# The incentive effects of long-term contracts on performance - Evidence from a natural experiment in European Football*

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**Abstract**

The empirical analysis of the impact of long-term contracts on performance is challenging for two reasons: first, it is difficult to get adequate performance measures and second, potentially negative incentive effects of long-term contracts are countervailed by selection effects when workers with higher abilities get longer contracts. We adopt data from professional sports to disentangle selection and incentive effects. The famous Bosman judgement in European football provides a natural experiment as it has led to an exogenous increase in the contract length of players independently of ability, and can hence be used as an instrumental variable to solve the endogeneity problem associated with the contract length. Using data from the German "Bundesliga", we find evidence that long-term labor contracts reduce average performance of professional players. In addition, we find that longer contracts influence the distribution of performance asymmetrically in the sense that they increase the probability of poor performances but do not reduce the probabilities of good performances.

Keywords: contracts, incentive effects, moral hazard, instrumental variables, natural experiment

JEL classification: J33, J41, J44, C21

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## 1 Introduction

**Motivation** The main problem when empirically assessing how individuals (would) respond to different incentive schemes, e.g. induced by changes in their contractual situation, is the interplay of incentive and selection effects. Therefore, measuring incentive effects by simply comparing the behavior of individuals operating in different contractual environments is likely to be obscured by the presence of selection bias when contract assignment does not occur randomly, but is the result of choices made by individuals who are heterogeneous, for example, with respect to preferences or ability. As a consequence, in trying to disentangle these two effects, researchers have either resorted to controlled environments such as field or lab experiments or have searched for natural experiments induced, for example, by unanticipated policy changes (see e.g. the surveys by Prendergast (1999) and Chiappori and Salanie (2003)), and the literature review below).

A second problem occurs with respect to measurement and observability of crucial variables such as individual performance or (unobserved) innate ability. Individual performance is often hard to measure either because of a job's complexity, consisting of many different tasks or because only aggregate performance measures are available. Moreover, different jobs require different amounts of innate ability, and the more it matters, the more one has to resort to proxies such as education or experience on the job, provided they exist. Finally, when attempting to measure the incentive effects induced by contracts, detailed contract information (such as base wages, bonus components, duration) is required, which is often unavailable to the researcher.

As a result of these problems, empirical research is often carried out in sectors such as agriculture, forestry or the automobile industry (assembly lines), where jobs are relatively easy and where individual performance measures are readily available. In this respect, also the professional sports sector has proven highly popular, not because tasks are necessarily easy or innate ability does not matter, but because of the great amount of information on performance, contracts and allegedly useful proxies for ability such as a player's career games (for a discussion, see e.g. Kahn, 2000).

In this paper, we assess the incentive effects of *long-term contracts* which play an important role in many contexts such as procurement or employment relationships in arts, in professional sports and for civil servants, for instance. We exploit a natural experiment from European professional sports where the unanticipated regime change caused by the "Bosman judgement"[1] which will be described in greater detail below allows to seperate incentive effects from selection issues.

The Janus-faced role of long-term contracts as incentive devices is well-known: As long as it is not yet secured, a long-term contract serves as a potentially powerful *pre-contractual* incentive device. However, once signed, the incentive structure changes dramatically: Because an agent's income is now secure for a longer period of time (until the contract expires), *post-contractual* effort is likely to decline due to an insurance effect, an effect we will refer to as *moral hazard*. This leads to the testable prediction that a workers's performance should be lower for higher contract durations. However, when empirically identifying the moral hazard effect associated with long-term contracts, the same types of problems as outlined above arise because high ability workers may get longer contracts.

In this respect, while the availability of data (e.g. on player characteristics and performance, contracts and team characteristics) in the sports context is better than in most other industries, the interplay of incentive and selection effects remains, as the assignment of long-term contracts is not random, but will again depend on (potentially unobservable) characteristics of individuals. For professional sports, many papers (see already Kahn (1993)) find that top players are more likely to get long-term contracts. Clearly, as top players have on average longer contract durations and ceteris paribus perform better than mediocre ones, this suggests a *positive* relationship between contract duration and performance, and negative moral hazard effects may be hardly identifiable.[2]

**Institutional background** Before explaining our empirical strategy for disentangling incentive from selection effects, we must first provide some institutional background information required to understand the impact of the Bosman judgement on the European professional sports market. First and in contrast to the US, it is common for players to move frequently to other teams throughout their careers. Transfers often take place while a player's contract with his current team is still valid which, in our data, is the case in roughly 75% of all transfers. In these cases, the player's current contract determines the threat point in (re-)negotiations

[1] See European Court of Justice of the European communities, Case C415/93, December 1995.

[2] In fact, while existing studies in the sports sector such as Stiroh (2007); Berri and Krautmann (2006); Maxcy (1997) confirm that long-term contracts do serve their role as pre-contractual incentive devices as predicted, the evidence is much less clear with respect for the (post-contractual) moral hazard effect.

between the player, and the managers of the new and the current team. In particular, valid contracts give the current team a veto right for the transfer, and team managers can hence extract high payments from new teams (called *transfer fees*) for letting the player go. Importantly, the transfer fee will be increasing in the remaining duration of the player's contract, because the current team can threaten to "lock up" the player for a longer time.[3]

Before the Bosman judgement, the current team even had some veto power after a player's contract had expired; but less so compared to the case before contract expiration. For expired contracts, transfer fees were administered by the governing bodies of European soccer (national and international), and were increasing in the strength of the new team and decreasing in the strength of the old team in order to stabilize the league's competitive balance.

The main effect of the Bosman judgement was putting an end to this latter practice; since then, teams no longer have veto power over out-of-contract players and hence are no longer entitled to transfer fees when such players are transferred. As long as a player's contract had not yet expired, the judgement had no effect, though.[4] As a consequence of the legal change, the impact of contract duration on transfer fees actually paid has increased after the Bosman judgement, the reason being that new clubs can now engage out-of contract players without paying transfer fees at all. In section 2, we develop a simple theoretical model explaining that this leads to a higher average contract length *after* the Bosman judgement which hence can, from a theoretical point of view, be used as an instrumental variable when assessing the impact of the contract length empirically. The model is quite simple as it is based on the trade-off between the disadvantage of long-term contracts (moral hazard which is unaffected by the Bosman judgement) and the benefit from receiving higher transfer fees, but seems appropriate to capture the judgment's main impact, and to structure the empirical material.

**Empirical framework and main results** As just explained, the unanticipated regime change induced by the Bosman judgement can be used as an instrumental variable in disentangling the selection and the incentive effect, as it has lead to an *exogenous* variation in

[3] For example, Feess et al. (2009) find that one more year of remaining contract duration increases the average transfer fee by 120 per cent.

[4] The process was initiated in 1993 by the Belgian player Jean-Marc Bosman who was out-of-contract, but whose current team R.C. Liegois demanded a transfer fee which was higher than any club was willing to pay. The case went up the European Court of Justice which in December 1995 considered transfer fees for expired contracts a violation article 39 of the Treaty of Rome, as it hampered the mobility of professionals and was therefore not in accordance with European Labor Law.

contract durations, without directly affecting further variables of interest. The empirical analysis is based on data for the German top professional soccer league (”Bundesliga”) covering the seasons 1994/95 to 1999/2000. The first two seasons are in the pre-Bosman regime whereas the remaining are in the new regime. Using the two leading German soccer magazines “Kicker” and "Sport-Bild", we have compiled a data set with detailed information on player performance, contract duration, and annual remuneration.

The empirical analysis proceeds as follows: First, we demonstrate that the Bosman judgement has lead to a sharp and significant increase in the average contract length, thereby confirming our theoretical model as well as our main idea of using the Bosman judgement as an instrument for the contract length. Second, simple OLS regressions show that the contract duration has a strong and significant positive impact on performance, even when controlling for player quality using the available proxies. With respect to the strength of the moral hazard effect, this allows for two interpretations: either it simply does not exist in our context (for instance, due to the fact that player performance on the pitch can be observed week by week, due to career concerns or due to some kind of gift exchange in response to the trust expressed by long-term contracts) or it cannot be identified using OLS. In this latter case, the available quality proxies are simply not good enough to fully account for the heterogeneity in quality, and moral hazard is not identified due to the problem of unobserved heterogeneity.

In a third step, we estimate the model by 2SLS where the contract duration is instrumented by a dummy variable indicating the regime change after the Bosman judgement. As it turns out, the OLS estimates are reversed and we find a (weakly significant) negative impact of contract length on performance. Hence, exploiting the Bosman judgement, a moral hazard effect induced by long-term contracts is identified in our analysis.

Finally, we employ recently developed methods to estimate the causal effect on conditional quantiles of the performance distribution. Our results indicate an asymmetric effect of contract duration on the distribution of performance. We find that longer contracts increase the probability of playing badly (reduce the lower quantiles), but do not affect the probabilities of top performances (no effect on higher quantiles). We believe that this is a very interesting answer to a question that, to our knowledge, has never been addressed in applied contract theory.

**Relation to the literature** The main purpose of our paper is to identify a (post-contractual) moral hazard effect in a setting where agents self-select into different contracts. As explained above, disentangling incentive and selection effects is widely recognized as one of the most intriguing problems in empirical contract theory, and failure to do so leads to biased estimates concerning the incentive effects attributed to the contractual environment. As a result, to separate incentive and selection effects, many prominent studies were carried out in contexts where the contract assignment was either fully exogenous resulting in a natural experiment, (e.g. Abbring et al., 2003; Ackerberg and Botticini, 2002; Banerjee et al., 2002) or where the same individuals could be observed under *different* contractual environments (e.g. Lazear, 2000; Paarsch and Shearer, 1999, 2000; Shearer, 2004; Bandiera et al., 2007). Moreover, as for the problem of performance measurement, attention is typically directed to sectors such as agriculture, forestry or the automobile industry (assembly lines) where tasks or jobs are relatively easy and individual performance measures are readily available, and where the focus is then on assessing the impact of incentive pay (piece rates as opposed to fixed wages) on individual output.[5]

While the performance effect of incentive schemes such as piece rates is by now reasonably well understood, much less work has been done on empirically assessing the incentive effects of long-term contracts. In particular, we are not aware of any previous study which exploits a legal regime change as a natural experiment to identify the performance-related impacts of long-term labor contracts, in particular (post-contractual) shirking. Recent studies for the sports sector such as (e.g. Stiroh, 2007; Berri and Krautmann, 2006; Maxcy, 1997) confirm that long-term contracts serve their (pre-contractual) role in stipulating effort incentives, but results are much less clear with respect to post-contractual incentives. For example, using various performance measures, the evidence presented in Stiroh (2007), Berri and Krautmann (2006) and Maxcy et al. (2002) is weak, and how study strongly suggests that this might be attributed to the fact that quality proxies alone do not allow for disentangling the moral hazard effect from the selection effect.

Also related is the empirical work on intertemporal effort provision (e.g. Asch, 1989; Oyer, 1998), when performance is evaluated (and a bonus is paid) periodically which leads to a drop

[5] One notable exception is Abbring et al. (2003) who analyze the incentive effects of insurance contracts.

in performance at the beginning of each evaluation period compared to the performance when the evaluation date comes closer. Also in line with the moral hazard effect identified in our paper, are the experimental results in Falk et al. (2007) who analyze incentive effects of long-term contracts (induced by guaranteed employment) and where effort levels tend to be lower compared to spot contracting.

The theoretical idea underlying our paper is that the contract duration agreed upon by the player and the management of his team reflects the trade-off between rent seeking and shirking (see section 2). In this respect, we build on previous work where we have spelled out this trade-off in full formal detail, and where we show that the Bosman judgement should lead to longer contract durations Feess and Muehlheusser (2003).[6] More generally, the role of contracts as rent seeking devices has been stressed in the economic literature since Diamond and Maskin (1979), and Aghion and Bolton (1987) who emphasize the close relationship between breach penalties and contract durations.[7]

The Bosman judgement has established more flexible labor markets, and that more flexibility leads to longer contracts has empirically already be shown by Kahn (1993) by using data from US Major League Baseball. Finally, other notable recent contributions on transfer fee rules in European football focus on investment incentives (Terviö 2006) and inefficiencies related to the distribution of jobs between novices and experienced players (Terviö 2009).

From an econometric point of view this paper is based on the modern analysis of causal effects in the framework of potential outcomes. The question we address is how the performance of a player is expected to change when he gets a long-term contract instead of a short-term contract. A survey of this framework is e.g. Imbens and Wooldridge (2009). We estimate both average causal effects and quantile causal effects.

The remainder of the paper is structured as follows: In section 4, we develop a simple theoretical model allowing to capture the basic trade-offs governing the duration of contracts in our context, and the impact of the Bosman judgement. In section 3 we describe the data and perform some descriptive analyses. Section 4 describes the econometric model, and section

[6] Related arguments why the average contract length should increase after Bosman are verbally developed by Antonioni and Cubbin (2000).

[7] In an different context, Joskow (1985) stresses the role of long-term contracts in stipulating investment incentives. Moreover, from a purely theoretical point of view, Harris and Holmström (1987) and Fudenberg et al. (1990) analyze under which conditions a long-term contract can be replicated by a sequence of short-term contracts.

5 discusses the empirical results. Section 6 concludes.

# 2 Theory

## 2.1 The model

In this section, we develop a fairly simple model allowing to capture the trade-off between effort choice and rent-seeking motives when deciding on the contract duration. The model explains why the Bosman judgement has led to longer contracts, and why the interplay between selection and incentives effects leads to an ambiguous relationship between contract duration and player performance.

At date $-1$, a player and the management of his initial team ("team $i$") decide cooperatively on the player's contract duration $D$, thereby maximizing their joint surplus.[8] The player then starts playing for team $i$ at date 0, and we normalize his career horizon to 1. For the player's performance denoted by $e(e_0, D)$, we take a reduced-form approach and assume that it depends on ability $e_0$ and on contract duration $D$ in the following way:[9]

**Assumption 1** *The player's average performance per unit of time in team $i$, $e(e_0, D)$, is increasing in ability $e_0$, and decreasing in contract duration $D$.*

The first effect is straightforward. As for the second, rather than endogenously deriving the moral hazard effect by determining the player's optimal (average) effort for a given contract duration, we take a reduced-form approach by simply assuming that the moral hazard effect exists for any given $D > 0$.[10] Finally, we assume that the monetary surplus generated by the player is equal to $e$, so that the terms "productivity" and "performance" are used synonymously throughout.

To capture the rent-seeking motives of long-term contracts, we assume that at some date $t \in (0, 1)$, a productivity shock occurs which gives the player a higher productivity $E(e_0)$ per

[8] For our purposes, it is irrelevant how the two parties divide the surplus by setting the player's wage appropriately.

[9] As is standard in the literature, this productivity is meant to capture the marginal revenue that can be attributed to a player such as, for example, increases in TV money, merchandizing sales or premia from international competitions.

[10] We prefer to adopt a simple reduced form-approach for three reasons: first, the model is mainly supposed to structure the empirical material, and in particular why the validity of the Bosman judgement as an instrument is not only supported by econometric tests, but also by clear-cut theoretical considerations. Second, we have developed an explicit theoretical on the moral hazard problem already in Feess and Muehlheusser (2003).

unit of time in another team, the "new" team $n$.[11] We assume $E(e_0) > e(e_0, 0)$ so that the player is more productive in team $n$ regardless of the contract duration with team $i$. When team $i$ and the player decide upon the contract length, both $e(\cdot)$ and $E(e_0)$ are common knowledge.

As there is no asymmetric information in the model, we make the standard assumption that negotiations are efficient such that they maximize the joint surplus of the parties involved in the negotiations. As already mentioned above, this implies that the initial contract will maximize the joint surplus of the player and team $i$. Furthermore, the renegotiation process at date $t$ will be efficient, i.e. the player joins team $n$ at date $t$ regardless of the terms of the initial contract, and regardless of the transfer system. The player then plays for team $n$ with productivity $E(e_0)$ per unit of time until the end of his career at date 1.

### 2.2 Renegotiation at Date $t$

Following the discussion in section 1, the two legal regimes can be formalized as follows:

**Definition 1** *A transfer system $l = P, B$ is characterized by two legally administered transfer fees (veto sums) $r^{lc} = (r^{lV}, r^{lN})$ per unit of time which team $i$ must accept if the player wants to join team $n$ at date $t$ under contract situation $c \in \{V, N\}$.*

The upper index $V$ indicates that the player still has a valid contract, whereas $N$ means that the contract has expired. The fees $r^{lc}$ are *veto sums* for the initial team, and thus constitute *upper* bounds for the transfer fees actually paid. To distinguish these fees from transfer fees paid, we refer to the first ones as veto sums. Furthermore, expressing all magnitudes per unit of time is helpful to separate the impact of the transfer fee system from the impact of the contract length.

Recall from section 1 that we have $r^{PV} = r^{BV} = \infty$ since there is no fee under either system which the old team *must* accept if the player has still a valid contract. For expired contracts, the old team has lost all of its veto power after the Bosman judgement, i.e. $r^{BN} = 0$. Under system $P$, however, the old team had positive veto power even for expired contracts, but less so compared to valid contracts. Hence, $0 < r^{PN} < \infty$.

We summarize the legal situation in the following table:

[11] The assumption that the shock occurs with certainty is not restrictive. We could also allow for the possibility of probabilistic shocks somehow distributed over time, so that the player would continue to play for team $i$ until a shock occurs.

| | Valid contract ($V$) | No contract ($N$) |
|---|---|---|
| Pre-Bosman ($P$) | $r^{PV} = \infty$ | $0 < r^{PN} < \infty$ |
| Bosman ($B$) | $r^{BV} = \infty$ | $r^{BN} = 0$ |

*Table 1: Veto sums for team i*

Note that $r^V - r^N > 0$ under both regimes, and that this difference is larger under regime $B$.

To determine the renegotiation payoff, assume that the player has signed a contract with team $i$ stipulating some contract length $D$, and that he wants to join team $n$ at date $t$. Define $\alpha_n(e_0, r^{lc})$ as team $n$'s renegotiation payoff per unit of time as a function of ability, and of the veto sum depending on the transfer fee system and the player's contractual situation. As will become clear, no further notation is needed for the player and for team $i$ who are jointly reaping the remaining part of the surplus generated per unit of time.

Instead of modeling this three-party renegotiation process explicitly, we follow our reduced-form approach and impose the following natural assumptions on $\alpha_n(e_0, r^{lc})$:[12]

**Assumption 2** *For all $r^c \in [0, \infty]$, the renegotiation payoff of team $n$ per unit of time is (i) strictly increasing in $e_0$, (ii) weakly decreasing in $r^{lc}$ and (iii) satisfies $\frac{\partial^2 \alpha_n(e_0, r^c)}{\partial e_0 \partial r^c} \leq 0$.*

Team $n$ benefits from hiring a player with higher ability as this increases the available renegotiation surplus, and thereby ceteris paribus also the share of team $n$. Next, $r^c$ measures the old team's veto power, so that the new team's share is weakly decreasing in $r^c$.[13] The last property implies that team $n$'s marginal benefit from hiring a player with higher ability is decreasing in the old team's veto power. To see that this is also a natural assumption, note that a sufficient condition for it to hold is that team $n$ gets a fixed percentage of the renegotiation surplus for any $r^c$ *given*.[14] For example, suppose team $n$ gets $\frac{1}{4}$ of the surplus for $r = r^V$ and

[12] In Feess and Muehlheusser (2003), these properties are endogenously derived from a bargaining game in which the player negotiates simultaneously with both clubs in Nash-bargaining fashion. They build on an idea initially developed by Burguet et al. (2002). Note that applying the Shapley value is tedious here, because we would have to specify the payoffs not only when club $i$ either has full veto power (when $r = \infty$) or no veto power at all ($r = 0$), but also for all $r \in (0, \infty)$. In fact, to the best of our knowledge, the Shapley value has been used in contract theoretic models only when, using our terminology, either $r = 0$ or $r = \infty$ were considered. Examples include Segal and Whinston (2000) who use a slightly more general concept when analyzing exclusive dealing clauses or Hart and Moore (1990) for the case of asset ownership.

[13] We assume weak monotonicity only, since in reality it does not make any difference for the threat points in the renegotiations whether the veto sum for a mediocre player is 200 or 300 million Euro.

[14] Recall that this percentage itself is decreasing in $r^c$ due to part (i) of the assumption.

$\frac{1}{2}$ for $r = r^N < r^V$. When the renegotiation surplus increases from 100 to 200, say, his payoff increases by 25 (from 25 to 50) if $r = r^V$ and by 50 (from 50 to 100) if $r = r^N < r^V$.

As each regime $l = P, B$ is completely specified by two numbers $r^{lV}$ and $r^{lN}$, we save on notation by writing $\alpha_n^{lc}(e_0)$ from now on. Recalling that $\alpha_n^{lc}(e_0)$ is expressed per unit of time, the overall renegotiation payoff is obtained by summing over time. [15]

Then, team $n$'s total renegotiation under regime $l = P, B$ is given by

$$\pi_n(e_0, D, r^l) = (D - t) \cdot \alpha_n^{lV}(e_0) + (1 - D) \cdot \alpha_n^{lN}(e_0) \tag{1}$$

The first (second) term is the payoff for the period for which the player's contract is still valid (has expired). Using Assumption 2 and Table 1, this leads to the following result:

**Proposition 1** *(i) Under both regimes, the total renegotiation payoff for team n is decreasing in the contract length D, i.e.* $\frac{\partial \pi_n(\cdot)}{\partial D} < 0$.

*(ii) The marginal effect is larger under regime B, i.e.* $\left|\frac{\partial}{\partial T}\pi_n(\cdot, r^B)\right| > \left|\frac{d}{dR}\pi_j(\cdot, r^P)\right|$.

*Part (i)* follows immediately from the fact that the old team's veto power is, under both regimes, higher when the player has still a valid contract. And as the difference between the veto power with and without valid contract is higher under regime $B$, so is the impact of the contract length.(recall from table 1 that $r^{BV} - r^{BN} > r^{PV} - r^{PN}$).

### 2.3 The Initial Contract

Under each regime, the duration of the initial contract will maximize the joint surplus of the player and team $i$ given by

$$JS(e_0, D, r^l) = te(e_0, D) + \Big[(D - t)\left(E(e_0) - \alpha_n^{lV}(e_0)\right) + (1 - D)\Big(E(e_0) - \alpha_n^{lN}(e_0)\Big)\Big] \tag{2}$$

.

Until date $t$, the player plays for team $i$ with productivity $e(\cdot)$ per unit of time. In renegotiations at date $t$, the two parties get the total surplus $(E(e_0))$ minus the share of team $n$.

[15] Note that we can, without loss of generality, restrict attention to $T \in [t, 1]$ as the player is out-of-contract when the shock occurs for all $T \leq t$.

As shown above, this share differs for the two phases where the player's contract would still be valid $(D-t)$ or has expired $(1-D)$, respectively.

Eqn. (2) shows nicely that all we need to take care of when deriving the contract length that maximizes the joint surplus of the old team and the player are the *total* surplus and the share of team $n$; we do not have to think about the surplus division between team $i$ and the player. Eqn. (2) exhibits the underlying trade-off when deciding on the optimal contract duration: Since $\alpha_n^{lV} < \alpha_n^{lN}$, team $n$'s renegotiation share is decreasing in $D$ which means that long-term contracts can be used by the contracting parties as rent-seeking devices. On the cost side, since $e(e_0, D)$ is decreasing in $T$, the longer the contract, the lower a player's average performance in team $i$. Hence, the privately optimal contract length balances the benefit from rent-seeking and the costs from moral hazard at the margin.

Assuming an interior solution, the optimal contract duration $D^l(e_0)$ under regime $l = P, B$ is implicitly given by the following first order condition:

$$\frac{\partial}{\partial D} JS(\cdot) = t\frac{\partial}{\partial D^l} e(\cdot) + \alpha_n^{lN}(e_0) - \alpha_n^{lV}(e_0) = 0$$

This leads to the following result:

**Proposition 2** *(i) The optimal contract duration is higher under regime B, i.e.* $D^B > D^P$*.*

*(ii) Under each regime, the optimal contract duration is increasing in the player's ability, i.e.* $\frac{\partial D^l}{\partial e_0} > 0$ *for* $l = P, B$*.*

**Proof.** *Part (i)*: As

$$\alpha_n^{BN}(e_0) - \alpha_n^{BV}(e_0) > \alpha_n^{PN}(e_0) - \alpha_n^{PV}(e_0),$$

$-t\frac{\partial}{\partial T^l} e(\cdot) = \alpha_n^{lN}(e_0) - \alpha_n^{lV}(e_0)$ requires that $-t\frac{\partial}{\partial T^l} e(\cdot)$ is higher under system $B$. And as $e(\cdot)$ is decreasing in $D$ (see Assumption 1), $D^B > D^P$ follows.

*Part (ii)*: Using the implicit function theorem, we get

$$\frac{\partial D^l}{\partial e_o} = -\frac{\frac{\partial\left(\alpha_n^{lN}(e_0) - \alpha_n^{lV}(e_0)\right)}{\partial e_0}}{t\frac{\partial^2}{\partial (T^l)^2} e(\cdot)} > 0$$

as the denominator is negative (by concavity of the objective function (2)), while the numerator is positive by part (ii) of Assumption 2. ■

For *part (i)*, just recall that the marginal benefit from increasing the contract duration is higher after the Bosman judgement as the veto power shrinks to zero for expired contracts. This explains why the Bosman judgement has lead to an exogenous increase of contract durations. The intuition for *part (ii)* is that high-ability players get longer contracts because the *total* renegotiation surplus is higher for better players, and this also increases the incentive to secure a higher percentage of the surplus by increasing the contract length. Both results are not only highly intuitive, but will also clearly be confirmed by the data.

Part (ii) of Proposition implies that there are countervailing effects when measuring the impact of the contract duration on performance. On the one hand, longer contracts ceteris paribus reduce performance, but on the other hand, high-ability players get longer contracts. These opposing effects render the total effect ambiguous: Defining by $e_0^{-1}(D^l)$ the ability level of a player who gets contract duration $D^l$, from *part (ii)* of Proposition 2 clearly follows that $e_0^{-1}(D^l)$ is strictly increasing. Then, the average performance under contract duration $D^l$ is given by $e(D^l) = (e_0^{-1}(D^l), D^l)$. Taking the derivative, then yields

$$\frac{\partial e(D^l)}{\partial D^l} = \frac{\partial e(\cdot)}{\partial e_0} \frac{\partial e_0^{-1}(D^l)}{\partial D^l} + \frac{\partial e^l}{\partial D^l} \lesseqgtr 0,$$

as the first term is positive (selection effect) while the second one is negative (moral hazard effect). This makes the problem of measuring moral hazard empirically challenging.

## 3 Data

Our data cover six consecutive seasons in the German top professional soccer league (”Bundesliga”) from 1994/95 to 1999/2000. The first two seasons are in the pre-Bosman regime, and the remaining four are in the new regime. Using the two leading German soccer magazines “Kicker” and "Sport-Bild", we have compiled a data set with detailed information on player performance, contract duration, and annual remuneration. Player performance per season is measured by a composite index called "kick index" that takes into consideration both position-specific factors such as the number of assists per match for a striker or the number of saves for a goalkeeper, and team specific factors as the result of a match. The exact definition of the

Table 1: Distribution of contract duration in the two regimes

| Contract duration | Pre-Bosman | Bosman |
|---|---|---|
| 1 | 2.84 | 1.56 |
| 2 | 23.40 | 20.91 |
| 3 | 53.90 | 39.01 |
| 4 | 13.48 | 30.16 |
| 5 | 3.55 | 6.52 |
| 6 | 2.84 | 1.85 |
| performance index * | 72.73 | 71.65 |
| wage * | 785461 | 1015467 |
| previous contract expired * | .29 | .23 |

* mean

index is given in the Appendix.

The original sampling scheme was based on flow sampling, i.e. players entered the data when they signed a new contract. This implies that all observations for the season 94/95 are players with a new contract. These players are then followed over the next seasons, and each season new players are added when signing a new contract. The information on the contract terms had to be collected from the print copies of the magazines as they are not officially published. Overall, we have complete information on 313 players who on average are observed in 2 seasons, yielding a sample size of 621 player-season observations. About 16% (101 of 621) of these play under a contract signed before the Bosman judgement.

Table 1 displays the distribution of contract durations in both regimes. It is obvious that there is a clear shift in this distribution with respect to the durations of three and four years: while the proportion of contracts with an duration of three years drops by 15%-points, the proportion of contracts with four years increases by almost the same amount. The proportion of short-term contracts (up to two years) remains more or less unchanged whereas the proportion of very long term contracts (longer than 4 years) also increased somewhat. These findings suggest that the Bosman judgement mostly affected players who previously would have gotten a three-year contract, but now sign four-your contracts.

The final three lines in Table 1 show the sample means of the performance index and the dummy variable indicating whether the previous contract had expired when the new contract was signed. As expected, the proportion of expired contracts is lower under the Bosman regime because the impact of the contractual situation on transfer fees has increased after the Bosman

Table 2: Evolution of contract duration and wage

| season | 94/95 | 95/96 | 96/97 | 97/98 | 98/99 | 99/00 |
|---|---|---|---|---|---|---|
| contract duration | 2.917 | 2.831 | 3.224 | 3.295 | 3.278 | 3.266 |

verdict. The performance index is not significantly different in the two regimes.

Table 2 presents the development of the average contract duration over time and confirms a clear-cut discrete jump between seasons 95/96 and 96/97 (the first under the Bosman regime). Contract duration is roughly 3 months larger after the Bosman judgement which reinforces the credibility of the Bosman instrument as an exogenous shifter in contract characteristics.

As control variables in the empirical analysis we use individual characteristics of the players like age, number of league games, position, being a member of the national team, and nationality. Furthermore, we use two dummy variables, one for contract renewal with the old team and one indicating whether the previous contract had expired before signing the new one. The first dummy variable can be interpreted as an indicator of the a good team-player match which caused the management-player coalition to renew the contract. On the other hand, letting the contract expire indicates that their were insufficient reasons to renew the contract early on, which can be seen as an indicator of unfulfilled expectations. Following our theory, incentives to renew contracts are similar to incentives of signing long-term contracts in the first place, and we will indeed find hints in the empirical analysis that players with expired contracts have lower abilities. Team characteristics are summarized by the team's budget, which picks up the power to attract good players but also other unspecified team fixed effects.[16] Descriptive statistics of all variables used in the regressions are given in the Appendix.

# 4 Econometrics

We estimate the incentive effect of long-term contracts within the framework of the modern causal analysis literature (see e.g. Angrist and Pischke (2009) or Imbens and Wooldridge (2009) for recent surveys). In this literature, the causal effect is defined as the difference of potential outcomes in different treatment states.

[16] We also estimated the model with team fixed effects, but when controlling for budget most team fixed effects are insignificant.

In our case, treatment is the contract duration $D$. To simplify the presentation, we describe contract duration by a dummy variable in this section, with $D = 1$ if a long-term contract (e.g. longer than 3 years) is signed. In the empirical analysis we use both the actual contract duration and the dummy for a long-term contract as teatment variables. The outcome variable performance is denoted with $Y$.[17] Throughout this section, upper case variables denote random variables and lower case variables denote their possible realizations. The causal effect is defined as

$$Y_i|D_i = 1 - Y_i|D_i = 0 \text{ or } Y_{1i} - Y_{0i}$$

Because no observation can be observed in both states this individual causal effect is not identified. However, under specific assumptions we can estimate features of the distribution of the random variable $(Y_{1i} - Y_{0i})$. Most applications focus on estimating the expectation of $(Y_{1i} - Y_{0i}), E(Y_{1i} - Y_{0i})$, which is called the average causal (or treatment) effect. Recently, methods have been developed to estimate quantile causal effects, which allow to show a more detailed picture of the treatment on the distribution of potential outcomes. We now describe the methods to estimate these causal effects in more detail.

## 4.1 Average causal effects

We assume that performance can be described by the following simple linear relationship:

$$Y_i = \alpha D_i + \mathbf{X}_i\beta + U_i, \tag{3}$$

where $\mathbf{X}$ is a vector of control variables such as age or player's position and $U$ is the error term. Then $\alpha = E(Y_{1i} - Y_{0i}) = (\alpha + \mathbf{X}_i\beta) - (\mathbf{X}_i\beta)$ is the average causal effect of the contract duration on performance. It can consistently be estimated by OLS if $E[U|D, \mathbf{X}] = 0$. If there are unobserved variables such as players' ability that are correlated with $T$ after controlling for

[17] Following standard econometric notation, we use $Y$ to denote the outcome. This corresponds to $e$ in the theoretical section.

$\mathbf{X}$ the OLS estimate of $\alpha$ is not the causal effect. Assume that the true model is

$$
\begin{aligned}
Y_i &= \alpha D_i + \mathbf{X}_i\beta + \gamma e_{0i} + U_i && \text{(4a)} \\
D_i &= \mathbf{X}_i\delta_X + \delta_{e_0} e_{0i} + V_i && \text{(4b)}
\end{aligned}
$$

i.e. both performance and contract duration are a function of ability $e_0$. Then it is well known that estimating equation 3 with OLS gives the estimate $\widetilde{\alpha} = \alpha + \gamma\delta_{e_0}$.The term $\gamma\delta_{e_0}$ is the selection bias caused by the fact that, conditional on $\mathbf{X}$, ability $e_0$ has an impact on performance ($\gamma$) and on the contract duration ($\delta_{e_0}$). Only if at least one impact is zero there is no selection bias.

One solution to this endogeneity problem are instrumental variables. An instrument generates variation in $D$ that is uncorrelated with $A$, i.e. it generates exogenous variation in $D$. Technically, we require that

$$
\begin{aligned}
E[\mathbf{Z}'U] &= 0 && \text{(5a)} \\
E[D|\mathbf{X},\mathbf{Z}] &\neq E[T|\mathbf{X}] && \text{(5b)}
\end{aligned}
$$

where $\mathbf{Z} = (Z_1, \mathbf{X})$ is the vector of all exogenous variables and $Z_1$ is the instrument. If we are willing to assume that the effect is homogeneous, i.e. the same for all players, we can interpret the 2SLS estimate of $\alpha$ as the average causal effect of contract duration. These average effects may be heterogeneous, however, i.e. they may differ across subgroups of the population. For example, the effect of a long contract may be different for players who actually get a long contract (the so-called average treatment effect on the treated) and for all players in the population (the average treatment effect). These effects may be different if e.g. team managers have some information on the shirking attitudes of players and only sign long contracts with players who are expected not to shirk. For this subgroup the causal effect may be zero, but in the population of all players it may be negative. Heckman et al. (2006) suggest a simple test for essential heterogeneity in treatment effects. It involves regressing the outcome $Y$ on all control variables, the propensity score (the probability of treatment conditional on $\mathbf{Z}$), the interaction of the propensity score with all controls and polynomials of the propensity score. If the linear model is rejected against the model with polynomials we also have to reject the assumption of homogeneous effects. Using this test, we find only weak evidence for effect heterogeneity. For this reason we believe that 2SLS identifies the average causal effect in our application.

### 4.2 Quantile causal effects

Let us now consider quantile treatment effects. Abadie et al. (2002) propose an estimator of quantile treatment effects based on the estimator of Abadie (2003). This approach has the drawback that it only applies to binary treatments and binary instruments which is too restrictive in our application. For this reason, we apply the estimator proposed by Chernozhukov and Hansen (2005), which is based on different identifying assumptions and allows for continuous treatment and multivalued instruments.

Starting point is the definition of conditional quantiles of potential outcomes. Let $q(d, x, \tau)$ denote the $\tau^{\text{th}}$ quantile function for treatment level $D = d$ and $\mathbf{X} = \mathbf{x}$. In the binary case Chernozhukov and Hansen (2005) define the quantile treatment effect as[18]

$$QTE_\tau(\mathbf{x}) = q(1, \mathbf{x}, \tau) - q(0, \mathbf{x}, \tau). \tag{6}$$

The critical representation used by Chernozhukov and Hansen (2005) is that each potential outcome $Y_d$ conditional on $\mathbf{X} = \mathbf{x}$ can be expressed as

$$Y_d = q(d, \mathbf{x}, U_d), \tag{7}$$

where $U_d|\mathbf{Z} \sim$ Uniform(0,1). The variable $U_d$ is responsible for heterogeneity of outcomes for individuals with the same observable characteristics and treatment $d$. The treatment decision is represented by $D = \delta(\mathbf{Z}, V)$. Key assumptions are that $q(d, \mathbf{x}, u)$ is strictly increasing in $u$ and a condition called rank similarity. Rank similarity is defined by stating that for each value of $D$, say 0 and 1, $U_1$ is equal in distribution to $U_0$, conditional on $\mathbf{Z}$ and $V$.[19] In other words, rank similarity requires only rank invariance in expectations.

In order to estimate the model, we specify a linear quantile model

$$Y_d = q(d, \mathbf{x}, U_d) = \alpha(\tau)d + \mathbf{x}\beta(\tau), \tag{8}$$

[18] In the continuous treatment case $QTE_\tau(x) = \partial q(d, x, \tau)/\partial d$

[19] Rank similarity is a weaker condition than rank invariance which implies $U = U_1 = U_0$.

where the coefficients $\alpha(\tau)$ and $\beta(\tau)$ can be seen as describing the conditional quantile function of the potential outcome $Y_d$ conditional on $\mathbf{x}$. Since $Y_d$ is latent we cannot estimate Eq. (8) directly by standard quantile regression. However, under suitable regularity conditions Chernozhukov and Hansen (2005) show that it may be identified by the conditional moment condition

$$P[Y \leq q(D, \mathbf{X}, \tau|\mathbf{Z}] = P[Y < q(D, \mathbf{X}, \tau|\mathbf{Z}] = \tau \tag{9}$$

If we define $R = Y - q(D, \mathbf{X}, \tau)$ then Eq. (9) implies that the $\tau$-th quantile of $R|\mathbf{Z}$ is zero. Chernozhukov and Hansen (2005) show a simple way to use this moment condition to estimate Eq. (8). See their paper for more details. Their proposed estimator uses the linear projection of $D$ on $Z$ as instrument for $D$.

In order to apply this methodology to our data, we assume that we observe a cross section of contracts, some of which are signed in the pre-Bosman regime and the others in the Bosman regime. We assume that this is a random event, at least conditional on the observed covariates. It appears reasonable that this assumption is valid in the seasons just prior and after the regime change.

# 5 Results

In this section we discuss the estimation results. First, we present the results for the average causal effects. These are evaluated by some sensitivity checks in section 5.2. Finally, we present evidence for quantile causal effects.

## 5.1 Average causal effects

We first present the results based on OLS regressions; and then show how these results change when controlling for the endogeneity of the contract duration. The dependent variable is the performance index, and contract duration is measured by years as well as by a dummy variable of the contract is longer than 3 years. Using also a discrete version is motivated to mimic the classic treatment effect literature where treatments are usually binary. Control variables are the number of league games played as a measure of experience, a dummy for being in the national team, age and age squared, and indicators for position and nationality.

Table 3 displays the OLS regression results. The estimated effect of remaining contract duration is significantly positive, indicating that one additional contract year increases performance by 1.64 performance points. If this was indeed a causal effect teams would be able to increase players' performance by longer contracts. If we measure contract length by the dummy *contract longer than 3 years* the estimated effect is 1.8 performance points.

Table 3: OLS Regression

| | (1) | (2) |
|---|---|---|
| contract duration | 1.637** | |
| | (0.39) | |
| contract longer than 3 years | | 1.797** |
| | | (0.76) |
| contract renewal | 3.949** | 3.706** |
| | (0.92) | (0.93) |
| previous contract expired | 1.051 | -0.00614 |
| | (0.92) | (0.88) |
| league games | -0.527 | -0.479 |
| | (0.43) | (0.43) |
| plays for national team | 3.315** | 3.291** |
| | (0.74) | (0.75) |
| age | -1.231** | -0.830* |
| | (0.48) | (0.47) |
| age squared | 2.699** | 1.836** |
| | (0.91) | (0.88) |
| team budget | 2.852** | 2.952** |
| | (0.35) | (0.35) |
| Constant | 81.35** | 81.75** |
| | (6.55) | (6.66) |
| Observations | 621 | 621 |
| $R^2$ | 0.265 | 0.251 |

Standard errors in parentheses
Further controls: players position, nationality
Standard errors corrected for clustering
$^{*}p < 0.10$, $^{**}p < 0.05$

The following two tables present the results of several 2SLS regressions. The first column refers to the first stage regression of contract duration (Table 4) and the dummy *contract longer than 3 years* (Table 5), respectively. The second column displays the second stage using only the Bosman dummy as instrument. In the final column we add age and age squared to the instrument list. The age variables are completely insignificant in the second stage in column 2 of both tables. Hence, age seems to affect performance only via the contract duration which seems plausible as we control for experience measured by the number of league games.

Both when treating the contract length as a categorial and as a binary variable, it is obvious that the Bosman regime has a strong positive impact on contract length. It increases average contract duration by .37 years and increase the probability of a long-term contract by .25, respectively. The estimated causal effect of contract duration on performance in column 2 of Table 4 is negative but insignificant. If we use age and age squared as additional instruments the negative effect becomes significant at the 10% level. Both age variables are significant in the first stage. Using the Sargan overidentification test, we cannot reject the validity of these instruments. Hence, using the Bosman dummy and the age variables as instruments, there is evidence of a negative incentive effect indicating that increasing contract duration by one year will reduce expected performance by 2 points.

With respect to the other control variables, it is interesting to note that the impact of the expiration of the previous contract becomes significantly negative in column 3 (and again the point estimate in column 2 is very similar) indicating that, consistent with our theory, the incentive to renew contracts is indeed higher for players with strong ability. The effect of contract renewal is significantly positive. We interpret this variable as a measure of a good team-player match. The number of league games is insignificant throughout.

Table 5 shows the results when contract duration is measured by the dummy *contract longer than 3 years*. The results are very similar to those discussed above. Using only the Bosman dummy as instrument generates an insignificant estimate of the causal effect. The point estimate in column 3, obtained by using age and age squared as additional instruments, is significant on the 10% level and implies that having a long contract reduces performance by 4 points.

### 5.2 Quantile causal effects

In Table 6, we show the estimates of the standard quantile regressions and the instrumental quantile regressions for the variable contract duration.[20] We estimated the regressions at the .1, .2, ..., and .9 quantiles. The coefficients can be interpreted as the shift in the quantile of the

[20] A complete set of estimation results is available on request.

Table 4: 2SLS Regression

| | (1) | (2) | (3) |
|---|---|---|---|
| contract duration | | -2.15 | -1.64$^{*}$ |
| | | (2.56) | (0.87) |
| Bosman regime | 0.37$^{**}$ | | |
| | (0.093) | | |
| contract renewal | -0.16 | 3.33$^{**}$ | 3.49$^{**}$ |
| | (0.095) | (1.06) | (0.89) |
| previous contract expired | -1.14$^{**}$ | -3.33 | -2.69$^{**}$ |
| | (0.083) | (3.08) | (1.16) |
| league games | 0.038 | -0.43 | -0.40 |
| | (0.044) | (0.46) | (0.39) |
| plays for national team | 0.070 | 3.40$^{**}$ | 3.44$^{**}$ |
| | (0.078) | (0.79) | (0.75) |
| age | 0.41$^{**}$ | 0.32 | |
| | (0.047) | (1.15) | |
| age squared | -0.89$^{**}$ | -0.60 | |
| | (0.087) | (2.40) | |
| team budget | 0.10$^{**}$ | 3.28$^{**}$ | 3.21$^{**}$ |
| | (0.036) | (0.47) | (0.38) |
| Constant | -1.40$^{**}$ | 77.2$^{**}$ | 79.5$^{**}$ |
| | (0.68) | (7.50) | (3.50) |
| Observations | 621 | 621 | 621 |
| $R^2$ | 0.362 | 0.150 | 0.179 |

Standard errors in parentheses
Standard errors corrected for clustering
Further controls: players position, nationality
Column 1: First stage regression
Column 2: Second stage regression, only Bosman IV
Column 3: Second stage regression, Bosman and age as IV
p-value of Sargan overidentification test: .91
$^{*}p < 0.10$, $^{**}p < 0.05$

Table 5: 2SLS Regression

| | (1) | (2) | (3) |
|---|---|---|---|
| contract longer than 3 years | | -3.289 | -4.042$^{*}$ |
| | | (3.81) | (2.18) |
| Bosman regime | 0.245$^{**}$ | | |
| | (0.048) | | |
| contract renewal | -0.00924 | 3.631$^{**}$ | 3.642$^{**}$ |
| | (0.049) | (0.95) | (0.91) |
| previous contract expired | -0.457$^{**}$ | -2.376 | -2.711$^{**}$ |
| | (0.042) | (1.96) | (1.18) |
| league games | 0.00458 | -0.497 | -0.412 |
| | (0.023) | (0.44) | (0.40) |
| plays for national team | 0.0643 | 3.460$^{**}$ | 3.566$^{**}$ |
| | (0.040) | (0.78) | (0.75) |
| age | 0.153$^{**}$ | -0.0709 | |
| | (0.024) | (0.74) | |
| age squared | -0.328$^{**}$ | 0.243 | |
| | (0.045) | (1.48) | |
| team budget | 0.0401$^{**}$ | 3.192$^{**}$ | 3.202$^{**}$ |
| | (0.018) | (0.40) | (0.38) |
| Constant | -1.424$^{**}$ | 75.55$^{**}$ | 75.69$^{**}$ |
| | (0.35) | (8.21) | (1.92) |
| Observations | 621 | 621 | 621 |
| $R^2$ | 0.265 | 0.195 | 0.177 |

Standard errors in parentheses
Standard errors corrected for clustering
Further controls: players position, nationality
Column 1: First stage regression, dependent variable contract longer than 3 years
Column 2: Second stage regression, only Bosman IV
Column 3: Second stage regression, Bosman and age as IV
p-value of Sargan overidentification test: .90
$^{*}p < 0.10$, $^{**}p < 0.05$

conditional distribution of performance induced by another year of contract duration.

The first rows displays the results without correcting for the endogeneity of contract duration. As in section 5.1, contract duration has a positive effect on performance except at the two lowest quantiles. At the median, the estimated effect is 1.81 which is similar to the OLS effect.

Table 6: Quantile Regression

| | Quantiles | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | .1 | .2 | .3 | .4 | .5 | .6 | .7 | .8 | .9 |
| | Quantile Regression | | | | | | | | |
| contract duration | 0.171 | 0.910 | 1.119** | 1.486** | 1.810** | 1.683** | 1.877** | 1.779** | 1.154** |
| | (0.82) | (0.59) | (0.54) | (0.45) | (0.52) | (0.56) | (0.36) | (0.49) | (0.34) |
| | Instrumental Quantile Regression | | | | | | | | |
| contract duration | -3.078** | -2.244** | -0.998 | -1.262 | -0.482 | -0.034 | 0.928 | -0.998 | -1.157 |
| | (1.26) | (0.97) | (1.06) | (1.09) | (1.13) | (1.19) | (1.19) | (1.36) | (1.34) |
| Observations | 621 | | | | | | | | |

Standard errors in parentheses
Further controls as in Tables 4 and 5
$^{*}p < 0.10$, $^{**}p < 0.05$

When we instrument contract duration by the Bosman indicator and the age variables, the results change significantly. We find a significant and strong negative effect of contract duration at the lowest two quantiles and no significant effects in the upper part of the conditional distribution of performance. The negative effects at the low quantiles is strong enough to obtain the (weakly) significant negative average causal effect in section 5.1.

This finding indicates that another year of contract duration increases the probability of a poor performance. The expected conditional 0.1-quantile of performance for a player with average characteristics and a 3-year contract is 61.5. In other words, there is a 10% chance of a performance below 61.5 with a three-year contract. With a four-year contract, the predicted 0.1-quantile is 58.4 and the predicted 0.2-quantile is 63.8. Hence, with a four-year contract, there is a 20% chance of a performance below 63.8. This is what we mean by saying that another contract year increases the probability of a bad performance: in this example, the probability of an performance worse than 63.8 is increased from approximately 15% to 20% if the contract length increases from 3 to 4 years.[21] However, the probability of a top performance (e.g. the .8-quantile) is not affected by the contract length. Put differently, our results suggest that the effect of long-term contracts is that players have small incentives to put in extra effort if they

[21] Approximation by linear extrapolation

have a bad day or a stretch of bad games.

## 6 Conclusion

In this paper, we have exploited the natural experiment caused by the Bosman judgement to identify the incentive effect caused by the insurance effect of long-term contracts. We show that the results from a simple OLS regression are misleading even though we have apparently good proxies for player ability. We find a negative effect of longer contracts on average performance. In addition, we are able to identify that longer contracts influence the distribution of performance asymmetrically in the sense that they increase the probability of poor performances but do not reduce the probabilities of good performances.

The negative effect of long-term contracts on performance is remarkable given the nature of the labor market we analyze. As mentioned above, performance is observed every week, and players are in fact graded in different soccer magazines and in data bases available to club managers. Therefore, it seems unlikely that players shirk deliberately in the ninety minutes a game lasts, but rather that the insurance effect of long-term contracts leads to lower effort outside the pitch, for instance concerning the general life style, the practice habits, and the motivational focus on the job. As mentioned above, the estimates of the quantile causal effects suggest that long contracts do not generate incentives for extra effort if things on the pitch are not going well. Our findings suggest that long-term contracts are likely to have considerable negative impacts on effort in fields where individual performance is more difficult to observe and to measure than in professional sports.

Given the already mentiones fact that player performance is observable on the pitch, one might wonder why the moral hazard problem associated with long-term contracts is not solved by incentive pay. In reality, however, less than 10% of the average player's annual salary is performance related (see Ziebs (2002) ), and practitioners argue that there are good reasons for this. Although performance is observable, it is hardly contractible due to the complexity of tasks a player has to cope with, and rewarding the number of goals or assists, for instance, would lead to distortions well-known from multi task principal agent problems. Moreover, paying high amounts for getting on the team would most likely increase the injury risk and may even lead to sabotage when players compete for the same position. Last but not least, it can be argued that high incentives diminish the team spirit. We do not comment further on these reasons as all that counts for our purposes is that incentive pay is in fact so low that it can hardly solve the moral hazard problem.

# 7 Appendix

## 7.1 Descriptive statistics

Table 7 gives the descriptive statistics of the variables used in the empirical analysis.

Table 7: Descriptive Statistics

| | mean | std. dev. |
|---|---|---|
| performance index | 71.87 | 9.27 |
| contract duration | 3.30 | 1.03 |
| log(contract value) | 14.84 | 0.88 |
| age | 27.87 | 3.57 |
| plays for national team | 0.33 | |
| previous contract expired | 0.24 | |
| contract renewal | 0.18 | |
| team budget | 41.17 | 12.83 |
| goalkeeper | 0.08 | |
| defense | 0.25 | |
| midfield | 0.43 | |
| german | 0.65 | |
| eu_country | 0.14 | |
| eastern_europe | 0.11 | |
| Observations | 621 | |

## 7.2 Performance index

The performance index has been constructed as follows (Ziebs, 2002, p. 100)

Table 8: Performance Index

| Performance Independent of Players Position | Points | Performance dependent of Players Position | Points |
|---|---|---|---|
| | | Goalkeeper | |
| Team Win | 5 | Goal Against Team | -20 |
| Team Loss | -5 | No Goal Allowed | 10 |
| Goal scored | 20 | Ordinary Save | 10 |
| Goal against own team | -10 | Difficult Save | 20 |
| Penalty Kick Successful | 10 | Defender | |
| Penalty Kick Missed | -10 | Tackle Won | 5 |
| Goal Assist | 15 | Tackle Lost | -5 |
| Responsible for Goal Against Team | -20 | Long Pass | 1 |
| Responsible for Penalty Kick | -10 | Shot on Goal Prepared | 2 |
| Goal Missed | -5 | No Goal Allowed | 10 |
| Shot on Goal (-16m) | 4 | 1-2 Goals Against | -5 |
| Shot on Goal (16m+) | 1 | 3-4 Goals Against | -10 |
| Yellow Card | -3 | 5-6 Goals Against | -20 |
| Yellow/Red Card | -15 | 7+ Goals Against | -30 |
| Red Card | -20 | Midfielder | |
| | | Tackle Won | 3 |
| | | Tackle Lost | -2 |
| | | Long Pass | 1 |
| | | Shot on Goal Prepared | 5 |
| | | Forward | |
| | | Tackle Won | 1 |
| | | Shot on Goal Prepared | 2 |

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