over their entire careers.[5] Thus a more sensitive test of the initial effects of sunk costs would consist of an analysis of NBA players' first trade. The principal question is whether draft order (having selected a player early or late in the draft) influences teams' decisions to trade players, controlling for their performance as well as other logical predictors. To answer this question, we again used event history analysis.

Method

Sample. The sample of players for this second event history analysis included those who played in the NBA for two or more years. We used a two–year cutoff because players are typically traded during the interim period between seasons, and being traded implies playing at least a second year with a new NBA team. For the analysis of a player's initial trade, we followed players until they were traded from their original team or until the 1990–1991 season, the last year for which we obtained data. Of the 241 players in the sample, 157 were traded at least once.

Dependent variable. The dependent variable was the hazard rate for a player's first trade. Although players are often traded more than once, we focused on the first trade to test specifically whether sunk costs affected the likelihood that NBA teams would trade their draft picks.

Model. Because the decision to trade a player may involve many of the same factors that contribute to the decision to cut players, we again modeled the hazard rate as a linear function of the performance variables, injury, team record, duration, and draft number. The predicted direction of the effects of these control variables is the same as in the previous analysis of career length.

Results

Table 4 shows the results of the event history analysis for the first trade. The full model with control variables and draft number represented a significantly better fit than the null model ($\chi^2 = 40$, 8 d.f., $p < .01$).

In this sample, 157 events were observed. The proportional hazard rate for a player's first trade was therefore .21. The

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This idea was suggested by one of the *ASQ* reviewers of an earlier draft of this manuscript.

expected time until the event of being traded for the first time occurred at 4.8 regular seasons, without controlling for the independent effect of an increasing hazard rate over time.

Table 4

The Effect of Draft Number on First Trade*

Variable	β
Forward/center	3.07
	(24.40)
Scoring	−10.43
	(9.32)
Toughness	−19.04•
	(11.23)
Quickness	−7.52
	(11.39)
Injury	−37.64
	(34.47)
Win	−1.51••
	(.55)
Tenure	7.73•
	(4.50)
Draft number	2.68•••
	(.63)
Intercept	−176.30•••
	(38.12)
Number of events	157
Number of spells	746
Chi-square	40

• $p < .05$; •• $p < .01$; ••• $p < .001$, one-tailed tests.
* Standard errors are in parentheses. All coefficients and standard errors are multiplied by 100.

Draft number had a significant, positive effect on the hazard rate for being traded ($\beta = .03$, $p < .001$, one-tailed test). The hazard rate function was raised by 3 percent with every increment in the draft number. Moving from the first to the second round of the draft therefore increased a player's chances of being traded by 72 percent.

The results also showed that players were less likely to be traded if they were on winning teams ($\beta = -.02$, $p < .001$, one-tailed test). For example, by playing on a team that increased its win percentage by 10 percent, a player would reduce his risk of being traded by 20 percent.

Surprisingly, *scoring* did not have a statistically significant influence on the hazard rate ($\beta = -.10$, $p < 0.1$, one-tailed *t*-test). As in the analysis of career length, however, *toughness* affected the hazard rate for a player's first trade ($\beta = -.19$, $p < .05$, one-tailed *t*-test). A one-standard-deviation increase in this index (based on players' rebounding and blocked shots statistics) decreased the hazard rate function by 17 percent, or added approximately one year to the time until the first trade.

DISCUSSION

The descriptive data on the usage and retention of NBA players are interesting in their own right. As can be seen from these results, a major determinant of a player's time on the court is his ability to score. These same skills also

helped players survive in the league. In contrast, defensive skills such as rebounding and blocked shots (included in our toughness index) were not very predictive of playing time, although they did improve a player's chances of staying on a particular team and remaining in the league. Quickness did not appear as a strong indicator of either playing time or survival over time. Thus one might conclude that, as much as coaches preach the value of team-oriented skills such as rebounding, blocked shots, assists, and steals, they are actually more likely to use individual scoring in their important personnel decisions.

To test the sunk-cost hypothesis, we analyzed data on playing time, survival in the league, and the likelihood of being traded. Each of these analyses supported the sunk-cost effect. Regressions showed that the higher a player was taken in the college draft, the more time he was given on the court, even after controlling for other logical predictors of playing time, such as performance, injury, and trade status. Similarly, the higher the draft number of a player, the longer was his career in the NBA and the less likely he was to be traded to another team, controlling for performance and other variables.

The results of this study are not only consistent, they are also provocative. They challenge conventional models of decision making because the use of sunk costs is specifically excluded from models of rational economic choice. The results of this study also challenge prevailing practices of running professional basketball teams, since as one NBA insider noted, "coaches play their best players and don't care what the person costs. Wins and losses are all that matters."[6]

Alternative Explanations

Because sunk-cost effects are controversial, it is important to discuss a number of alternative interpretations of the results. These alternatives involve versions of economic or decision rationality that could account for the same pattern of results obtained from the NBA data.

One alternative might involve the NBA salary cap. It could be argued that teams were obligated to play and retain top draft choices because of the difficulty of making trades under the salary cap. If the salary of top draft choices could not be spent on substitute players, then teams may have had no choice other than to use their most highly drafted players, regardless of their performance. The logic of this argument has two problems, however. First, under NBA rules, teams are free to trade players who are not performing up to a desired level, as long as their total team salary (after the trade) does not exceed the designated salary cap. Second, under the rules of the salary cap (passed in 1983 and put into effect starting with the 1984 season), teams can exceed the cap by waiving a player (for any reason) and signing a substitute player. The major limitation is that the substitute player's salary cannot exceed 50 percent of the former player's salary. Thus, if a highly paid, early draft choice does not perform up to expectations, it is actually easier to replace that player than the more modestly paid, lower draft choice. There is simply more room under the salary cap to

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From an interview with Terry Lyons, Vice President for International Public Relations, National Basketball Association.

find a qualified player with a high rather than low salary. A sports writer recently lamented this fact in discussing the Golden State Warriors' difficulties in trading Latrell Sprewell (their 23rd pick in the 1992 draft):

> Like it or not, they have an All-Star shooting guard in Sprewell, who is going to be very difficult to trade for value right now because of the salary cap. In the NBA, you have to trade salary slot for salary slot. To find a comparable talent who would match Sprewell's relatively low $900,000 salary would be impossible. (Nevius, 1995)

Another argument concerns the fan appeal of highly drafted players. The reasoning here is that top draft choices, having been stars in college basketball, would likely attract more fans to the stadium than lower draft choices. Thus, regardless of their performance, it might make economic sense for teams to play those who were most highly drafted. The biggest problem with this alternative is that fan appeal is ephemeral. Though popularity among fans may be based on a player's college reputation for the first year or two he is in the NBA, it is likely that popularity erodes quickly if it is not backed up by performance at the professional level. One only has to consider how fast fans soured on college stars such as Ralph Sampson and Danny Ferry, two top draft picks, after their performance in the NBA did not live up to expectations. As a result, we consider fan appeal to be a reasonable alternative for some of the early data on playing time, but not for playing time in years 3 through 5. For these later years, fan appeal is probably so intertwined with NBA performance that it is effectively controlled when prior performance is controlled in the analyses of playing time and turnover.

A third alternative interpretation concerns the value of the draft lottery as a predictor of future NBA performance. It can be argued that draft order contains information not reflected in other performance statistics and that, even with its flaws, the draft is a good predictor of players' future performance. If this is true, then it would be wise for teams to be extremely patient with their top draft choices. Teams should logically play and retain their most highly drafted players, since these are the athletes that will likely perform best in the long run.

Because of the seriousness of this alternative, we conducted some additional quantitative analyses to determine its merits. We checked whether draft order could, in fact, predict subsequent performance of players, beyond what is known about their current level of performance. We regressed the overall performance of players (using an index of scoring, quickness, and toughness) on draft number, as well as prior year's data on each of the performance factors, position, trades, and injuries. The results showed draft number to be a significant predictor of subsequent performance during players' second and third years in the NBA, but not for their fourth or fifth years. Thus, while draft order does appear to contain some useful information on players' early performance, it is not a significant predictor over longer periods of time. This means that the effects of sunk costs cannot be explained by a set of rational expectations contained in the NBA draft. It also means that when NBA teams are especially patient with high draft

choices, such patience cannot be defended simply on the grounds of predicting future performance.

The final alternative we consider concerns the development of young players. Few athletes enter the NBA with the skills to make an immediate impact at the professional level. An investment of playing time is often necessary to bring players up to the level of competition in the NBA. Thus if top draft choices hold the most promise, it may logically make sense to invest the most playing time in these select players. Of course, one might argue, conversely, that playing time should be invested in lower draft choices, since these players must make the greatest jump in skill level from the college to professional ranks.

To help determine whether it is wise for teams to invest a disproportionally large amount of playing time in high draft choices, we conducted some additional analyses. We regressed performance for years 2 through 5 on prior year's performance, prior year's time on the court, draft number, and the interaction of draft number and playing time. The results of these four analyses supported a straightforward investment hypothesis. For years 2, 3, and 4, an increase in performance was associated with the investment of prior playing time. But none of the interactions of draft choice and playing time proved to be significant predictors of subsequent performance. From these results it does not appear that it pays off for teams to invest playing time disproportionally in high rather than low draft choices, at least when prior performance is held constant.

As evidenced by these several analyses, we believe the sunk-cost effect can survive a great deal of logical and empirical scrutiny. Because of the many controls allowed by this research, and the consistency of the results across separate empirical tests, we can be relatively confident that sunk costs influenced personnel decisions in professional basketball. Teams did play their most expensive players more and hang on to them longer than players they expended fewer resources to obtain. Such observations would be obvious, of course, if it were not for the fact that the effects were always *beyond* those explained by performance variables. Such observations would likewise be ambiguous if it were not for the fact that other alternatives did not account for the results as well as the sunk-cost hypothesis did.

Although we have discounted a series of alternatives to the sunk–cost hypothesis, this does not mean that decision makers have not pursued what *they* have perceived as logical goals in making personnel decisions. Team managers and coaches may play high draft choices, beyond what is merited by their on–court performance, because they believe these players will soon excel. They may believe that a little extra playing time will soon pay off in on–court performance. All of these beliefs can appear logical to the actors involved, and they may indeed be the kind of reasoning (or justification) stimulated by sunk costs. Our purpose has not been to rule out these notions as *perceived* rationales for decision making, but to demonstrate that these arguments

cannot serve as rational explanations for the sunk–cost effect.

Reexamining the Sunk–cost Literature

The sunk–cost effects observed in the NBA data are analogous to those found by Arkes and Blumer (1985) in their resource utilization studies, in which subjects chose to use more expensive tickets or food over those that were the same in all respects except cost. The NBA results can, however, be contrasted with studies using project–completion scenarios. As noted earlier, Garland and his associates found that decision makers were more likely to invest in a project when it was closer to completion but that the amount of prior investment did not, by itself, predict future allocations. Why is there such a disparity between the results for sunk costs in the product–usage versus project–completion situations? And does this disparity mean that sunk costs are not important to projects for which large sums have been expended without there being substantial progress toward completion?

The disparity in results between product–usage and project–completion situations may have more to do with the way controlled laboratory research is carried out than with the way the world naturally works. Although Garland could not find sunk–cost effects that were independent of project–completion information, this does not mean that costs are unimportant. In natural settings, decision makers may regularly confound the amount they have expended with progress on a project. Such a perceived linkage (or bias) was, for example, observed in Ross and Staw's (1993) study of the construction of the Shoreham nuclear power plant. In the Shoreham case, expenditures moved from approximately $70 million to $5.5 billion over twenty years. During much of this construction period, decision makers believed that the more that was spent on the nuclear plant, the closer would be the plant's opening date. Had decision makers fully anticipated the failure of Shoreham ever to go on–line (and its eventual sale to the State of New York for $1.00), they would have surely abandoned the project at an earlier date. Therefore, experiments that try to hold constant the perceived progress on a project (e.g., by presenting clearcut evidence of success and failure) may be missing a key element of what binds actors to losing courses of action.

Theoretical Mechanisms

Many theoretical mechanisms have been offered for the sunk–cost effect. Arkes and Blumer (1985) described the sunk–cost effect as a judgment error, that people believe they are saving money or avoiding losses by using sunk costs in their calculations. This desire not to "waste" sunk costs can, as we noted, result from an assumed covariation between cost and value (in resource utilization decisions) or between expenditure and progress (in project–completion decisions). It can also result from a more primitive form of mental budgeting in which decision makers simply want to recoup past investments, regardless of their utility (e.g., Heath, 1995). In both formulations, people may be attempting to achieve economic gain; they just do not have the proper tools and information to do it right.

Other explanations of sunk–cost effects go beyond cold miscalculation. They involve "warmer" psychological processes such as framing (Kahneman and Tversky, 1979), self–justification (Aronson, 1984), or behavioral commitment (Kiesler, 1971; Salancik, 1977). A framing explanation would emphasize that sunk costs are losses that must be accepted if the individual decides to dispose of a product or withdraw from a course of action. Thus if framing were the crucial force underlying sunk–cost effects, one might see sunk–cost effects only when an investment is defined in terms of losses rather than gains. Self–justification theory also tends to focus on negative situations, contexts in which one might suffer an embarrassment or loss of esteem if sunk costs were ignored. A crucial assumption of the self–justification explanation is that there is some personal responsibility for prior expenditures; without responsibility, sunk costs may not be a potent factor in decision making. Finally, a behavioral commitment explanation would stress that sunk costs are important only when they have implications for accompanying beliefs. Sunk–cost effects would therefore be strongest when expenditures are made publicly, freely, irrevocably, and are linked to other values or intentions of the decision maker.

The main purpose of the present study was to validate the sunk–cost effect in a natural organizational setting, not to sort out the competing theoretical processes that may underlie this effect. But the NBA data do seem to imply a more complex behavioral process than many social scientists are comfortable with. Consider the fact that most players in our sample (157 out of 241) were traded sometime during their careers. This means that draft order continued to have meaning in player personnel decisions, even though the team deciding to use or cut a particular player may not have been the team that originally drafted him. Such a result implies that people may perceive an association, however faulty, between draft order and the prospect of future performance. For example, even though top draft choices such as Ralph Sampson or Benoit Benjamin failed to produce for the teams that drafted them and were subsequently traded to other franchises, they still retained enough value to survive for many years in the NBA. Being drafted high may create such a strong expectation of performance that the belief persists long after the decline in court skills. As a result, sunk costs can actually be passed from one team to another, with each transaction being influenced by an overestimate of the performance of the traded player.[7]

Although the transferability of sunk costs across NBA franchises makes the effect look like a cold error in calculation or misjudgment, there is more to sunk costs than this. Consider the fact that being traded, by itself, was always associated with a decrease in the usage and retention of NBA players, even when performance was held constant (see Tables 2 and 3). This means that the further teams were from the original drafting of players, the less committed they were to them (see Schoorman, 1988, for a similar effect on performance appraisals in industry). The influence of being traded on player usage and retention

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Personal communication with Max Bazerman, 1994.

therefore implies that "warmer" psychological processes such as justification and commitment may also play a part in the sunk–cost phenomenon.

In our view, the presence of cognitive bias, commitment, wastefulness, and justification may all be interwoven in natural situations. In the case of the NBA, taking a player high in the draft usually involves some extremely high, often biased, estimates of the person's skills. The draft also involves a very visible public commitment, one that symbolizes the linkage of a team's future with the fortunes of a particular player. Moreover, the selection of a player high in the draft signals to others that a major investment is being made, one that is not to be wasted. If the draft choice fails to perform as expected, team management can expect a barrage of criticism. Having to face hostile sports commentators as well as a doubting public may easily lead to efforts to defend or justify the choice. In the end, team management may convince itself that the highly drafted player just needs additional time to become successful, making increased investments of playing time to avoid wasting the draft choice.

As illustrated by this basketball scenario, there may be multiple processes underlying the sunk–cost effect. Such complexity may be anathema to the traditional goal of seeking a single parsimonious cause for behavior and for finding the one theoretical model that dominates others as a causal explanation. Yet we may be forced to live with this kind of complexity in understanding sunk–cost effects. Picking apart the various theoretical explanations has little utility if the natural situation includes multiple causal forces. Our task in future research therefore goes beyond showing whether a particular set of antecedents can lead to sunk–cost or escalation effects. It is to map the set of consonant and conflicting forces as they naturally occur in organizational settings, knowing full well that this search involves as much understanding of the context as the theoretical forces involved.

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APPENDIX: Means, Standard Deviations, and Correlations among Variables*

Variable	1	2	3	4	5	6	7	8	9	10	11
1. Draft number											
2. Forward/center	−.07										
3. Minutes in year 2	−.45**	.03									
4. Scoring in year 1	−.34**	.02	.38**								
5. Toughness in year 1	−.05	.08	.02	.05							
6. Quickness in year 1	−.07	.03	.15*	.09	−.05						
7. Trade in year 2	.29**	.03	−.34**	−.18**	−.14*	−.10					
8. Injury in year 2	.00	−.07	−.10	.08	−.03	.06	−.12				
9. Win in year 2	−.13*	.11	−.01	−.12	.03	−.09	−.07	.02			
10. Minutes in year 3	−.41**	.03	.75**	.34**	.09	.10	−.25**	.02	.04		
11. Scoring in year 2	−.29**	.01	.57**	.39**	.05	−.01	−.09	.03	.00	.56**	
12. Toughness in year 2	−.06	.07	.07	.04	.76**	−.19**	−.10	−.04	.09	.07	.15*
13. Quickness in year 2	−.04	.06	.23**	.16*	−.20**	.65**	−.14*	.02	−.04	.10	.13*
14. Trade in year 3	.21**	−.09	−.36**	−.12	−.14*	−.06	.26**	−.04	−.14*	−.38**	−.28**
15. Injury in year 3	−.09	.00	.04	.07	.01	−.02	−.03	.12	−.05	−.19**	.02
16. Win in year 3	.01	.12	.07	.07	.10	.00	−.05	−.05	.46**	.06	.16*
17. Minutes in year 4	−.30**	−.09	.57**	.29**	.00	.10	−.29**	.09	.02	.68**	.45**
18. Scoring in year 3	−.31**	.01	.45**	.36**	−.04	−.02	−.01	−.08	−.02	.58**	.57**
19. Toughness in year 3	−.08	.06	.01	−.03	.81**	−.25**	−.12	−.10	.17*	.11	−.01
20. Quickness in year 3	−.12	.09	.34**	.02	−.23**	.80**	−.13	.04	−.06	.24**	.17*
21. Trade in year 4	.09	−.04	−.27**	−.04	−.01	.01	.09	−.03	.04	−.33**	−.25**
22. Injury in year 4	.03	.11	.06	.07	.08	.07	.02	.00	−.04	.01	.04
23. Win in year 4	.02	.04	.10	.08	.04	.02	−.15*	−.02	.22**	.03	.09
24. Minutes in year 5	−.23**	.01	.41**	.22**	.04	.06	−.12	−.04	.03	.58**	.38**
25. Scoring in year 4	−.25**	−.13	.38**	.42**	−.21**	−.01	−.11	.09	−.02	.46**	.59**
26. Toughness in year 4	.04	.02	−.08	−.07	.79**	−.32**	−.08	.01	.03	.03	−.15*
27. Quickness in year 4	−.11	.01	.33**	.22**	−.26**	.61**	−.07	.09	−.02	.27**	.23**
28. Trade in year 5	.16*	−.03	−.26**	−.04	−.09	−.30	.23**	.13	−.01	−.14	−.11
29. Injury in year 5	−.14	−.02	.15*	.09	.08	.14	−.08	−.02	.00	.10	.02
30. Win in year 5	.00	.03	.03	−.02	−.08	.04	−.13	−.09	.22**	−.05	.03
Mean	21.11	.62	1464.32	.01	−.02	−.05	.23	.05	48.20	1603.73	−.01
S.D.	(12.74)	(.49)	(918.78)	(.73)	(.73)	(.73)	(.42)	(.21)	(13.38)	(967.80)	(.77)
N	275	275	241	275	275	275	241	241	241	211	241

Variable	12	13	14	15	16	17	18	19	20	21	22
13. Quickness in year 2	−.16*										
14. Trade in year 3	−.12	−.06									
15. Injury in year 3	.02	.03	−.06								
16. Win in year 3	.08	.01	−.23**	−.14*							
17. Minutes in year 4	.07	.13	−.25**	−.07	.02						
18. Scoring in year 3	−.02	.05	−.17*	−.07	.05	.38**					
19. Toughness in year 3	.80**	−.25**	−.13	−.07	.10	.08	.02				
20. Quickness in year 3	−.23**	.75**	−.10	−.02	.02	.22**	.13	−.20**			
21. Trade in year 4	−.03	−.05	.11	−.05	−.06	−.30**	−.24**	−.04	−.07		
22. Injury in year 4	−.25	.07	−.13	.20**	.09	−.26**	−.08	−.06	.00	.01	
23. Win in year 4	.08	−.06	−.11	.03	.59**	.12	.07	.04	.01	−.02	−.02
24. Minutes in year 5	.08	.14	−.16*	−.19*	.10	.70**	.36**	.05	.18*	−.28**	−.14
25. Scoring in year 4	−.17*	.14	−.13	−.17**	.04	.48**	.54**	−.12	.09	−.15*	−.11
26. Toughness in year 4	.83**	−.26**	−.07	−.04	−.07	.03	−.14	.83**	−.30**	.01	−.01
27. Quickness in year 4	−.21**	.73**	−.13	.01	.03	.22**	.22**	−.23**	.71**	−.06	−.01
28. Trade in year 5	−.14	−.09	.08	−.07	−.12	−.27**	−.06	−.10	−.10	.20*	.03
29. Injury in year 5	.08	.08	−.17*	.20*	.07	−.06	.03	.11	.09	−.05	.29**
30. Win in year 5	−.03	−.01	−.15	−.04	.36**	.07	−.15	−.03	.05	.01	.02
Mean	.03	−.02	.26	.06	49.13	1737.95	.00	.04	−.03	.25	.10
S.D	(.78)	(.85)	(.44)	(.24)	(14.09)	(917.85)	(.74)	(.74)	(.68)	(.43)	(.30)
N	241	241	211	211	211	187	211	211	211	187	187

Variable	23	24	25	26	27	28	29	30
23. Win in year 4								
24. Minutes in year 5	.14							
25. Scoring in year 4	.07	.45**						
26. Toughness in year 4	−.02	.05	−.05					
27. Quickness in year 4	.01	.17*	.32**	−.20**				
28. Trade in year 5	−.18*	−.19**	−.08	−.02	−.05			
29. Injury in year 5	.06	−.32**	−.10	.02	.07	−.08		
30. Win in year 5	.52**	.06	.05	−.10	−.05	−.22	−.11	
Mean	50.55	1759.82	.00	−.03	−.04	.22	.13	50.65
S.D.	(13.93)	(954.41)	(.73)	(.80)	(.85)	(.41)	(.34)	(13.51)
N	187	165	187	187	187	165	165	165

* $p < .05$; ** $p < .01$; two-tailed tests.
* The sample size for an intercorrelation is the lower number of the pair.