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**The Effect of Personal Contracts in Public
Administration in Israel on Length of Service**

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**Any views expressed in the Discussion Paper Series are those of the
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Abstract

Personal employment contracts for employees in professional positions in public administration in Israel create a more flexible employment format than that in collective agreements, with a higher level of pay than that in the case of collective wage agreements. Thus, personal contracts, whether used to keep excellent employees or used to afford greater managerial flexibility, are likely to affect the length of service of employees with such contracts. This study examines the effect of personal contracts on the length of service, and the actual difference in wage levels between those with personal contracts as opposed to those covered by collective agreements. The main finding is that the probability of leaving the job is higher among those who switched to personal contracts, but this is not a uniform result: in the first two years after the move to a personal contract the probability of leaving is lower than that of employees under collective agreements, but thereafter the situation is reversed, and those with personal contracts are more likely to leave.

השפעת החוזים האישיים במינהל הציבורי בישראל על משך השירות

נועם מיכלסון

תקציר

החוזים האישיים לעובדים במקצועות אקדמיים במינהל הציבורי יוצרים מתכונת העסקה גמישה, יחסית להסכם קיבוצי, בתמורה לשכר גבוה מזה המשולם על פי הסכם קיבוצי. לכלי זה, בין אם ככלי המשמש לשימור עובדים מצטיינים ובין אם ככלי המאפשר גמישות ניהולית גדולה יותר, עשויה להיות השפעה על משך השירות של העובדים במתכונת העסקה זו. בעבודה זו נבחנת ההשפעה של החוזים האישיים על משך השירות, וכן נבחנות המשמעויות הכמותיות של החוזים האישיים מבחינת גובה השכר. המסקנה המרכזית היא שההסתברות-לעזיבה של עובדים שעברו לחוזה אישי הייתה גבוהה יותר, אך נראה כי תוצאה זו איננה אחידה: בשנתיים הראשונות לאחר המעבר ההסתברות נמוכה יותר, אולם בשנים הבאות ההשפעה מתהפכת – עובדים שעברו לחוזה אישי נוטים לעזוב יותר מאלה שלא עברו.

Introduction

At the end of the 1980s, the Sussman Committee examined the wage system in public service with the aim of simplifying and streamlining it. A report submitted in February 1989 comprehensively addressed the question of remuneration for outstanding employees and strengthening the connection between output and compensation. One of the main defects discovered was disassociation of the wage system in the public service from conditions in the Israeli labor market, a situation that hampers the recruitment of suitable employees, and obviously also retaining them in public service for any extended period. The Committee wrote, “The public sector requires some degree of flexibility in raising the salary of these and other employees who contribute to output, quality of service, and increased efficiency” (Sussman Committee, 1989, p. 8). The Committee’s recommendations were not implemented.

There are very few possibilities for decentralizing wages and for differential remuneration in the framework of a collective agreement. Assuming that complete elimination of collective agreements in the public service is impractical, individual remuneration is possible outside the collective framework, such as the personal contracts designed for employees in the academic professions.¹ To a certain extent, this tool provides the very flexibility whose lack was cited by the Sussman Committee, and which is likely to make it possible to recruit appropriate employees and - no less important - to retain them in the system in the long term. At the same time, a personal contract framework provides a non-binding (compared with a collective agreement) employment format, which could affect the duration of an employee’s service in this format.

An examination of the duration of service is likely to prove useful from two different perspectives: from the perspective of keeping employees in the system, it could give an idea of the usefulness of this tool; from the perspective of a non-binding employment format, it could provide a manager with an idea of the employee’s service outlook on which to base decisions about investing in human capital, long-term planning in his unit, etc.

Since 1997, the number of personal contracts for employees employed in public service in academic professions has continuously risen, but the question of their effect on the duration of service has not been examined to date. This study is designed to describe and characterize for the first time the personal contracts in their quantitative aspect, and to analyze their connection to the duration of service.

The study is organized as follows: Section 1 defines what personal contracts are and describes how they were created, followed by a review of the literature; Section 2 provides a quantitative review of personal contracts; following a short theoretical introduction, Section 3 empirically assesses the question posed in the study; and Section 4 discusses and summarizes the results.

¹ In contrast to employees in senior positions or positions requiring a degree of personal confidence, as will be emphasized below.

1) Personal Contracts – Background and a Review of the Literature

1.1) Personal Contracts in Public Service in Israel

Since its beginning, public service in Israel has operated in a fairly rigid employment and wage framework. One of its chief features is less wage dispersion than that in the business sector, because wages in public service are determined through collective bargaining, and collective wage agreements are signed at both the national level and the level of ministries or the various employment scales. In a wage system of this type, there is little flexibility in determining the salary of a new employee and changing the salary of an existing one. An employee's salary is derived directly from his occupation (his employment scale), seniority, education, the salary grade set for him, and special wage increments derived from the characteristics of his employment scale, job, the ministry that employs him, etc. The maneuvering room available to reward the employee for unobserved characteristics, such as ability, is extremely small, since such characteristics are mostly translated into salary through direct employer-employee negotiations, a channel that is almost non-existent in public service.²

Compensation mechanisms exist in public service for alternative remuneration that complements salary, principally employment security. This mechanism, which reduces uncertainty, has been significantly weakened in the business sector in recent years with the weakening of workers committees and the small number of collective agreements signed (Kristal and Cohen (1996), Sussman and Zakai (2007)). Nevertheless, Mazar (2010) has shown that the employees switching from the public to the business sector are “good” employees, while those moving in the opposite direction are “poorer.”³ Given this background, for many years public service has faced a major challenge in human resources that has grown in recent decades:⁴ how to offer high enough remuneration to attract “good” employees, and how to retain them in the system in the long term. The need to find a solution to this challenge has given birth to personal contracts in public service. The personal contract format existed before this, but it was designed for senior employees and employees in positions requiring a degree of personal confidence in the offices of ministers and directors general.

The introduction of personal contracts designed for employees in academic professions began in 1997.⁵ In that year, the Ministry of Finance and the Civil Service Commission established salary tables for these professions, including various salary grades, similar to the salary grades prevalent in the ordinary employment scales in public service. The

² While the salary grade is a tool for employee promotion and is awarded at the discretion of the managers, Mazar (2007) showed that an employee had a better chance of salary promotion in a given year mainly if he had not been promoted during the preceding year.

³ In a similar context, Fama (1980) analyzed the wage structure of CEOs, and asserted that when the salary system was insensitive to a firm's performance, there was a higher probability that good executives would leave. Bishop (1990) found that in companies with unionized workers, there was a closer connection between productivity and leaving voluntarily: better employees were inclined to leave workplaces lacking differential remuneration.

⁴ With the rise in the return on ability in Israel (Dahan et al. (2001), Yotav-Solberg (2002)).

⁵ No organized paper was found that summarized the particulars of the personal contract in public service. Information about its features in this section was obtained through the generosity of Henia Markovitz, head of the Planning and Control Section of the Civil Service Commission, who provided plentiful assistance, for which I greatly thank her.

tables were derived to some degree from the accepted employment scale for the employees in the ordinary wage system: for example, when the salary table for a personal contract for lawyers was devised, the salary for the lawyers' employment scale was taken into account. The salary tables were updated through a linkage mechanism, usually linkage to the cost of living, in contrast with the salary tables in public service, which are revised through collective wage agreements. The expenses reimbursement and overtime items remained differential. Starting in 2002, personal contracts were also signed with non-academic employees (mostly administrative), but the quality of such contracts and other information about them are unavailable.

There are two extremely significant differences between a personal contract and an employment scale framework in a collective agreement (hereafter – “employment scale-salary grade”) in salary and employment terms: in a personal contract, salary is 25-30 percent higher (officially) than in a corresponding job according to employment scale-salary grade, and the entire salary is taken into account for purposes of the provision for pension savings, unlike employment scale-salary grade, in which only a certain percentage is counted for this purpose (an average of 70 percent, a rate that decreases as the gross salary rises). On the other hand, there are certain wage increments granted only to employment scale-salary grade employees – a jubilee grant (for 25-30 years seniority), an increment for seniority in a salary grade, etc. Furthermore, in contrast to employment scale-salary grade employees, the seniority of personal contract employees does not affect their salaries.

The second significant difference concerns employment terms. One of the significant premiums of working in public service is job security, as embodied in the institution of tenure. The contract of a tenured employee has no termination date, and in most places, dismissal of such an employee requires the consent of the worker's union, which is obviously in no hurry to give such consent. The laying off process is lengthy and difficult, and the managerial flexibility involving the option of an employee's dismissal is practically non-existent. In contrast, a personal contract employee is not protected by the institution of tenure. His contract is for a limited time (usually one year), which is extended for an additional period when it expires – provided, of course, that his employer wishes to retain his services. Such an employment structure permits, de jure, the laying off of an employee according to management's needs. While the de facto process of laying off a personal contract employee is not as simple as it appears, it is still a more real and simpler possibility than laying off employment scale-salary grade employees. Another point that will become important later is a restriction on the employees entitled to a budgetary pension: if an employee has less than 10 years of seniority in his workplace, switching to a personal contract means losing the rights that he had accumulated to that point.

The salary promotion mechanisms for personal contract and employment scale-salary grade employees have both equal and differing aspects. There is a salary grade scale in both cases, in which an employee's progress depends on his performance, the length of time since his most recent promotion, etc., and there are also “systemic” wage adjustments in both cases. There are also differences, however: the salary tables of employment scale-salary grade employees are determined and change periodically through collective agreements at the national or the employment scale level (separate

agreements for doctors, engineers, etc.), while the salary tables of personal contract employees are revised periodically according to a linkage mechanism set in 1997 when these tables were created. Another difference lies in the influence of seniority on salary (seniority influences the wages of employment scale-salary grade employees and does not influence the salary of personal contract employees).

The Civil Service Commission does not restrict or dictate terms for a personal contract; a ministry's decisions are autonomous, subject to its budget. An employee receives an offer to switch to a personal contract if his manager decides to make such an offer – usually, according to the official rationale behind this tool, in order to give him an incentive to stay in his job in order to avoid losing him to another workplace, with an emphasis on the business sector.

The personal contract can be viewed from a different perspective, which is not the official one presented above. It is possible that certain managers prefer to have the managerial flexibility not to renew a contract, even at the price of paying a higher salary. In this light, an offer of a personal contract is not necessarily related to the employee's quality; it is a decision by the manager motivated by other considerations. Either way, the different employment format and the salary are likely to affect the duration of service, a question worthy of examination. The difference between these two perspectives – the official one and the unofficial one – lies in an assumption about the quality of the person receiving a personal contract (an outstanding employee, or one who is not necessarily outstanding) and in the normative interpretation of the profile of those who leave: if we believe that this tool is designed for outstanding employees and find that it does not have a positive effect on the duration of service, we can say that the tool is not fulfilling its purpose. If, however, we make no assumptions about the identity of the employees who switch to personal contracts and the purpose for which it is used with respect to the duration of service, we can only indicate a connection between a switch to a personal contract and the duration of service. In both cases, the conclusion we will draw is likely to be important for employers planning investment in the human resources at their disposal (courses, training, etc.).

While writing this paper, I chose to stick to the official viewpoint, but in places where conclusions were drawn from the results, I also stated the conclusions likely to be drawn from the other viewpoint.

1.2) A Review of the Literature

Literature about personal contracts in public service in Israel, particularly empirical literature, is unavailable due to a lack of access to data, among other reasons. It is possible to learn a little from the annual report by the Ministry of Finance Director of Wages,⁶ which lists various characteristics of the types of personal contracts in public service. The classification of the personal contracts in the current article relies mainly on this report.

Sussman (1995) addressed the question of personal contracts in the Israeli economy, not necessarily in public service, from the perspective of the weakening of the Histadrut Labor Federation. He grouped personal contracts into three categories: 1) Personal

⁶ For example, the 2008 report: <http://hsgs.mof.gov.il/Documents/2008-3.pdf>

contracts in workplaces in which a majority of the workers are employed under a collective agreement, and a minority under personal contracts. Personal contracts in such places are mainly with senior employees and temporary or unskilled workers; 2) Workplaces in which all the workers are employed under personal contracts, but the workers committee is a partner in the wage setting process; 3) Workplaces in which all the workers are employed under personal contracts and there is no workers committee. The personal contracts in public service, as described above, are between the first and second categories – they are not designed solely for senior employees, but not all the workers are employed under personal contracts. According to Sussman, the main difference between personal contracts and a collective agreement is the tradeoff between a significant rise in salary and the conceding of certain social rights, first and foremost job tenure. Sussman asserts that the increase in salary is a short-term consideration, because in the long term, a personal contract is no guarantee of a higher salary. Kristal and Cohen (2007) cite the process of transition to personal contracts in the Israeli economy as part of a broader process – the decline of collective agreements – and find that the process has led to greater inequality in salary.

A variegated literature exists in the UK concerning the privatization process in the local economy. One of the prominent features of this process is a transition to payment according to performance in the framework of a personal contract and a reduced role of collective negotiations in determining wages. As in Israel, these contracts featured higher and more differential pay, together with a decline in job security (Metcalf (1988), Ferner (1990), Colling and Ferner (1992), Parker (2005)). Brown et al. (1998) surveyed 32 UK companies, some of which switched to personal contracts and some of which did not, in order to understand all aspects of the personal contracts phenomenon: the significance of the transition to personal contracts, the motives for it, the characteristics common to the companies making the transition, etc. They distinguish between two processes in the transition to individual contracts (their definition of personal contracts): procedural individualization, in which the collective mechanisms for determining employment terms are dismantled, and substantive individualization, in which the employment terms are determined differentially for each employee. They claim that there is no necessary connection between the two processes, since the first process alters the way employment terms are set, while the second process determines the employment terms themselves. In their opinion, one of the key advantages of a switch to personal contracts is that each contract is tailored to an employee according to his abilities, preferences, etc. At the same time, each such adaptation incurs transaction costs, since personal negotiations with each employee are required in order to determine his initial salary and for a raise. A collective agreement eliminates these costs, but does not ensure that the proposed collective wage is the most efficient for each employee. It is therefore possible that a collective agreement would be preferable when a group of employees is very uniform, or at least when large groups of employees can be distinguished according to the type of job, education, etc. Companies that switched to personal contracts indeed reported that their main reason for switching was the need to diversify salary, particularly the wish to pay lower wages to unskilled workers and higher wages to skilled ones.

Tuckman and Finnerty (1998) found that personal contracts in the UK were not completely individual in the sense of a special contract for each employee; in practice, groups of personal contracts were formed, and a personal contract was tailored to each

employee from the group suitable to him, according to his various characteristics. They also asserted that a considerable number of workplaces remained that fitted into Sussman's (1995) first category, meaning workplaces in personal contracts coexisted with collective agreements, or at least into his second category. In places in which there were both personal contracts and collective agreements, employees working under personal contracts reported greater satisfaction than that reported by personal contract employees in workplaces without collective agreements – probably because the first group had a better opportunity to compare their wages with the alternative. From a structural perspective, this is also the case in Israel – groups of personal contracts next to collective wage agreements.

Welch and Leighton (1996) analyzed surveys of the personal contracts signed in the 1990s. They found that most of the transition to personal contracts was in the business sector, while the transition in public service was mostly in health services and local administration, particularly among managers. In contrast to Tuckman and Finnerty (1998), who found that personal contract employees felt they had more control over their salary, Welch and Leighton found that in most cases, the employers determined the salary, and the employees had little to do with it.

The question asked in this study has not been empirically tested in other places. Most of the studies dealing in the connection between the employment structure and the duration of service discuss the effect of temporary contracts on the future duration of service after an employee has been accepted as a regular employee (for example, see Hagen (2003), Boockmann and Hagen (2005), Larsson et al. (2008), where there are also references to additional literature on the subject). On the other hand, there is extensive literature analyzing the effect of promotion and/or a pay rise on many result variables, including the duration of service. Personal contracts do not reflect only promotion or a pay rise in the ordinary sense, since most promotions do not require a decision to give up something, as does the decision to switch to a personal contract (which entails a loss of job security). Nevertheless, it is worth examining personal contracts in the framework of an analysis of promotions – and in particular, the role of promotions in an employee's decision to leave his workplace.

The term "promotion" has been interpreted in different ways, and the accepted definition in the theoretical literature is a change in an employee's location in the hierarchy, accompanied by a change in his production technology (Bernhardt (1995), Gibbons and Waldman (1999)). There are also different definitions of hierarchy, as can be seen in Prendergast (1993) and Manove (1997). Empirical studies on this subject used different definitions for the term "promotion," and Pregamit and Veum (1999) proved that the empirical results were sensitive to the definition used. In their article, they conducted a panel analysis based on data from the National Longitudinal Survey of Youth, and concluded that when an employee reported that he was promoted, he was referring to one of eight kinds of promotion (for example, promotion in status only, promotion accompanied by a move to another unit, to a more senior position, etc.). The largest group of respondents who reported that they had been promoted — 30 percent — changed nothing in their job or status; they merely received a pay raise.⁷ Therefore, even

⁷ On the other hand, 90 percent of those who reported that they had been promoted also won a pay rise.

though it is unclear whether the job of an employee who switched to a personal contract changed with the switch, it still can be stated that the (significant) rise in salary indicates a kind of promotion.

Does promotion influence an individual's decision to leave? The evidence, both theoretical and empirical, is not clear cut. Theoretically – the models of Lazear and Rosen (1981, 1990) and Sichermand and Galor (1990) predict that promotion will retain an employee longer in his position, while Waldman (1984) and Benhardt and Scoones (1993) reached the opposite conclusion. Empirically, Pregamit and Veum (1999) found that promotion was unrelated to leaving. Dias da Silva and Van der Klaauw (2006) used employer-employee files from Portugal to examine the significance of promotion and its effects. The term “promotion” was interpreted in two different ways: 1) A change in position; 2) an advance in salary. They found that promotion by either definition had a positive effect on the probability of leaving. Specifically for public service, the findings of Howes (2002) can be cited; he found that the most significant salary increases (doubling pay within four years) for government service employees maintaining homes of needy people led to an extremely significant drop in the rate of those leaving.⁸

In summary, the literature indicates that personal contracts in Israel are similar to those signed in the UK as part of the privatization process and the desire to render the wage system more flexible, but no empirical studies of their effect on the duration of a worker's employment were found. Personal contracts can be seen as a kind of promotion (in salary, at least), but the evidence of the effect of promotion on the duration of employment is not conclusive.

2) A Quantitative Review of the Personal Contracts in Public Administration

2.1) A Description of the Database

The data on which this study is based are taken from a special Bank of Israel database of the wages of employees in public administration, which includes mainly the various government ministries and other public entities. The database includes all the salary and human resources data (age, gender, position, etc.) for each employee. The number of years of education is unknown, but the level of education, in particular whether or not an academic employee is involved, is derived from his position (i.e., his employment scale). This study uses only figures for December of each year.⁹ The number of employees included in each file naturally changes each year, varying from 60,000 in December 1990 to 73,000 in December 2009. The special database makes it possible to examine for the first time the growing phenomenon (as will be proved below) of personal contracts, and their effect on the duration of service.

One important figure that is missing is the reason why an employee left: there is no way to distinguish an employee who left voluntarily from one who was laid off. The lack of this information ostensibly makes it difficult to examine the study question, but several

⁸ Concerning the effect of working in public service on the duration of work, Swindinsky (1992) found that in the US and Canada, the existence of unions in the workplace had a positive effect on the decision to remain, but no such connection was found in the UK.

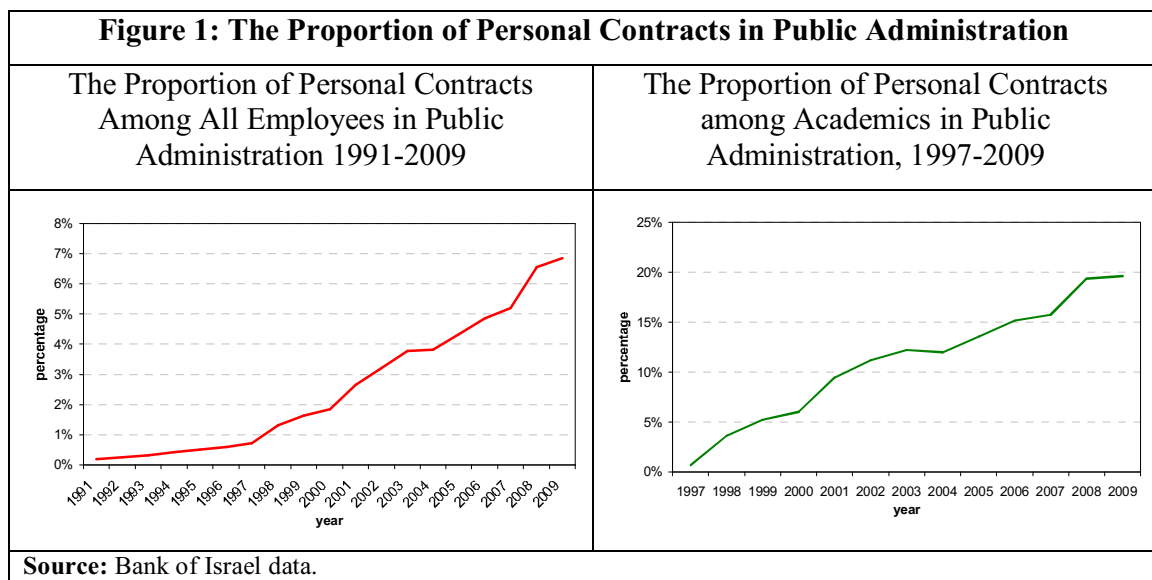
⁹ December figures are used in other studies based on this database, which were found to be sufficiently representative, because no special increments or reimbursements of expenses are paid for this month.

qualifications greatly reduce the magnitude of this problem. First, as explained above, laying off an employee in public administration is far from a simple matter. Second, it can be assumed that personal contract employees were those whom the system regarded as worth retaining (that was why a personal contract was signed with them), and the likelihood that they would be fired was therefore lower than average. Third, Pregamit and Veum (1999) found that there was no difference in the effect of promotion on leaving between employees who were laid off and those who left voluntarily.

For the purpose of this study, I divided the population into four groups, according to the character of their employment scales. The first group contains administrative employees in unskilled jobs; the second group consists of technicians, most of whom had a semi-academic education (diploma studies and so forth); the third group contains academic employees, most of whom have defined professions (lawyer, economist, etc.); and the fourth group consists of senior level employees. Most of our interest is in the third group, since the official reason why personal contracts were created is based on this group. When salary is discussed, it refers to gross salary, excluding annual increments (clothing, convalescence pay, etc.) and special personal increments (compensation and the like), unless otherwise stated.

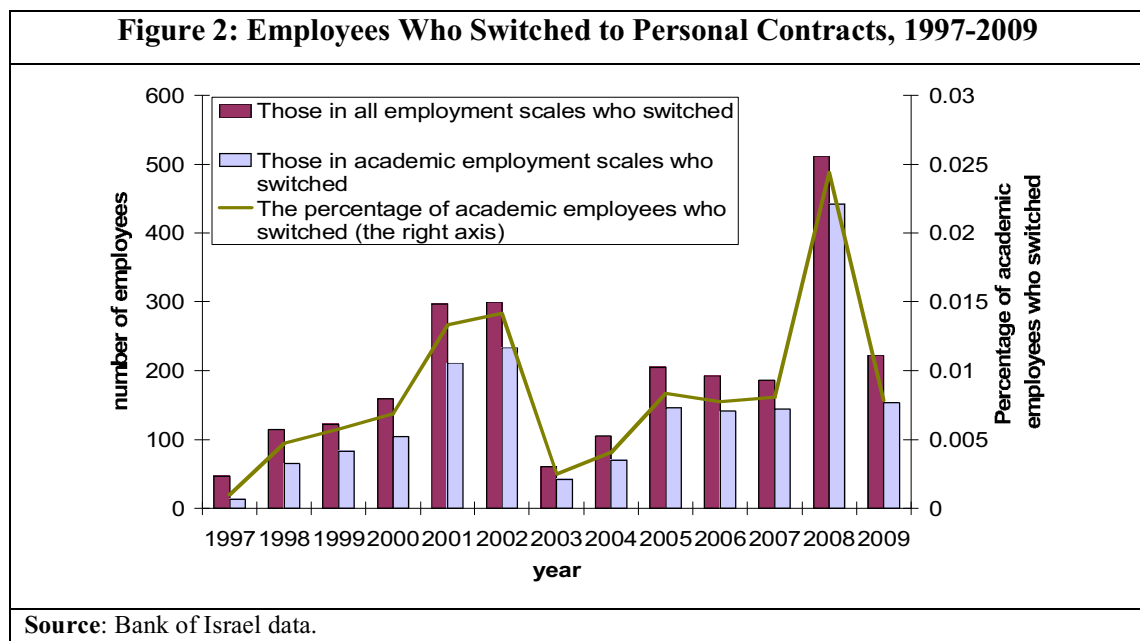
2.2) Descriptive Statistics

The development of personal contracts can be seen clearly in the left part of Figure 1. As noted above, up until 1997, the vast majority of employees under personal contracts was at the senior level (e.g., directors) or in positions requiring a special degree of trust (e.g., personal advisors), and constituted no more than 1 percent of all employees. Since that time, the proportion of personal contracts has grown to almost 7 percent.¹⁰



¹⁰ The signing of personal contracts with unskilled workers began in 2002.

The proportion of personal contracts among academic employees is displayed in the right part of Figure 1, and its current importance is obvious: about 20 percent of all academics in public administration now work under personal contracts. At the same time, it would not be correct to say that all 20 percent were offered a personal contract in order to retain them in the system, since this population includes both diligent employees and newly recruited employees, and the 20 percent also includes employees who **entered** public service under a personal contract. Figure 2 displays the number of employees switching to personal contract among all employees and among academics only; an employee is considered to have switched if he was in public administration in both year $t-1$ and year t : in an ordinary employment scale in year $t-1$ and under a personal contract in year t .



As can be seen, in each year, most of those switching to a personal contract were in the group of academics; for the entire period, over 1,800 of the 2,500 employees who switched were in this group.

2.3) The Value of Personal Contracts

As mentioned, the salary of an employee under a personal contract is officially about 25-30 percent higher than that of a corresponding employee under a collective agreement (i.e., in an ordinary employment scale). I will now test the robustness of this value using salary regressions. I will also test an additional monetary significance of personal contracts – the speed of salary promotion, compared with that of employment scale-salary grade employees. It should be mentioned that all the following analyses apply to both men and women. While it is proper to distinguish between men and women in any economic employment analysis, the shortage of observations for testing the study question in Section 3 does not allow this to be done. I therefore chose to focus here on the average magnitude of the personal contract's effect, as was also done in the other analyses.

For the purpose of estimating the value of a personal contract in salary terms, the following model was estimated: the dependent variable is the logarithm of the gross real salary (in December 2007 prices), and the explanatory variables are the employee's gender, age, age squared, and seniority. Where seniority is concerned, note that because the number of years of seniority has no official effect on the salary of an employee under a personal contract, there is no data for these employees. I filled out this information artificially by adding a number of seniority years for an employee according to the number of years of seniority years listed for him in previous years (this information is available only if he was an employment scale-salary grade employee in at least one previous year), or by estimating the years of seniority with the help of the employee's age and the actual number of years that he worked.¹¹

An employee's salary grade is one component of his salary. Using the salary grade in the regression is difficult, however, because each employment scale (including personal contracts) has its own salary grade scale, with a different numbers of grades and numerical designations. It was therefore necessary to create a variable defined as the "salary grade order": each salary grade in each employment scale is assigned its relative position in the salary grade scale of that employment scale. For example, in an employment scale with 20 salary grades, an employee with a salary grade of 4 will receive the value 20 ($100 * 4/20$), the same as an employee with a salary grade of 3 in an employment scale having 15 salary grades. Dummy variables were also added for each group of employment scales and for each year, and, of course, a dummy variable for personal contracts – what we are most interested in. The population is all the employees in 1997-2009. At the same time, in columns (2) and (3), I included an interaction between female gender and personal contract in order to test for a possible difference in return between men and women. The results displayed in Table 1 are for different specifications of the model.

Column (1) displays the basic regression results. Column (2) adds interactions in order to test a different return for the salary grade order and seniority among personal contract employees than among employees without personal contracts. The dummy variable for personal contracts increases considerably when interactions are added – from 7.8 percent in Column (1) to 10 percent in Column (2). The interaction between women and personal contract indicates that women profit more than men from a personal contract, but the difference between them in favor of men still exists if the negative effect of the variable "women" by itself is added. The negative sign of the salary grade order and seniority interactions indicate that two of the key elements in determining an employee's salary are worth less for personal contract employees.

Note that although seniority has less effect among personal contract employees, the return on seniority for these employees is still positive at 0.7 percent.¹² This is surprising, because according to the above, seniority does not affect the salary of personal contract employees. One possible explanation is that the estimate of seniority is biased, and the years of seniority themselves add nothing directly to a personal contract employee's

¹¹ That is, multiplying the coefficients from a regression of years of seniority on the age and actual work years for employees with proper seniority data. It should be noted that the R-squared value for this regression is very high.

¹² An F test of the hypothesis "seniority + seniority X personal contract = 0" rejects the hypothesis.

salary, but there is a connection between years of seniority and differential salary elements, such as the group of travel allowance, vehicle expenses, overtime, reimbursement for telephone, etc.

Table 1 – Results of the Salary Regressions

	(1)	(2)	(3)
	All Employees	All Employees	Academics Only
Personal contract	0.0785*** [0.0061]	0.1010*** [0.0096]	0.3288*** [0.0134]
Women	-0.2254*** [0.0024]	-0.2304*** [0.0024]	-0.1576*** [0.0040]
Age	0.0333*** [0.0006]	0.0336*** [0.0006]	-0.0005 [0.0014]
Age squared	-0.0003*** [0.0000]	-0.0003*** [0.0000]	-0.0000** [0.0000]
Seniority	0.0126*** [0.0002]	0.0127*** [0.0002]	0.0080*** [0.0004]
Salary grade order	0.0041*** [0.0001]	0.0042*** [0.0001]	0.0138*** [0.0001]
Women X personal contract		0.1447*** [0.0095]	0.1118*** [0.0107]
Seniority X personal contract		-0.0058*** [0.0006]	0.0059*** [0.0014]
Salary grade order X personal contract		-0.0010*** [0.0002]	-0.0081*** [0.0003]
Constant	8.2884*** [0.0132]	8.2835*** [0.0133]	8.6052*** [0.0300]
Control for years	Yes	Yes	Yes
Control for the employment scale groups	Yes	Yes	
Number of observations	935,150	935,150	180,577
R-squared	0.491	0.492	0.544

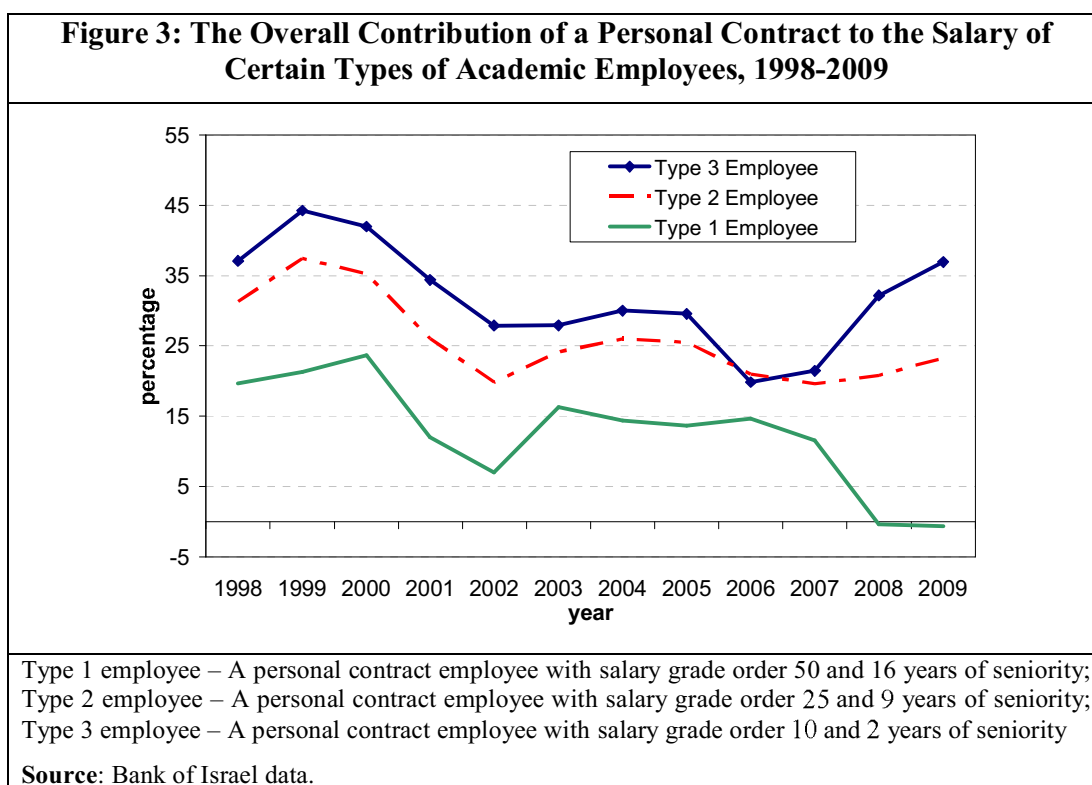
Source: Bank of Israel data. The standard error is in parentheses.

* denotes a p-value less than 0.1; ** denotes a p-value less than 0.05; *** denotes a p-value less than 0.01.

The regression estimates displayed in Column (3) are similar to those in Column (2), but the tested population consists only of the group of academic employment scales, the heart of this study. The results indicate significant differences in the factors that influence salary and the value of a personal contract. First of all, the value of a personal contract is estimated at 33 percent. At the same time, the contribution of the salary grade order is more than triple that for all the employment scale groups (0.0138, compared with 0.042), while at the same time the negative contribution of the salary grade order is also greater to an even larger extent (-0.0081, compared with -0.0010), a fact that offsets the size of the personal contract's contribution. It further emerges that the return on seniority is greater among personal contract employees. On the basis of the above explanation, it can be hypothesized that the differential elements affected by seniority are more common among academic employees, and the return is therefore greater than among employment scale-salary grade employees. This hypothesis requires further research.

As stated, the population examined in the three regressions in Table 1 consists of all the employees, or academics only, in 1997-2009. It is interesting, however, to check whether there were differences in the value of a personal contract between different years. For this

purpose, the model in Column (3) of Table 1¹³ was tested for each year in 1998-2009¹⁴, and for academic employees only. With the help of the relevant estimation results displayed in Table 1 – for the personal contract dummy variable, seniority in a personal contract, and salary grade order in a personal contract – the contribution of a personal contract to an individual with salary grade 50 (i.e., in the middle of the salary grade scale) and 16 years of seniority was calculated. These values are close to their average among personal contract employees. The development of the overall contribution of a personal contract to this type of employee (a Type 1 employee) and other types of employees (as explained below) is displayed in Figure 3.



The results of the one-year regressions (not displayed here) indicate that the overall contribution of a personal contract declined by 2 percent each year, reaching a (low) negative level by the end of the period. At the end of the period, the salary of a personal contract employee was 0.5 percent lower than that of a similar employee in the employment scale-salary grade framework. The decline in the value of a personal contract could have been due to a drop in the real value of a personal contract, meaning a salary lower than the employment scale-salary grade salary all along the entire salary grade scale, or to a drop in the return for a salary grade in a personal contract, compared with the return for a employment scale-salary grade employee. In terms of the coefficients, - either the personal contract coefficient or the salary grade order X personal

¹³ Without the women X personal contract interaction, however, in order to obtain an average picture for the two groups.

¹⁴ Due to a lack of observations, 1997 was not included in the test.

contract coefficient declines in later years. The estimates from the one-year regressions (not displayed here) support the first possibility – that the return on a salary grade in a personal contract decreased - (i.e., became more negative) over the years, and the salary of an “average” personal contract employee with a salary grade order of 50 and 16 years of seniority falls in comparison with that of his counterpart in the employment scale-salary grade framework. Indeed, the values of both the personal contract coefficient and the “seniority X personal contract” coefficient changed over the years, but the difference in the return on salary grade had an extremely significant effect on the salary ratio between personal contract employees and employment scale-salary grade employees.

This analysis is highlighted in Figure 3, which displays the overall return on a personal contract for two additional types of representative employees: employees with a salary grade order of 10 and two years of seniority, and employees with a salary grade order of 25 and nine years of seniority. For both of these categories, the contribution of a personal contract changed over the years, but a significant positive difference was maintained in comparison with non-personal contract employees.

According to the above, it follows that the significant contribution of a personal contract, which has remained high since 1998, is located in the lower part of the salary grade scale, i.e., lower than a salary grade order of 50 (the middle of the scale). Above this salary grade order (and given seniority typical of this salary grade order), the contribution of a personal contract in recent years is not significant, and has even been negative since 2008. One possible explanation of the general downtrend in a personal contract’s contribution is that the proportion of academic employees joining public service under personal contracts has increased over the years: only 15 percent of academics who joined public service in 2000 were under personal contracts, compared with 40 percent in 2008 and 34 percent in 2009. The correlation between the overall return on a personal contract and the proportion of total academics newly recruited to public service in 1998-2009 who were under personal contracts was -0.589. It is clear that the difference is not merely quantitative; the use of personal contracts became more prevalent: accepting an employee on a temporary basis that does not include the obligation to grant tenure. In such a case, it is possible that the salary offered by public service will not be much higher than that offered in the employment scale-salary grade framework, which would explain the decline in the return on a personal contract.

2.4) Personal Contracts and Salary Promotion

The findings in the preceding section relate to all personal contract employees, both those who joined public service and received a personal contract and those who previously worked in the employment scale-salary grade framework and switched to a personal contract. As we saw, the rising proportion of employees entering under personal contracts is correlated with the decline in the return on a personal contract. The picture for the significance of personal contracts for academics entering public service is insufficiently clear, and it appears that the use of such contracts is different for these employees; in contrast, the picture for those switching from employment scale-salary grade to a personal contract is clearer, and indicates its use as a tool for significant salary promotion for an outstanding employee in order to retain him within the system. In view of this, it is worthwhile considering the meaning of a transition to a personal contract from the salary promotion perspective.

In the following regressions, the explained variable is the change in the logarithm of the real salary between year t and year $t-1$. The explanatory variables are gender, age, age squared, and seniority, and dummy control variables for transition to a different employment scale, transition to a different employment scale group, transition to a personal contract, and remaining in a personal contract (i.e. if the employee was in a personal contract in both year t and year $t-1$). The change in the salary grade order is also included as an explanatory variable, as is the change in the logarithm of the real salary between year $t-1$ and $t-2$ and between year $t-2$ and $t-3$, two variables that proved to be extremely significant in explaining the increase in salary (Mazar, 2007). Table 2 displays the model estimation results for three population groups: all employees (Columns 1), employees who moved from any group to the group of academics only (Column 2), and academic employees in selected professions who were not in personal contracts (Column 3)¹⁵ – in 1997-2009.

Table 2 – Regression Results for Salary Promotion

	(1)	(2)	(3)
	All Employees	Employees Who Moved to Academic Professions	Academic Employees in Selected Professions
Transition to a personal contract	0.2381*** [0.0091]	0.0921*** [0.0108]	0.1426*** [0.0179]
Change in the logarithm of real salary between year $t-1$ and year $t-2$	-0.4535*** [0.0020]	-0.4346*** [0.0044]	-0.4493*** [0.0048]
Change in the logarithm of real salary between year $t-2$ and year $t-3$	-0.1748*** [0.0016]	-0.1474*** [0.0036]	-0.1598*** [0.0038]
Women	0.0019*** [0.0003]	-0.001 [0.0007]	0 [0.0008]
Age	-0.0013*** [0.0002]	-0.0057*** [0.0004]	-0.0040*** [0.0004]
Age squared	0.0000*** [0.0000]	0.0000*** [0.0000]	0.0000*** [0.0000]
Seniority	-0.0007*** [0.0000]	-0.0005*** [0.0001]	-0.0009*** [0.0001]
Change in salary grade order	0.0022*** [0.0001]	0.0032*** [0.0001]	0.0034*** [0.0002]
Transition to a different employment scale	0.0561*** [0.0071]	0.1153*** [0.0081]	0.0766*** [0.0119]
Transition to a different employment scale group	-0.0154** [0.0077]	-0.0883*** [0.0085]	0.3015*** [0.0206]
Remaining in a personal contract	0.0073*** [0.0018]	0.0140*** [0.0018]	
Constant	0.0602*** [0.0035]	0.1778*** [0.0101]	-0.3885*** [0.0585]
Number of observations	665,905	131,424	106,415
R-squared	0.214	0.225	0.239

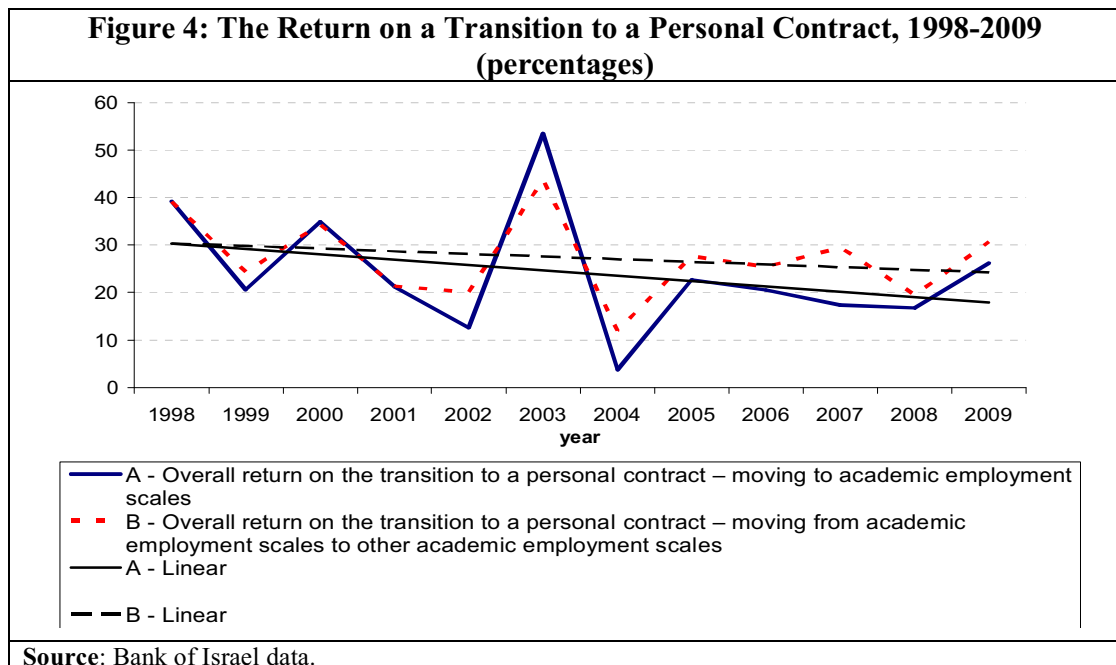
Source: Bank of Israel data. The standard error is in parentheses.

* denotes a p-value less than 0.1 ** denotes a p-value less than 0.05; *** denotes a p-value less than 0.01.

¹⁵ This refers to employment scales for the social sciences and humanities (e.g., economists), engineers, auditors, lawyers (and other scales in the law field), journalists, and social workers. The proportion of these employment scales among total academic employees is about 75 percent, and the special characteristics of this group will be explained later.

The most interesting variable is obviously the dummy variable for the transition to a personal contract, but in every transition to a personal contract, the employment scale also changes,¹⁶ and attention should therefore focus on the sum of the two. The results show that the value of a transition to a personal contract is estimated at 23 percent, and the value of a transition to a different employment scale is 5.6 percent, making the total increase in salary of an employee switching from employment scale-salary grade to a personal contract an estimated 28.6 percent. Among employees who switched to personal contracts within the academic group, the value of the transition was 20.7 percent (9.2 percent from the personal contract and 11.5 percent from the transition to a different employment scale). Among employees in selected professions, the value of the transition was 22 percent. The table also indicates that personal contract employees benefit from faster promotion: an employee who was under a personal contract in both a given year and the following year received a salary increase of 0.7 percent (among all employees) or 1.3 percent (among those switching to academic professions). These raises were higher than those of a similar employment scale-salary employee. This finding brings into question Sussman's assertion (1995) that a personal contract yields only short-term profits, because it appears that progress in salary is also more rapid for those under personal contracts.

As in the preceding section, here, too, the variance in the effect of the transition to personal contract over the years among academic employees who switched to personal contracts and among employees in selected academic professions who switched to such a contract was tested. A similar picture was obtained (Figure 4): The overall return on a transition to a personal contract declined over the years – from about 40 percent in 1998 to about 30 percent in 2009.



¹⁶ Because a change in employment scale is technically defined as a change in the symbol of the employee's employment scale, and the same is true for a transition from an ordinary employment scale to a personal contract, or between two employment scales within the personal contract employment scales.

To summarize this section: it is clear that a significant proportion of public service employees are working under personal contracts, although a distinction should be made between personal contracts signed with existing employees for the purpose of retaining them in the system and personal contracts signed with newly recruited public service employees, the purpose of which is insufficiently clear. The return on an academic's personal contract declines over the years, but it is difficult to check whether this decline is real, or reflects a difference in populations. Evaluating the overall return on a transition to a personal contract (i.e., with the addition of the return on a transition to a different employment scale) evades the problem of different populations, and also indicates a drop in the return on a transition to a personal contract, albeit a smaller one.

3) The Effectiveness of Personal Contracts

Following the brief description of the quantitative history of the personal contracts mechanism, a description that focused on the group of academics, we can direct our attention to considering the main study question: whether the personal contracts tool, which was designed to retain outstanding employees in the system, is really effective (or, according to a different perspective, whether personal contracts have any effect whatsoever on the duration of service). The analysis framework includes two main theoretical matters that should be discussed before attempting to answer this question.

3.1) The Theoretical Framework

3.1.1) A Natural Trial and Propensity Score Matching

The parameter that must be measured in order to answer the study question is the effect of personal contracts on the duration of service. In terms of evaluating this treatment, we want to measure the average effect of the treatment (personal contracts) on those being treated (the employees who switched) in terms of work years following the treatment.

Formally – we wish to measure θ given by:

$$\theta = E[Y_1 - Y_0 | T = 1] = E[Y_1 | T = 1] - E[Y_0 | T = 1]$$

Where Y_1 is the result of the treatment, Y_0 is the result without treatment, and T is the treatment (which equals 1). θ is known as the average treatment effect on the treated – ATT. The problem begins when we cannot estimate $E[Y_0 | T = 1]$ - the situation of an individual placed in the treatment group, had he not really received the treatment. This element, which is called counterfactual, is unobserved, and therefore cannot be well identified.

Without such identification, we have a problem of selection, which makes the results biased, since there is no way to ensure that the individuals placed in the treatment group are really similar to the individuals who were not placed in it. The existence of an unobserved (or observed, but not controlled) factor that affects both placement in treatment and the results is possible, and if so, the estimation of the treatment effect is biased. In order to solve the identification problem, a group must be found that will constitute an adequate estimate of the counterfactual, so that it will be possible to find the average treatment effect on the treated. Assuming that all the factors causing individuals

to be selected for the treatment group are known, the counterfactual can be estimated. This is the conditional independence assumption – CIA. Formally:

$$(assumption\ 1)\ Y_0 \perp\!\!\!\perp T \mid X = x, \forall x \in \mathcal{X}$$

This means that given the observed variable X , which is one of the variables affecting placement in \mathcal{X} , the treatment group, the placement is independent of the result variable without treatment. In another form:

$$E[Y_0 \mid T = 1, X] = E[Y_0 \mid T = 0, X] = E[Y_0 \mid X]$$

Although $E[Y_0 \mid T = 1, X]$ is unobserved, $E[Y_0 \mid T = 0, X]$ can be used as a substitute for it, and this element is simply the result variable for the individuals who were not placed in the treatment group, or, in our case, those who did not receive personal contracts. At the same time, not all the individuals who were not placed in the treatment group can constitute a suitable comparison group; only those whose variables affecting placement in the treatment group have the same values as those who were placed in the treatment group are suitable. In order to obtain such a group, it must be assumed that there are no specific values of the explanatory variables that can absolutely guarantee placement in the treatment group (with a probability of 1). Formally:

$$(assumption\ 2)\ P(x) < 1, \text{ where } P(x) = P(T = 1 \mid X = x)$$

Are the two critical assumptions correct? The second assumption is simpler to check: if it appears that only some of the population with effect variables (i.e., factors that affect receiving a personal contract) received treatment, this assumption is correct. It is more difficult to check the first assumption, and I will address it later. As stated, in order to create a good comparison group, it is necessary to find individuals without personal contracts having the same characteristics as individuals with personal contracts. When the factors having an effect and their possible values are few, there is some likelihood that a sufficiently large sample will include individuals with matching characteristics. Usually, however, this is not the case – either because of the sample size or the multiplicity of both factors having an effect and their possible values.

On the face of it, multivariate regression is likely to help solve this problem by including factors that affect placement in the treatment group as explanatory variables, thereby exercising “control” over these factors and making it possible to obtain an unbiased estimate of the effect of treatment. This method, however, has disadvantages. One of them (noted, along with other disadvantages, in Ichino, et al. (2006)) occurs when the values of the variables that explain placement in the treatment group have significantly different probability distributions for individuals placed in the treatment group and individuals who were not placed in it. When more than 10 explanatory variables are involved (as in this study), it is likely that not all the probability distributions of the explanatory variables will be similar. In this case, estimating the effect of the treatment will be very dependent on the specification of the model, and the threat of selection will be more tangible.

Another method, which I will use, is to reduce the multiplicity of explanatory variables to a single variable – the probability of being placed in the treatment group, given the relevant variables: $p(X) \equiv P(T = 1 \mid X)$. Given $p(X)$, and if the assumption of CIA

(assumption 1) holds, Y_0 and Y_1 are independent of T (Rosenbaum and Rubin, 1983). This means that for individuals with the same $p(X)$, being placed in the treatment group or the control group is completely random. With the help of dependence on $p(X)$ instead of on X , the effect of the treatment is now given by:

$$\tau|_{T=1} = E_{p(X)}[(\tau|_{T=1,p(X)}) | T=1]$$

This means that the average effect of the treatment the expected effect of the treatment on all the individuals who received treatment, given $p(X)$. As stated, the main advantage of this method is that it reduces the number of dependency dimensions to 1.

3.1.2) Survival Analysis¹⁷

The use of propensity score matching (PSM) makes it possible to create a control group identical to the treatment group, and the distance from there to estimating the effect of the treatment is short. In the current context, however, there is another theoretical matter that must be addressed: the result variable here is the duration of service; the tools of survival analysis should be used when analyzing a variable of this type.¹⁸

Survival analysis is a framework for analyzing events that occurred after a given time, or that did not occur at all. Ostensibly, probit or logit probability regression, in which the dependent variable is dichotomous, receiving the value 1 if the event occurred and 0 otherwise, could provide a solution for this kind of analysis. Regressions of this type, however, have several disadvantages, the most prominent of which is that it treats an event that did not occur during the time span for which information is available as if it will never occur in the future. Observations of this type, which are called censored, will receive the value 0 as the dependent variable, even if it is unknown whether or not the event will occur later. One way of overcoming this problem is to omit observations in which the event can still occur, but omitting observations involves a loss of information, and in cases with a small number of “good” observations, this could prove extremely important. The other ways of solving this problem are no better.

There are several approaches to survival analysis. I have chosen Cox’s proportional hazard method, first proposed by Cox in 1972. Its main advantages over the other methods are the absence of any need to make assumptions about the true probability distribution of the event occurrences and the relative ease of adding time-dependent explanatory variables to the model. On the other hand, the word “proportional” in the method’s name and the absence of assumptions about the probability distribution indicate that the immediate result of this analysis method is identifying the relative probability, not the absolute probability.¹⁹ Since the current study focuses on the relative effect of personal contracts, this feature does not detract from the fitness of this analysis method.

¹⁷ Both this section and the implementation of the analysis in the SAS program is based to a large extent on Allison’s book (1995).

¹⁸ Other articles using survival analysis of a sample based on paired observations include Boockmann and Hagen (2008), Frederiksson and Johansson (2004), and Hujer et al. (1999).

¹⁹ Although the base probability in this model can also be estimated.

The risk for individual i is defined by the risk function:

$$h_i(t | x_{i1}(t) \dots x_{ik}(t)) = \lambda_0(t) \exp[\beta_1 x_{i1}(t) + \beta_2 x_{i2}(t) + \dots + \beta_k x_{ik}(t)]$$

Where $x_{i1}(t) \dots x_{ik}(t)$ are k characteristics of the individual at time t that contribute to risk. The expression $\lambda_0(t)$ represents the “basic risk” – the risk that an event will occur, given that all the individual’s characteristics are 0. Since we are interested in the relative risk, we want to estimate the ratio of the risk between i and j , i.e.:

$$\frac{h_i(t)}{h_j(t)} = \exp[\beta_1(x_{i1}(t) - x_{j1}(t)) + \dots + \beta_k(x_{ik}(t) - x_{jk}(t))]$$

In such a presentation, there is no need to estimate the basic risk, since it is canceled out; we estimate only the relative risk. The estimation method uses maximization of the partial likelihood function. The partialness of this method is reflected in its non-inclusion of information about β , which is obtained from the basic risk, as a result of which the results are not the most efficient. It has been proven, however, that the loss of efficiency is not great (Efron, 1977). At the same time, the results are robust, because they do not include any assumptions whatsoever about the probability distribution of the basic risk. In addition, only the order of events has significance in this method, not the time when the event occurs.

The partial likelihood is defined as follows: assume that there are n individuals, and it is known that an event occurred at time t . The partial likelihood of individual 1 (L_1) will give the probability that an event occurred to individual 1, given that the event occurred at time t . In other words:

$$L_1 = \frac{h_1(t)}{h_1(t) + h_2(t) + \dots + h_n(t)}$$

The denominator includes only individuals still in the risk group at time t , meaning those for whom the event has not yet occurred. According to the above definition of risk, this equation can be expanded as follows:

$$L_1 = \frac{\lambda_0(t) \exp[\beta_1 x_{11}(t) + \dots + \beta_k x_{1k}(t)]}{\lambda_0(t) \exp[\beta_1 x_{11}(t) + \dots + \beta_k x_{1k}(t)] + \dots + \lambda_0(t) \exp[\beta_1 x_{n1}(t) + \dots + \beta_k x_{nk}(t)]}$$

Where the expression $\lambda_0(t)$ is immediately canceled out. If there are n individuals, the partial likelihood function will be the product of L for all the individuals for whom the event occurred at any time t whatsoever:

$$PL = \prod_{i=1}^n \left[\frac{e^{\beta' X_i(t)}}{\sum_{j \in R(t_i)} e^{\beta' X_j(t)}} \right]^{\delta_i}$$

The risk function is in the numerator (without the basic risk) of individual i for whom the event occurred, with a vector X of the various characteristics, as observed at the time of the event. The denominator contains a summation of all the individuals in risk group $R(t_i)$, i.e., those for whom the event had not yet occurred when it occurred for individual

i . δ_i is an indicator that receives the value 0 if individual i is censored, and 1 otherwise. Consequently, only non-censored individuals contribute to the partial likelihood function through the numerator, while all the individuals in the database contribute through the denominator. The estimates for β are obtained through maximization of the logarithm of the partial likelihood:

$$\max_{\beta} \log PL = \sum_{i=1}^n \delta_i [\beta' X_i(t) - \log \sum_{j \in R(t_i)} e^{\beta' X_j(t)}]$$

3.1.3) The Question of Equal Event Times in Survival Analysis

A final theoretical question that is extremely relevant to the type of data available to me is dealing with events whose times of occurrence are identical (tied data) in the framework of survival analysis. As explained above, in the likelihood of each event, it is important to know for which individual it occurred before it occurred for other individuals. In a case in which the data for the event time are the same for several individuals, however, calculating the likelihood is not simple. In this case, a distinction should be made between two sub-events: in one, both the data for the time of occurrence are the same and the *actual* times are the same; in the other, the data for the time of the occurrence are the same, but the actual times of occurrence are not. In this study, the second case is the relevant one: the data for the time when an employee leaves are in years (did he leave in 2003? In 2004?), while in reality, employees who left during the same year also probably left in different months, and it is even more likely that they left on different days. It is possible to deal with this problem by weighing all possible orders of occurrence of cases at a given time, i.e., if it is known that the event occurred for k individuals at time \hat{t} (a given year), but the true order of the events (in which month) is unknown. There are $k!$ possible orders of the events, which we will denote by $(A_1, A_2 \dots A_{k!})$. We are looking for the probability that is the union of the probabilities in each of the events, which is obtained from the summation:

$$\Lambda_{\hat{t}} = \sum_{i=1}^{k!} L(A_i)$$

Where $L(A_i)$ is the product of L as defined in the above equation for order A_i . If k is sufficiently large, the calculation is liable to be very lengthy, and approximation methods will be required as a substitute for the exact calculation. Carrying out the exact calculation in this study, however, as displayed below, did not require computer resources and/or extraordinary times, and this calculation was therefore preferred over all approximation methods.²⁰

3.2) The Effectiveness of Personal Contracts – An Empirical Analysis

The Population

The population tested in order to answer the study question consisted of employees in 2001 and 2002. This population was selected because of the need to balance two

²⁰ In cases in which an approximation method was needed because the model did not converge, the Efron method was selected.

requirements: first of all, the population of those switching to a personal contract had to be as large as possible in order to obtain a more efficient estimate; second, the population had to have a relatively long service horizon, so that conclusions could be drawn about the profile of those who left. That is why this population was chosen: on the one hand, as can be seen in Figure 2, the number of those switching a personal contract was relatively large in both of these years; on the other hand, a service horizon of 8-9 years (until 2009) is long enough to draw conclusions about the leaving dynamics. The significance of the analysis is therefore effectively limited to this specific population, but the great variance in the value of the incentives in personal contracts, as presented in the preceding section, in any case complicates any attempt at generalization concerning the general quality and significance of those incentives; it will therefore be more correct to focus on an analysis of a specific population.

From the academic employees, the population includes only those in key employment scales for whom a majority of the transitions to personal contracts was observed.²¹ A restriction was imposed for employees who switched – at the time of moving to a personal contract, they had to have been in public administration for at most 10 years. This upper bound is essential, because, as explained above, the consequences of the transition from the perspective of those who had accumulated budgetary pension rights for up to 10 years differed from the consequences for those with more seniority.²² Employees who switched to personal contracts later than 2001-2002 were also eliminated.

The Analysis Method

I will begin the empirical analysis with a simple analysis²³ of the chances of leaving after a given number of years. The explanatory variables included in the model were a dummy variable for the transition to a personal contract, the employee's gender, the logarithm of his real salary, seniority, salary grade order, actual years in service, age and age squared, and the values of these last six variables one year before he left. Another variable, which constitutes a proxy for the employee's quality, is the average change in the **base** salary (which is affected only by the employment scale, salary grade, and seniority) since entering public service. Salary itself on entering public service was also included in order to control for different levels of the starting salary. The assumption underlying this variable is that a high-quality employee's speed of promotion is faster than that of a mediocre employee. The problem with this variable is that it also includes changes resulting from wage agreements, and control for the year in which the employee joined is therefore needed as well. Another problem is the high correlation between this variable and a transition to a personal contract, because a transition to a personal contract, as demonstrated above, raises salary by double-digit percentages. A final variable included is the change in salary, compared with the preceding year.²⁴ Table 3 displays descriptive

²¹ Social sciences and humanities (e.g., economists), engineers, auditors, lawyers (and other scales in the law field), journalists, and social workers.

²² The effect of a personal contract was also tested for the population without this restriction, and the results were similar.

²³ In the sense of ignoring the selection problem resulting from the fact that placement here was not random.

²⁴ The summary section explains the meaning and importance of these variables.

statistics of the explanatory variables for their value one year before leaving for employees who left, and their value in 2009 for employees who remained.

Table 3 – Descriptive Statistics of the Entire Population One Year before Leaving for Those Who Switched to Personal Contracts, and in 2009 Without Those Switched

Variable	All Employees	Employees Who Did Not Switch to Personal Contracts	Employees Who Switched to Personal Contracts
N	5,840	5,661	179
Age	45.7	46.0	38.2
Seniority	17.1	17.3	9.7
Salary grade order	50.4	50.2	56.8
Proportion of women	0.61	0.61	0.49
Total real salary (2007 prices)	10,362	10,262	14,077
Actual years in public service	11.1	11.2	9.3
Rate of change in salary since the preceding year	5.88	5.77	9.09
Average rate of change in salary since entering public service	5.74	5.25	20.75
Real salary upon entering public service	3,008.2	3,012.3	2,878.3
Years until leaving	6.11	6.10	6.37

Source: Bank of Israel data.

This table shows no significant differences in duration of service between employees who switched to personal contracts and other employees, but a prominent difference is evident in the averages of the explanatory variables. Some of this difference is due to the additional restriction on employees who switched (at most 10 years of seniority in public service). A significant difference is evident in the average rate of change in salary since entering public service (20.75, compared with 5.25), originating in the large rise in salary when switching to a personal contract and in the bias created by the definition of all salary for personal contract employees as base salary.

A regression model is likely to solve the problem of differences in the explanatory variables. Two techniques were considered for estimating the effect of a personal contract on the changes of leaving. One is logit regression. The dependent variable is defined by an employee's leaving by a given maximum number of years, and the seventh year was selected as the maximum number of years for this purpose.²⁵ For both cohorts (the group of employees from 2000 and the group from 2001) this meant leaving at almost the end of the observed period, i.e., in 2009. As mentioned, the explanatory variables receive at their value in the last observed year; for employees who left, this was their final year of work in public service, and for employees who remained, the year in which they completed the stipulated maximum number of years for remaining in public service.

The disadvantages of this survival analysis technique were explained above, and here they are more prominent, because 60 percent of those observed finished the observed period (2009) without the event having occurred, and are therefore counted as

²⁵ In order to highlight the meaning of the number of years before leaving: an employee who switched to a personal contract during 2001 and left in 2007 had six working years, 2001-2006, since in 2007 he was already not observed in the database.

observations receiving the value 0;²⁶ in point of fact, however, they should have been counted as censored observations, i.e., those for which information about their future is lacking. Removing these samples from the study population did not significantly change the results displayed below²⁷, but as explained above, this involves a loss of much information likely to be important.

A more accurate analysis will therefore use Cox’s proportional hazard model, taking into account censored observations of employees whose time of departure — before or after the observation period — is not known. In using this technique, there is no need to define any “threshold year” (like the 7-year threshold in logit), according to which the dependent variable is defined, since the relative effect of the transition to a personal contract is taken into account in each leaving event, i.e., in each year by itself. The logit results appear in Columns (1) and (2) of Table 4, while Columns (3) and (4) display the results of the Cox regression estimation. Columns (2) and (4) did not include the proxy variable for excellence because of its high correlation with the transition to a personal contract. All of the results are in terms of the odds ratio.

Table 4 – Results of Estimating the Factors Affecting the Probability of Leaving
The dependent variable in Columns (1) and (2) is 1 if the employee left within seven years, and 0 otherwise.

	(1)	(2)	(3)	(4)
Estimation technique	Logit (left by up to 7 years)	Logit (left by up to 7 years)	Cox’s Proportional Hazard Model	Cox’s Proportional Hazard Model
Switched to a personal contract	0.393**	1.612*	1.508***	1.538***
Women	0.572***	0.625***	0.883**	0.885**
Log of the real salary	0.088***	0.108***	0.386***	0.387***
Seniority	1.055***	1.049***	1.017***	1.018***
Salary grade order	1.025***	1.029***	1.019***	1.02***
Change in salary since entering public service	1.1117***		1.002	
Salary on entering public service	1		1	
Number of years in public service	0.501***	0.503***	1.083***	1.08***
Age	0.513***	0.522***	0.681***	0.681***
Age squared	1.007***	1.007***	1.004***	1.004***
Change in salary since the preceding year	1.007***	1.009***	0.998	0.999
Control for the year of joining public service	Yes	No	Yes	No
Control for ministries	Yes	Yes	Yes	Yes
Control for employment scales	Yes	Yes	Yes	Yes
Number of observations	5,087	5,129	“Good” observations: 1,994 Censored observations: 3,846	

Source: Bank of Israel data. The estimates are listed in terms of odds ratio. The number of observations is less than in Table 3 because data is missing for one of the explanatory variables in the regression.

* denotes a p-value less than 0.1; ** denotes a p-value less than 0.05; *** denotes a p-value less than 0.01.

²⁶ Actually, some of those observed left after eight years, and these observations also received – according to the definition – the value 0. I also checked the case in which eight years were defined as the maximum number of years, and the results were very similar.

²⁷ This is according to the model estimated for non-censored observations only. The results are not displayed here.

The differences between the two techniques in the estimation results are rather prominent in both direction and magnitude. The key variable – the transition to a personal contract – has a negative effect on the chances of leaving in the logit regression (odds ratio less than 1, Column (1)), and a positive effect in the Cox regression (Column (3)). A similar pattern also emerged for the number of years in public service. In the estimates for the effects of gender, logarithm of the real salary, seniority, salary grade order, age, and age squared only the magnitude of the effect was different, not the direction. On the other hand, the effect of the proxy variable for excellence became non-significant. Furthermore, a comparison of Columns (2) and (4) proves that the logit regression is more sensitive to the high correlation between the excellence variable and the transition to a personal contract: when the excellence variable is not included in the logit regression, leaving the personal contract variable by itself, and the employee's quality only to a certain extent, the effect of the transition to a personal contract becomes positive, albeit only with a p-value of 9 percent. In contrast, a similar exercise with Cox regression proves that this technique is almost completely non-sensitive to a change.

What causes the differences in results between the two techniques? Without entering into a detailed explanation of each difference, it can be said, as explained in the theoretical section, that the differences originate in different treatment of employees who remained until 2009 – whether or not they are part of the population for which the model was estimated, and if they are not included in it, whether these employees should be ignored, or whether they have important information that can be used. In the current case, in which many observations are censored (3,846 observations), the difference between the two techniques is significant, and is likely to be the cause of the large differences in results. In general, therefore, it is preferable to use the Cox model in survival analysis, rather than the logit model, especially in the case being discussed here, in which the differences between the two techniques are acute.

The above analysis, however, is unsatisfactory: as explained above, personal contracts are signed with active employees in order to retain them in the system by raising their salary significantly, combined with certain concessions on their part. In order to examine whether this method was successful, we need to know when an employee would have left, had he not been granted a personal contract. Looking at all the employees who did not receive personal contracts is not enough, because a selection that affects both the chances of getting a personal contract and the duration of service is possible. This is primarily a result of the idea underlying the personal contract from the official perspective: retaining excellent employees in the system. This means that the main difference between employees who received a personal contract and those that did not is in their quality – excellent versus not excellent – and quality is also likely to affect (in an uncertain direction) the duration of service. We therefore turn to the creation of a control group using the PSM method explained above. Pregamit and Veum (1999), who empirically tested the effect of promotion on the duration of employment, did not correct for the identification problem through the PSM or any other method, and their findings, like those presented above, are biased. If a personal contract is not received randomly, then there is some difference between employees who received one and those who did not from an unofficial perspective, too – albeit not necessarily a difference in quality – and the PSM method will help overcome this difference.

It is necessary to first identify the employees with the greatest probability of obtaining personal contracts, but who have not received them. Since there is no way to know which employees have been offered a personal contract, only some of whom have actually signed a personal contract, the probability of receiving one will be estimated through an analysis of the connection between the observed variables and the probability of obtaining a personal contract.

The population is the same as defined above, with three main differences: since we are interested only in the probability of moving to a personal contract in certain years, I did not apply the restriction on the transition to a personal contract during an employee's career, because this is irrelevant. Similarly, an employee who did not receive a personal contract in 2001 and remained in his workplace appears twice in the database – once as a “candidate” to receive a personal contract in 2001 and again as a candidate to receive one in 2002. Another difference is applying the restriction on years of service (no more than 10) to the entire population, not just those switching to personal contracts, because, as stated, this influences the decision to switch to a personal contract. Descriptive statistics of this population according to the main variables to be used in the probabilities regression are displayed in Table 5. The averages are for the year preceding the transition or the possibility of transition (i.e. 2000 and 2001).

Table 5 – Descriptive Statistics of the Entire Population in the First Year in which They Appear in the Sample – Both Those Who Switched to Personal Contracts and Those Who Did Not

Variable	All Employees	Employees Who Did Not Switch to Personal Contracts	Employees Who Switched to Personal Contracts
N	9,241	9,062	179
Age	39.2	39.4	31.6
Seniority	11.1	11.2	3.6
Salary grade order	39.3	39.5	28.6
Proportion of women	0.60	0.61	0.49
Total real salary	9,818	9,845	8,447
Proportion of employees working in ministries subject to the Civil Service Commission	0.74	0.74	0.95
Average rate of change in salary since entering public service	8.4	8.3	13.1
Salary grade order divided by seniority	7.0	6.8	15.9

Source: Bank of Israel data.

For an analysis of the probability of changing to a personal contract for the purpose of providing a propensity score for every observation, I used the popular method proposed by Dehja and Wahba (1999), and chose to use a logit model in which the dependent variable is whether the employee switched to a personal contract in year $t+1$, and the independent variables are the individual's observed variables in year $t-1$. The list of these variables includes the employee's gender, age and age squared, seniority and seniority squared, salary grade order and salary grade order squared, and the logarithm of his total real salary.²⁸ Several variables for the employee's environment were also included – the

²⁸ I also have data for the employee's family status, but a check revealed that this variable has no effect, and the profit in terms of quality of fit is negligible. On the other hand, the cost of including this variable is that in the control group, created on the basis of the regression estimates that include it, it is impossible to carry

proportion of personal contracts in the employee's ministry, the proportion of personal contracts in his unit, a dummy variable that receives the value 1 if the senior employee in the employee's unit²⁹ is employed under a personal contract, and another dummy variable that receives the value 1 if the employee's ministry is subject to the Civil Service Commission.

Two proxy variables for excellence were tested in the probability model: one, as explained above, is the rate of increase in the base salary between the entry into public service and the transition to a personal contract; the other is the salary grade order divided by seniority, assuming that of two employees with the same seniority, an excellent employee will have a higher salary grade order. The main results of the estimation in terms of the odds ratio are displayed in Table 6.

Table 6 – Estimation Results for Logit Regression on the Probability of Switching to a Personal Contract

	(1)	(2)
Subject to the Civil Service Commission	2.041** (0.3604)	2.691*** (0.3577)
Women	0.522*** (0.1796)	0.466*** (0.1759)
Salary grade order	0.951** (0.0246)	
Salary grade order squared	1 (0.000355)	
Age	1.002 (0.1339)	0.869 (0.116)
Age squared	1 (0.00176)	1 (0.00154)
Logarithm of the real salary	1.26 (0.2977)	0.825 (0.2822)
Seniority	0.826** (0.0782)	
Seniority squared	0.999 (0.00439)	
Excellence variable: salary grade order/seniority		1.016*** (0.00503)
Excellence variable: average rate of change in salary since entering public service	1.01*** (0.00262)	1.01*** (0.00276)
Percentage of employees under personal contracts in the ministry	1.038*** (0.00723)	1.029*** (0.00697)
Percentage of employees under personal contracts in the unit	1.039*** (0.00455)	1.04*** (0.00446)
Whether the senior employee in the unit is employed under a personal contract	1.223 (0.2189)	1.026 (0.2119)
Number of observations	8,671	8,671
McFadden's pseudo R-squared	0.296	0.260

Source: Bank of Israel data. The estimates are listed in terms of odds ratio. The standard error is in parentheses.

* denotes a p-value less than 0.1; ** denotes a p-value less than 0.05; *** denotes a p-value less than 0.01.

out accurate estimation of the Cox model with equal event times; various approximation methods must be used, as explained in Section 3.1.3. I therefore decided not to include a family status variable. As I said, this decision has no real effect on the results.

²⁹ The senior employee is defined as the employee with the highest salary.

The variables having an effect can be divided into two categories. One group consists of the individual's variables. In this group, it appears (Column (1)) that the salary grade order and seniority variables have an effect, but a negative one: the higher an employee is on salary grade scale, or the more seniority he has in his workplace, the lower his chances are of receiving a personal contract. Column (2) indicates that the ratio between them – also used as a proxy for excellence – is positive; the higher the employee climbed in the salary grade order in a shorter period of service, the greater his chances are of receiving a personal contract. The logarithm of the salary does not have significant effect, nor does age. On the other hand, an employee's gender has an extremely significant effect: a male's chances of obtaining a personal contract are more than double those of a woman. The significance of the two excellence variables strengthens the official perspective, according to which those winning a personal contract are relatively excellent, although the effect of excellence on the probability of obtaining a personal contract is not great: the odds ratio for the excellence variables is 1.01 for the "average rate of salary change since entering public service" and 1.016 for "salary grade order divided by seniority."

The second category consists of characteristics of the workplace. The chances of obtaining a personal contract of an employee whose ministry is subject to the Civil Service Commission (for example, this excludes Knesset employees, employees in the Office of the State Comptroller, etc.) are double or more those of an employee whose ministry is not subject to the Civil Service Commission. Also extremely prominent is the effect of the proportion of contract employees in the employee's ministry and unit, which indicates that personal contracts are concentrated in certain ministries.³⁰ The result likely reflects the assertion by Brown and Sisson (1975) that employees are inclined to compare their salaries with each other, especially in a relatively small environment (at the unit level, for example), and a relatively high frequency of personal contracts in a workplace will therefore lead to more pressure on employers by employees who did not receive a personal contract aimed at obtaining one. On the other hand, whether or not the senior employee in a unit is under a personal contract does not affect the chance of obtaining a personal contract.

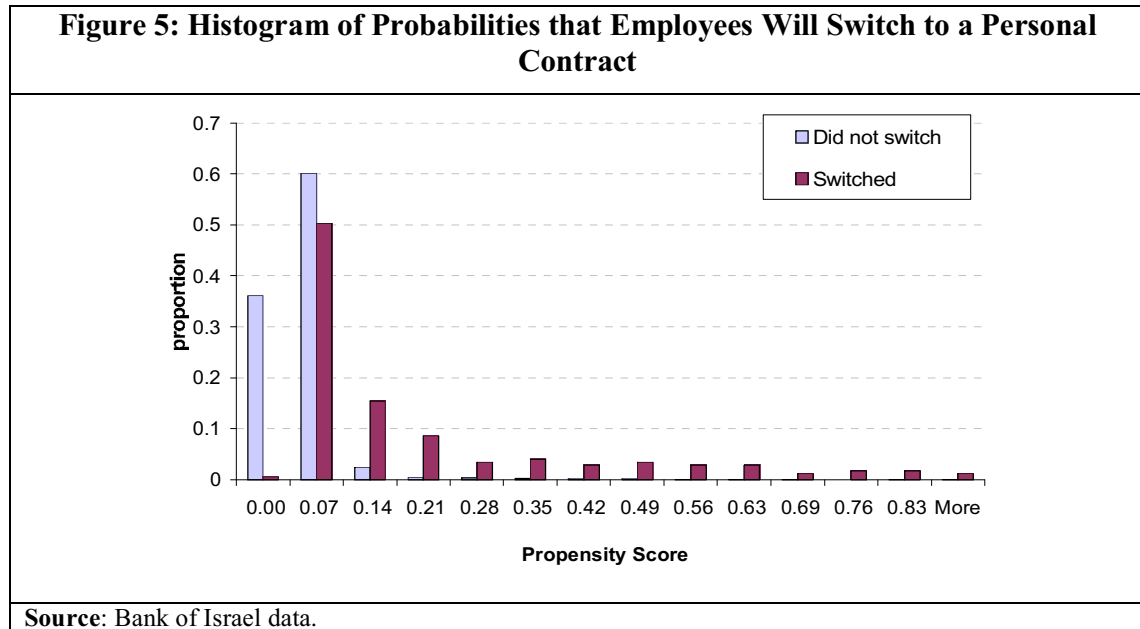
In the absence of an R-squared value in the logit regression, the explanatory power of the model can be checked with the help of statistics that simulate such a value (pseudo R-squared). The use of Mcfadden's R-squared indicates that the explanatory power of model (1) is 0.296.³¹ This value, which is high for the logit regression used in PSM analysis, reinforces assumption 1 (assumption of CIA) to some degree. Additional specifications for the model were tested,³² but the differences were large in neither the significance of these results, nor in the final main result.

³⁰ Because of the data's sensitivity, it is impossible to obtain probability distribution of personal contracts according to ministry.

³¹ Other methods for estimating the explanatory power yield similar results.

³² Inclusion of the "actual years in service" variable; inclusion of relative excellence variables within the ministry/unit.

Given the probability model³³, a “score”, or probability, can now be assigned to each observation in the sample on the basis of the observed variables. Figure 5 displays the histogram of the probabilities for employees who switched to a personal contract, and for those who did not switch.



The important result supported by the graph is the existence of common support, i.e., a range in which the probability of obtaining a personal contract among those who received one was similar to the same probability among those who did not receive one.³⁴ The existence of this range verifies assumption 2, and is likely to indicate that there are employees whose personal data (including unobserved data, such as quality and bargaining power) and environmental data qualified them for receiving personal contracts, but who did not receive them. These employees can serve as a control group that will make it possible to make an unbiased estimate of a personal contract’s effect on the horizon of service, as proved in the theoretical section.

In order to generate a good control group, for each employee who received a personal contract, an employee must be found whose personal characteristics match those of the personal contract employee, but who did not receive a personal contract. As proven above, this matching need not – and cannot – rely on equality in all characteristics; a similar probability forecast is sufficient. There are many methods of matching. The method used here pairs each employee who received a personal contract with one

³³ The model used is the one whose estimates are displayed in Column (1), because it includes both more detailed individual variables (salary grade order and seniority) and an excellence variable, in contrast to the model in Column (2), in which the individual variables are translated into an additional excellence variable. Furthermore, the R-squared value is more artificial. Concern about bias in the excellence variable resulting from large-scale wage agreements that increased salary in a way that does not reflect excellence was alleviated after regression with a control for the year of joining gave similar results.

³⁴ There are also observations in the group that did not switch for ranges higher than 0.23 but these do not appear in the graph because of the criteria.

employee who did not.³⁵ The nearest neighbor method was used for the pairing:³⁶ the employee in the control group whose probability forecast was the closest to that of the employee in the treatment group was selected. If an observation in the control group was found to be the most suitable for two (or more) observations in the treatment group, it was matched with both of them. The selection was made only from employees who never received treatment during their careers, i.e., never switched to a personal contract. This method ensures that each employee in the treatment group is paired with an employee from the control group.

The database includes 350 employees,³⁷ half of whom switched to personal contracts and half of whom did not. As explained above, the population of employees who switched to personal contracts (the treatment group) included both employees who switched in 2001 and those who switched in 2002. The populations of employees who did not switch to personal contracts (the control group), was selected by the above method from all employees in 2001 and 2002; the required condition was that they had not switched to a personal contract by the time they left public service, or by 2009.

Table 7 displays the averages of the explanatory variables among the employees who switched to a personal contract, all the employees who did not switch, and the control group selected by the above method. It can be seen that this pairing method created a good control group from the perspective of similarity between the averages: *t* tests show that the hypothesis that there is no difference between the two groups in the averages of the variables cannot be rejected. The balance between the groups now makes it possible to estimate the effect of obtaining a personal contract on the duration of service.

An initial illustration of a profile of the various durations of service can be seen in Figure 6, known as the Kaplan-Meier graph (Based on an article of Kaplan and Meier, 1958). This graph displays a survival function of the two groups – those who received a personal contract and those that did not. The special leaving profile of the two groups can be seen in Table 8: year 0 is the first year after the year in which the personal contract was obtained (2001 or 2002). Obviously, all those who received personal contracts remained, since this is the treatment group itself. Of the employees who did not obtain a personal contract, however, only 97.7 percent remained, meaning that 2.3 percent of the employees who were – according to their characteristics – candidates to obtain a personal contract, and did not receive it, left public administration the following year. The survival percentages were also lower among employees who did not receive a personal contract in the following year, but after that, it appears that employees who switched to a personal contract were more inclined to leave. In the eighth year (the last year observed for employees who obtained a personal contract in 2002), 68 percent of the employees without personal contracts and 56.6 percent of the employees with personal contracts remained.

³⁵ The order of the observations is important in this method, and their order was therefore determined randomly.

³⁶ This was one of the methods proposed by Deheja and Wahaba (1999) for pairing observations in the control group with corresponding observations in the treatment group.

³⁷ There were 179 employees who switched to personal contracts, but data were missing for four of them, and their propensity score was therefore not calculated.

Table 7 – Descriptive Statistics of the Treatment Group, the Control Group, and the General Populations

	(1) The Treatment Group – Switched to a Personal Contract	(2) The Control Group – Did Not Switch to a Personal Contract	(3) The General Population – Did Not Switch to a Personal Contract	(4) <i>t</i> value for Testing the Hypothesis: (1) = (2)
N	175	175	9,062	
Subject to the Civil Service Commission	0.95	0.95	0.74	0.25
Women	0.43	0.49	0.61	-1.07
Salary grade order	28.40	28.76	39.55	-0.23
Salary grade order squared	1,011	1,032	1,820	-0.2
Age	32.35	31.56	39.36	1.2
Age squared	1,093	1,026	1,645	1.32
Logarithm of the real salary	8.96	8.99	9.12	-0.93
Seniority	3.49	3.66	11.25	-0.52
Seniority squared	20.0	23.6	210.92	-0.93
Excellence variable: the average rate of change in salary since entering public service	15.51	13.33	8.33	0.38
The percentage of personal contract employees in the ministry	15.26	16.28	5.59	-0.68
The percentage of personal contract employees in the unit	21.80	23.25	2.84	-0.49
Whether the senior employee in the unit has a personal contract	0.47	0.49	0.20	-0.32

Source: Bank of Israel data.

A problem arises concerning the composition of the 2.3 percent of non-personal contract employees who left in the first year: were they not offered personal contracts, and subsequently left, or were they offered such a contract, and then refused it and left, or was there some combination of the two groups? A good control group is that composed solely of employees who were not offered contracts, since their leaving can then be attributed to the fact that they did not enter the treatment group **randomly, a fact that did not depend on them**. However, since I was unable to check this empirically, I checked the minimum proportion of the group for which the result for the first two years (that those who did not receive a personal contract in these years had a greater tendency to leave) was still valid. The calculation showed that this proportion was 20 percent: if at least 20 percent of the group of those leaving in the first two years left because they were not offered a personal contract, then the result concerning the greater tendency to leave in the first two years of those not receiving a personal contract is indeed valid.

This analysis shows that the effect of a personal contract – in the sense of retaining employees for a longer period – exists for at most two years (the year in which an employee switches to a personal contract and the following year), while in the third year and all subsequent years, employees who switched to a personal contract had a greater tendency to leave. This result will be discussed in Section 4, after this finding has been established.

Figure 6: A Kaplan-Meier Graph Analyzing the Survival Proportion of Employees in the Their Workplace

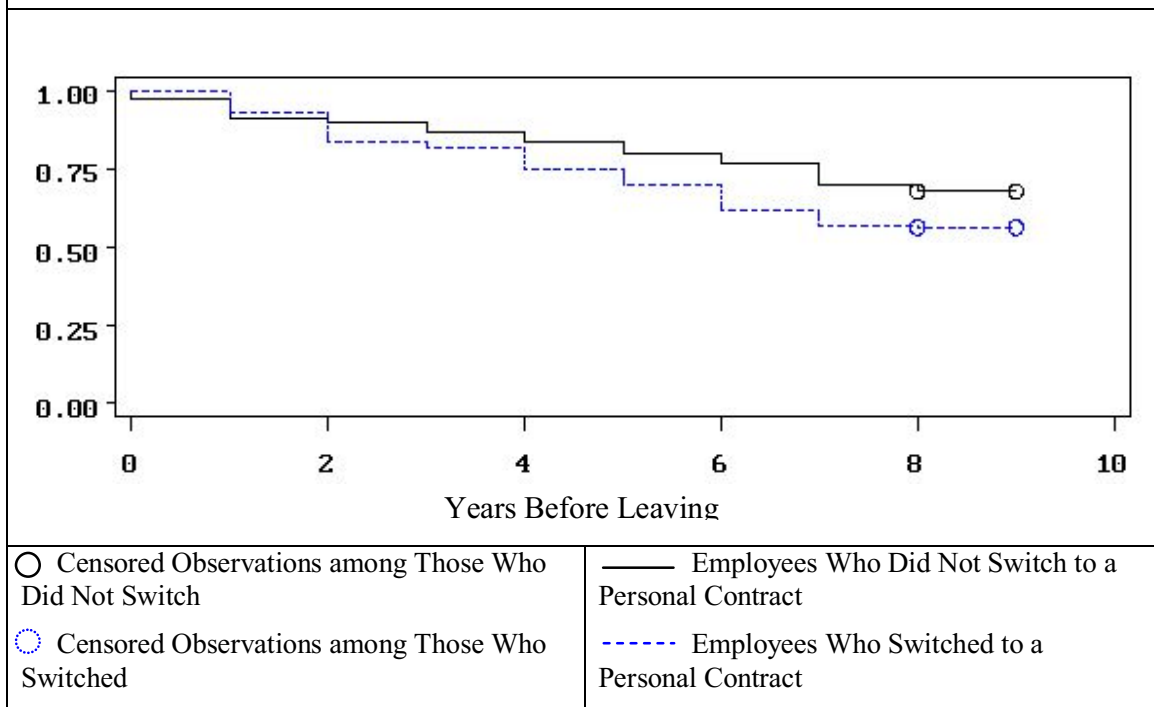


Table 8: Percentages of Employees Who Remained after a Number of Years

Year	Did Not Switch to a Personal Contract	Switched to a Personal Contract
0	0.977	1
1	0.914	0.931
2	0.897	0.840
3	0.869	0.817
4	0.834	0.749
5	0.800	0.697
6	0.766	0.617
7	0.697	0.571
8	0.680	0.566

Source: Bank of Israel data.

In order to test the reliability of this finding, the significance of the effect of obtaining a personal contract up until the second year was tested using two accepted statistical tests: the log rank test (Mantel, 1966) and the Wilcoxon test (Gehan, 1965). According to both tests, we can reject the null hypothesis at a level of significance less than 5 percent. This means that the groups differ from each other, and that those switching to a personal contract remain longer at the beginning of the period (subject to the above comment concerning the composition of those leaving). A similar test was conducted in order to

examine the difference between the groups from the second year onward, and here, too, the statistics indicate the significance of the difference, although this significance is slightly weaker (between 5 and 10 percent).

The conclusion derived from the analysis using the Kaplan-Meier method, however, is insufficient: it is based on an analysis of a single explanatory dimension — belonging to the control group or the treatment group — while the decision to leave is affected by other factors, most of which change over time. Among the variables with potential to affect the decision to leave, the literature lists salary, salary raises, age, and gender. These variables were not included in the above analysis; the analysis framework must therefore be expanded. For this purpose, I will use the Cox regression model, whose advantages were explained above, and in which time-dependent variables can be included. The explanatory variables in this model are the individual variables used in regressions on the unpaired population (the results of the estimation in Table 4), plus another variable: the logarithm of the state budget for that year (in real 2007 prices).³⁸ This variable constitutes a proxy for budgetary constraints in order to test whether budgetary constraints affect employees' decision to leave, and especially — to check whether the effect of the transition to a personal contract becomes weaker when this variable is included; weakening of the this effect likely indicates that personal contract employees will be the first to leave in a state of budgetary constraints, due to the greater flexibility in their employment. The regression results in terms of hazard ratio are displayed in Table 9.

The estimate of the main variable in this article — switching to a personal contract — yields a clear conclusion: the chances that an employee who switched to a personal contract will leave are substantially greater — by a factor of 2.78 — than those of an employee who did not switch. This result is almost double that in Column (3) of Table 4, indicating that the bias due to the selection is negative.³⁹ A separate test of the two periods reflected in Figure 6, however, shows that the picture in the graph is accurate: in the first two years, the chances that an employee who switched to a personal contract will leave are significantly lower than those of an employee who did not switch. The magnitude of the personal contract's effect is about 5 percent, meaning the chances that an employee who received a personal contract will leave are only 5 percent of the chances that his counterpart who did not receive one will leave. The third population consists of those who remained at least three years. The finding for them is also significant: the chances that an employee with a personal contract will leave are six times as great (on the average) as those of an employee who did not receive a personal contract. Figure 6 therefore provides a reliable picture of the difference in dynamic between employees from the two groups, and the regression results provide numerical meanings for this. Estimation of the model with and without the budget variable (not displayed

³⁸ An ordinary spending budget. The peak manpower in government ministries was tested as an alternative, but this estimation did not change the results.

³⁹ Although intuition says that in the absence of selection according to an employee's quality, the transition to a personal contract will have less effect (assuming that a high-quality employee is more inclined to leave), this would be true had the treatment variable (switching to a personal contract) been the only variable included in the regression. In multivariate regression, because the bias is affected not only by the connection between the omitted variable (the worker's quality), the explanatory variable (the transfer), and the result, but also by the connection between the omitted variable and the other variables, the intuitive result is not obtained.

here) does not change the results significantly, which weakens the hypothesis that the first employees to leave in the event of budgetary constraints are those with personal contracts.

Table 9 yields a number of additional conclusions concerning the factors in an employee leaving his job. First of all, it is clear that an employee's gender has a decisive effect on the chances that he will leave: the chances that women will leave are 50 percent those of men. This finding, however, is not significant in the first two years. Secondly, an employee's age and the square of his age also have a significant and nonlinear effect. The chances that an employee will leave decline with age. Starting at around age 37, however, the effect becomes positive, meaning that the chances that an employee will leave increase. This finding is also not significant in the first two years. The proxy variable for excellence, the rate of change in salary since entering public service, has a significant effect in each of the sub-populations, and in a uniform direction: more "excellent" employees are more inclined to leave.

Table 9 – Results of the Cox Model for Estimating the Factors Affecting the Probability of Leaving

	(1)	(2)	(3)	(4)
	All Employees	Up to 2 Years	3 Years or More	Only Employees Who Switched to a Personal Contract
Switched to a personal contract	2.789***	0.049**	6.549***	
Women	0.562**	0.55	0.394***	0.59*
Logarithm of salary	0.384**	0.491	0.082***	0.26**
Seniority	0.972	1.054	0.971	0.969
Salary grade order	1.005	0.983	1	1.004
Rate of change in salary since entering public service	1.006	1.039**	1.046**	1.015
Real salary upon entering public service	1	1.001	1	1
Logarithm of the State Budget (real, 2007 prices)	0.036	0	0.739	0.021
Ministry subject to the Civil Service Commission	4.677		6.92	1.883
Years in public service	0.908	1.256	1.068	1.005
Age	0.735**	0.753	0.548***	0.738
Age squared	1.004***	1.003	1.008***	1.004
Change in salary since the preceding year	1.001	1.002	1.01	1.001

Source: Bank of Israel data. The estimates are listed in terms of the hazard ratio for leaving.

* denotes a p-value less than 0.1; ** denotes a p-value less than 0.05; *** denotes a p-value less than 0.01.

Special attention should be paid to the variables connected with the employee's salary and his promotion. The salary level lessens the chances that he will leave, but the rates of change in the past year, in the years since beginning treatment, and since entering public service have no effect. The effect of the salary level is not significant in the first two years, but is significant from the third year onwards, and to a greater degree than among the general population. This finding fits in with the assertion by Jovanic (1979) that the salary level affects the decision to leave, and contradicts other studies (Topel and Ward, 1992, Munasighe, 2000), according to which salary raises affect the decision to leave, rather than the salary level. The effect of the logarithm of salary was also found to be

significant when the model is estimated exclusively for those switching to a personal contract (Column (4)). It was also found that the probability of women leaving was also lower in this sub-population, but no other variables in the model were found to have a significant effect on leaving.

To sum up this section, in which the study question was answered more precisely, it can be said that in general, personal contracts have not achieved their purpose among employees who obtained them in 2001 and 2002. No evidence was found that employees who switched to personal contracts remained longer in public service than employees corresponding to them who did not receive a personal contract; on the contrary, obtaining a personal contract increases the chances that an employee will leave. A more exact perusal reveals that this general conclusion blurs a difference in the leaving profile that nevertheless exists between the two groups: in the first two years, employees who did not switch to a personal contract tended to leave more than those who did receive one, while from the third year onward, employees with personal contracts left more than the others.⁴⁰ We will return to these results after confirming their robustness.

It is possible that the result suffers from a certain bias, since one important assumption in the methodology for estimating the effect of treatment is the absence of an indirect effect of the treatment on the control group (Heckman, et al., 1999). If we assume that at least some proportion of the control group consists of employees who were not offered a personal contract (even though they qualified for one), and in line with the claim by Brown and Sisson (1975) concerning the tendency of employees toward comparisons with each other, it is possible that the fact that a different employee similar to them obtained a personal contract, while they did not, affected their decision to leave. If this is the case, then the treatment has an indirect effect on the control group.⁴¹ According to this logic, the relative effect of a personal contract on leaving is greater than what is observed, since some of those in the control group who left did so because of an indirect effect of a personal contract, without which they would have remained. The results are therefore only a lower bound, while the true effect is even greater than what was observed.

From the unofficial perspective, in which considerations of flexibility – and not necessarily the employee's quality – are what motivate a manager to offer a personal contract, we cannot draw conclusions about the success or failure of personal contracts; we can only present the results, according to which a personal contract is correlated with a higher probability of remaining in the first two years, and with a lower probability in the subsequent years. An indication of the expected duration of service is likely to help a manager considering an investment in improving the quality of his employees through courses, training, etc.

⁴⁰ Even if the reliability of the result for the first two years is questionable (due to the composition of those leaving in the first year), this does not weaken the main result.

⁴¹ Managers in English companies reported that one of the considerations in granting an employee a personal contract was concern about a decline in motivation among employees who did not receive one (Brown et al. (1998)).

3.3) Robustness of the Results and Sensitivity Analysis

The reliability of the results can be questioned based on four main objections:

The first such objection can result from the fact that a joint analysis for men and women is not accepted in labor economics – all the more so when decisions by individuals concerning their career track are concerned - decisions that are made by men and women in different ways. As noted, the analysis is the result of a shortage of observations, and a separate analysis for each gender will yield no results because the sample is too small. A stability test for the pairing was nevertheless conducted. Since in the pairing method used each observations is paired with an observation having a similar “mark,” regardless of gender differences, pairs were also created in which the treated observation is male and the control observation is female. Although this may be a good pairing from a statistical perspective, its validity is questionable, since men should be compared with men, and women with women. An attempt was therefore made to pair women from the control group with women from the treatment group, and the same for men. The result is a sample of 350 observations of pairs, each of the same gender. While the regression on the probability of leaving was estimated for the entire sample, not separately for men and women (due to a lack of observations), it can be said that the sample is now more balanced, and represents more “genuine” pairs from a substantive perspective, not just from a statistical one. The results obtained indicate that the above conclusions are stable:⁴²

- 1) A personal contract has a positive effect on the probability of leaving;
- 2) In the first two years, a personal contract has a negative effect on the probability of leaving, at a 10 percent level of significance.
- 3) After three or more years, a personal contract has a positive effect on the probability of leaving, at a 10 percent level of significance.

A second objection is liable to come from the pairing method. In order to answer an objection of this type, I tested other pairing models, and the results were found to be robust, as described in Appendix A.

A third objection does not challenge the reliability of the results themselves, but their external validity, i.e., the ability to use this study as a basis for general conclusions about personal contracts. The limitation of many studies is their focus on a specific sample – in this case, employees who switched to a personal contract in 2001 or 2002. Are the results also valid for employees who switched to a personal contract in 2003, 2004, etc.? The empirical effort to answer this question did not yield unequivocal results, due to a lack of observations and the relatively short observed period of the duration of service. The downtrend in return on the transition to a personal contract, however, likely indicates that the incentives to stay are less than those during the study period. Given that the level of qualifications of the employees switching to a personal contract did not substantially change, it can be argued that the effect of a personal contract on the profile of those leaving is, according to this study, a lower bound. Renewed growth in the Israeli economy following the 2001-2003 recession is also likely to have a positive effect on the profile of those leaving in the years following the study period, as is – in which direction

⁴² The full results of this stability test can be obtained from the author.

is unclear – the transition from unfunded pensions to funded pensions. At the same time, these are nothing but unsupported hypotheses; an additional examination of the question focusing on the subsequent years is necessary in order to complete the picture.

A fourth objection concerns the likelihood of the CIA assumption. The use of this assumption for identification purposes and the key role it plays requires a very careful examination of its likelihood. As stated, this assumption means that the result variable and placement in the treatment or the control group are independent when control is exercised over the variables explaining the probability of inclusion in the treatment group, or over their propensity score. In other words, given control of the observed variables, there are no unobserved variables that explain the difference in placement, and which are liable to affect the result variable, and there is in any case no selection problem, and the model can be identified.

In the above model, control is exercised over individual and environmental variables that may possibly explain the chances of inclusion in the treatment group, but the possibility exists that there are unobserved variables affecting the transition to a personal contract and/or the decision to leave, and which are not taken into account (for example, good connections with the manager, the employee’s unobserved quality, love of risk). In this case, omitting them is likely to have a substantial effect on the results. Obviously, this possibility cannot be completely ruled out, but sensitivity analysis based on a simulation for the results can be conducted, as proposed by Ichino et al. (2006). The main idea behind their method is to create a new binary variable to reflect an unobserved variable and test the difference between the effect of the treatment when this variable is included in the pairing process and an analysis of the treatment effect and when no such variable is included in the analyses. Formally, if the CIA assumption is accepted, the following equation is obtained:

$$E[Y_0 | T = 1, X] = E[Y_0 | T = 0, X] = E[Y_0 | X]$$

We now assume that:

$$E[Y_0 | T = 1, X] \neq E[Y_0 | T = 0, X]$$

However:

$$E[Y_0 | T = 1, X, U] = E[Y_0 | T = 0, X, U] = E[Y_0 | X, U]$$

U is the unobserved variable, hereafter the “confounding variable” that we assume affects placement in the treatment group. The probability distribution of U is characterized through four parameters:

$$p_{ij} \equiv \Pr(U = 1 | T = i, Y = j, X) = \Pr(U = 1 | T = i, Y = j), \quad i, j \in \{0,1\}$$

where T receives the value 1 if the individual is placed in the treatment group and 0 otherwise, and Y is the result variable, also measured as 0 or 1. According to this definition, each individual receives a certain value of U , which is included in the probability model for forecasting placement in the treatment group (i.e. obtaining a personal contract) and in analysis of the factors for leaving. This process is repeated many times (here – 200 times), and the average result of the treatment effect is compared

to the original result, meaning the model without U . Significant resemblance between the results will reinforce the CIA assumption by making it possible to argue that if an unobserved variable of significance is omitted, its omission will not greatly change the result.

In order to provide a meaningful basis for the p_{ij} values, Ichino et al. suggest selecting p_{ij} values that will determine a probability distribution for U that resembles that of a different binary variable that affects the placement for treatment (the employee's gender, for example). The effect of U on the chances of placement and on the result of treatment can be estimated through the following two parameters:

$$\Lambda \equiv \frac{\frac{\Pr(T=1|U=1, X)}{\Pr(T=0|U=1, X)}}{\frac{\Pr(T=1|U=0, X)}{\Pr(T=0|U=0, X)}}, \quad \Gamma \equiv \frac{\frac{\Pr(Y=1|T=0, U=1, X)}{\Pr(Y=0|T=0, U=1, X)}}{\frac{\Pr(Y=1|T=0, U=0, X)}{\Pr(Y=0|T=0, U=0, X)}}$$

Γ measures the effect of U in terms of the odds ratio, and is obtained from a logit regression of the result variable on U and on the other variables that explain the probability of leaving. Λ measures the effect of U on selection for the treatment group in terms of the odds ratio, and is obtained from a logit regression that estimates the chances of obtaining a personal contract.

If the confounding variable constitutes a potential challenge to the results, then the inequality $p_{01} > p_{00}$ holds, (and U has a positive effect on the result variable), which requires $\Gamma > 1$, while $\Lambda > 1$ would indicate that the confounding variable has a positive influence on the chances of entering the treatment group.

One change is required in order to apply the above analysis in this study. Since the result variable is not binary, we define a dummy variable I that receives the value 1 if the result variable is greater than a certain value:

$$p_{ij} \equiv \Pr(U = 1 | T = i, I(Y > y^*) = j)$$

and in terms of this study, the effect of U on the chances that an employee will leave by a given year. Estimation is conducted through logit regression, as was done above. While the Cox model should have been preferred for the estimation, inclusion of this model in the sensitivity analysis conducted here is not trivial, and requires special research and development.

In the analysis displayed below, the probability distribution of U is determined according to two variables. One is the proportion of men, which has a positive effect on the probability of obtaining a personal contract ($\Gamma > 1$). The second is based on the salary grade order: since the salary grade order is by definition a continuous variable, the binary variable receives the value 1 if the salary grade order in the year preceding the possibility of a switch to a personal contract is less than 35 (slightly below 39, the average salary grade order in the population) and 0 otherwise. As will be displayed in the table, the probability distribution of U derived from this artificial binary variable produces the desired characteristics of U . Furthermore, four values of y^* (3-6 years) were tested. The

procedure was run 200 times for each value of y^* , and the average effect of the transition to a personal contract on the duration of service is displayed in Table 10, together with the average sizes of Λ and Γ .

Table 10: Summary of Sensitivity Analysis Findings

		y^* Values (minimum year of leaving)			
		3	4	5	6
(1)	The average effect of the treatment on the result (ATT) without the confounding variable ⁴³ (odds ratio)	1.916	1.16	1.709	2.366
(2)	The probability distribution of U , based on the proportion of men.				
(3)	The average effect of the treatment on the result (ATT) ⁴⁴ (odds ratio)	2.27	1.20	1.50	2.01
(4)	$\Pr(U=1 T=0, Y=0)$	0.39	0.38	0.38	0.38
(5)	$\Pr(U=1 T=0, Y=1)$	0.43	0.44	0.46	0.42
(6)	$\Pr(U=1 T=1, Y=0)$	0.55	0.41	0.49	0.46
(7)	$\Pr(U=1 T=1, Y=1)$	0.50	0.50	0.57	0.56
(8)	The effect of the confounding variable on the placement (Λ) (odds ratio)	1.63	1.67	1.67	1.69
(9)	The effect of the confounding variable on the result (Γ) (odds ratio)	1.32	1.43	1.62	1.41
(10)	The probability distribution of U , based on the proportion of employees with grade order less than 35.				
(11)	The average effect of the treatment on the result (ATT) (odds ratio)	2.15	1.26	1.59	2.15
(12)	$\Pr(U=1 T=0, Y=0)$	0.41	0.41	0.40	0.41
(13)	$\Pr(U=1 T=0, Y=1)$	0.44	0.44	0.42	0.43
(14)	$\Pr(U=1 T=1, Y=0)$	0.63	0.62	0.61	0.59
(15)	$\Pr(U=1 T=1, Y=1)$	0.81	0.81	0.71	0.72
(16)	The effect of the confounding variable on the placement (Λ) (odds ratio)	2.82	2.96	2.85	2.89
(17)	The effect of the confounding variable on the result (Γ) (odds ratio)	2.06	1.73	1.65	1.80

Row (1) displays the positive effect of the transition to a personal contract on the probability of leaving by year y^* . A comparison of these results with those appearing in Rows (3) and (11) shows that the differences are not particularly large, and in any case do not differ in the direction of the effect. This is true for both of the variables on which the probability distribution of U was based. For years 5 and 6, the measured effect without the confounding variable (Row (1)) is greater than that measured when the confounding

⁴³ The estimate of the treatment variable from logit regression without the confounding variable for pairs that were matched without taking the confounding variable into account.

⁴⁴ The estimate of the treatment variable from logit regression with the confounding variable for pairs that were matched while taking the confounding variable into account.

variable is included in the analysis (Rows (3) and (11)). This result is intuitive, because if the confounding variable describes an unobserved variable that is positively correlated with placement in treatment and with the result (Rows (8), (9), (16), and (17)), we would expect a positively biased estimate for the effect of the treatment if the confounding variable is omitted, which is indeed the result we obtained. At the same time, the overestimate is only 7-17 percent higher than the “true” estimate, so we conclude that the treatment has a real effect even when any unobserved effect whatsoever is deducted from it.

The result for years 3 and 4 is the opposite, indicating that the effect of the treatment measured without the confounding variable is an underestimate of the true effect – the effect measured with the confounding variable. This result is not clear, since the effects of the confounding variable on the placement and on the result are positive, and given that the treatment has a positive effect on leaving in years 3 and 4, we would have expected, as occurred in years 5 and 6, estimating the treatment effect without taking the confounding variable into account to cause underestimation, not overestimation, as in this case. Without an unequivocal explanation for this,⁴⁵ we can nevertheless state that the differences are very small for year 4, so that most of the problem lies in year 3. Even if we regard this result as a problem, it can cast doubt only on the validity of the conclusion about the short-term effect of the treatment;⁴⁶ the long-term effect of the treatment (as measured by the tests for years 5 and 6) is stable.

There is a reason why the statistical significance of the effect is not displayed here. Ichino et al. note, following Imbens (2003), among other things, that the emphasis in the test is on the **differences** between the ATT sizes in the analyses with and without the confounding variable. The absolute size and the significance are only of secondary importance. To sum up, we can say that this test give further support for this study’s main finding, because even if any variable with an impact on placement and treatment is omitted (meaning that the CIA assumption is unstable), the main result is still stable.

4) Discussion, Summary, and Conclusions

The personal contracts in public administration were created as a tool. One of the main uses of this tool was the possibility of offering good employees a higher salary in order to retain them in public administration, or at least to provide managers with managerial flexibility, without any obligation to employ a worker in the long term. It appears that despite the significant return on a transition to a personal contract in 2001-2002, the positive effect of this tool on the duration of service, if any, was short-lived – no longer than two years, after which the transition had a negative effect. In the following paragraphs, I offer a possible reason for this phenomenon.

There is a tradeoff in the transition to a personal contract – a higher salary in exchange for the loss of job security (relatively). If we assume that an individual’s revealed preferences indicate his utility function, it can be stated that the value of job security (and

⁴⁵ And without regarding the result as exceptional, since Boockmann and Hagen (2008) also reported a similar result in some of their sensitivity tests.

⁴⁶ In other words, it is possible that the period after which a personal contract begins to exert a positive influence on the probability of leaving is not two years, but three or four years.

the total other losses incurred in a transition to a personal contract) are greater than the salary increment accompanying the transfer, and that the transfer has a negative impact on total utility, causing an employee to leave his workplace. The findings of Mazar (2010) concerning good employees leaving public service for the business sector are a likely indication that an individual's considerations led him to the realization that if he conceded the main relative advantage of working in public administration – job security – it is better for him to receive an even higher salary, which the business sector is able to offer him.⁴⁷ From this perspective, it can be stated that the employee leaves when he understands that his current career track is not optimal for him (Sicherman and Galor 1990).

This perspective alone, however, does not explain an individual's initial decision to switch to a personal contract, since the tradeoff that this involves was known to him even before his transfer. Furthermore, as noted above, although *de jure* it is easier to fire a personal contract employee, *de facto* it is not so simple.

Another perspective that may provide an explanation is the attitude towards risk. If we say that the employees paired with those who obtained personal contracts are those whose characteristics make them also suitable for obtaining one, and since there is no restriction on the number of personal contracts in a unit or a ministry (other than the unit or ministry's budget constraint), it is possible that these employees were also offered a personal contract, but declined the offer because they were risk averse, meaning that they did not wish to concede their job security. It is reasonable to assume that risk aversion also affects an employee's wish to change his workplace, due to the uncertainty that this entails. In this case, the personal contract is no more than a proxy for the attitude towards risk and its influence on the duration of employment, and no causal effect can be asserted. The disadvantage of this explanation lies in the difference in the profile found for the first two years after the switch and the profile for the subsequent years; there is no reason why risk-averse employees (those who did not receive a personal contract) should be more inclined to leave in the first two years. I therefore propose another explanation, which in my opinion better accounts for the findings.

Jovanovic (1979) argues that the match between employee and employer is made according to the employee's productivity, as it is perceived *ex ante*, where productivity is the probability distribution among different workplaces. Due to incomplete information, an alternative employer interested in an employee can learn about him only through his achievements in his previous workplace. When new information is learned, a move to a new workplace becomes possible. Similarly, Bernhardt and Scoones (1993) regard a promotion as a kind of signal to other employers, following which an employee can obtain a higher salary from a different workplace. In order to prevent this transfer, the original employer must offer a higher salary (or promotion) than he planned – a kind of preemptive wage. An explanation for the effect of a personal contract on the transition can therefore be proposed from this perspective: In an inflexible salary system with salary raises not necessarily dependent on excellence, a personal contract creates a special signal that is studied by other employers. In the initial years, employees who

⁴⁷ Keep in mind that despite the higher salaries in personal contracts, the salary scale is still fixed, and its flexibility is also limited.

switched to a personal contract remain in order to acquire the signal; after a short period of working under a personal contract (empirically – about two years), they have higher chances of obtaining better work, as a result of the signal they have acquired, and the chances that they will leave are accordingly higher. On the other hand, employees who did not obtain a personal contract, i.e. the afore-mentioned signal, but who were deserving of it, see no special value in remaining at their workplace, and therefore tend to leave. After the same short period, the employment alternatives of employees who switched to a personal contract are better than those of employees who did not switch, and from a certain perspective, they also are more inclined to leave.

Following the model proposed by Bernhardt and Scoones (1993), we can say that a “preemptive salary” is a possible solution to the new problem created by the personal contract, but it cannot be stated what salary is required for it to function as a “preemptive salary.” The regression results, which indicate that salary level has a negative effect on the chances of leaving, support the likelihood that this proposal is indeed effective. At the same time, any change in the term and/or value of a personal contract should take into account the indirect effects on employees in a suitable position for obtaining a personal contract, but who did not obtain one.

All this is relevant only if the official perspective is adopted that personal contracts are granted to excellent employees in order to retain them in the system (the probability regression results support this perspective). From an unofficial standpoint, all that can be said is that according to this study, the effect of a personal contract on the duration of service is positive in the short term and negative in the long term. Is this result desirable? On the one hand, if the employee is good, his manager will have an interest in retaining him in the long term; on the other hand, it may be that there are jobs in which a long duration of service is unimportant, and that the long-term negative effect is less important than the quality of the employees who can be retained in the short term by offering them a personal contract. Deciding between these two possibilities is beyond the scope of this study. In any case, the duration of service expected for a given employee is important to his manager in deciding about an investment in the improvement of his employees’ quality through courses, training, etc. It therefore follows that the study conclusions are important even if this perspective is adopted.

This study focused on employees who switched to a personal contract in 2001 and 2002. As mentioned above, however, the significant variance in the return on the transfer to a personal contract, and, in practice, the decline in this return, are liable to detract from the attractiveness of a personal contract, compared with its power during the study period, and could have an even stronger negative impact on the effectiveness of this tool. It is therefore worthwhile monitoring personal contracts, with a later reassessment of the study question from a suitable time perspective.

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Appendix A: Robustness Testing for the Pairing Method

The pairing method selected above is only one of several possible methods. Each method has advantages and disadvantages, and it is difficult to adapt one specific method to a single study question. Since a small sample is involved, the selection of the method can affect the results obtained. I therefore tested the main results obtained using three other pairing methods (actually two methods, but with different parameters).

The first method is the same method used above, nearest neighbor (NN), but without the possibility of pairing one observation from the control group with more than one observation from the treatment group. The second method, which was tested with two different specifications, is looking for a pairing in a small interval (Caliper), and if no suitable pairing is found, the observation from the control group is not included in the database. Two intervals were checked: 0.001 and 0.0001. When the interval is smaller, the pairing is better, but a small interval is liable to leave observations with no pairing.

The main results obtained from using these pairing methods, which are displayed in Table A-1, demonstrate the robustness of the results.

Table A-1 – The Main Results from Pairing under Different Methods

	Nearest Neighbor Without Repeats	Caliper 0.001	Caliper 0.0001
Number of observations (treatment + control)	350	136	98
Are the treatment and control groups balanced?	Yes	Yes	Only for 10 of the 13 variables
Are the Kaplan-Meier graphs for the two groups similar?	Yes	Yes	Yes
Is the treatment effect significant and in a positive direction?	Yes	Yes	Yes
Is the treatment effect on employees who remained up to two years significant in a negative direction?	Yes (at a 10 percent level of significance)	Yes	Not enough observations
Is the treatment effect significant ⁺ in a positive direction for employees who remained over two years?	Yes	Yes	Yes

⁺ At a 5 percent level