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ESSAYS ON COMPENSATION STRUCTURES AND HUMAN CAPITAL MIGRATION

VOLUME I

By

Elda Pema

A DISSERTATION

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ABSTRACT

ESSAYS ON COMPENSATION STRUCTURES AND HUMAN CAPITAL MIGRATION

By

Elda Pema

This work focuses on compensation systems and migration patterns of high skilled labor. The first two chapters analyze compensation structures prevalent in internal labor markets. In particular, this part investigates tournaments and delayed-payment schemes. The third chapter looks at the impact of fiscal policies on the interstate migration of human capital. The unifying theme of these essays is human capital, its rate of return, and its dynamics in a world with a public sector.

The first chapter investigates the hypothesis that faculty promotions and compensation in institutions of higher education are determined on the basis of rank-order tournaments. A panel data set of economics professors from the Big Ten economics departments is used. Findings suggest that the performance-based ordinal ranking of economics faculty within their department has an impact on their salary. Higher wage differentials among grade levels appear to drive larger productivity differentials and increase incentives for self-promotion. Finally, evidence shows that people of higher perceived ability have lower probabilities of being promoted. This finding is interpreted as evidence of the handicapping phenomenon that arises in heterogeneous tournaments.

The second chapter investigates delayed-payment schemes in the same internal labor market characterized by high skilled employees. The findings in this chapter reject

the human capital theory explanation of steep wage-experience profiles resulting from human capital accumulation over time, in favor of the hypothesis that such profiles are a result of long-term incentive-compatible contracts.

The first two chapters indicate that mobility affects the findings on the compensation structure. The third chapter, therefore, moves away from compensation of high skilled labor and looks at the migration patterns of human capital in a world with a public sector. This part employs Census data to assess the impact of local fiscal policies on the interstate migration of human capital. The literature on human-capital investment, mobility, and local public goods provision claims that high tax rates on labor in a state will induce a migration of skilled labor among identical jurisdictions. Findings suggest that higher average income-tax and sales-tax rates in the host state negatively affect migration rates of skilled labor. With respect to public spending on education, the data show that a higher share of education spending is associated with lower migration rates of high-skilled labor.

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CHAPTER 1

INTERNAL LABOR MARKETS REVISITED: TOURNAMENTS IN ACADEMIA

I. INTRODUCTION

Under the standard theory of wage determination, wages are equal to the value of marginal product. In recent years, however, more attention has been paid to the efficiency properties of alternative compensation schemes. In particular, a number of studies have drawn attention to relative compensation systems such as tournaments. (See, *e.g.*, Lazear and Rosen, 1981; Green and Stokey, 1983; Nalebuff and Stiglitz, 1983; Malcomson, 1984; O'Keefe, Viscusi, and Zeckhauser, 1986; Rosen, 1986; Bull, Schotter, and Weigelt, 1987; Lazear, 1989.) These studies emerged, in part, as an alternative explanation of wages in settings where pay for performance is infeasible, due to asymmetric information or measurement problems. The case of academia emerges as an interesting setting where, even though performance measures are public information, the compensation structure exhibits features consistent with "internal labor markets".[1] In particular, most promotions occur within the organization and are associated with large raises that are non-commensurate with performance.[2]

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[1] See Doeringer and Piore (1971) for a thorough discussion of internal labor markets. They define an internal labor market to be "an administrative unit...within which the pricing and allocation of labor is governed by a set of administrative rules and procedures" (pp. 1-2). They later identify internal promotions and vertical and horizontal wage differentiation within the organization as some of the characteristics of internal labor markets.

[2] The data used in this study suggest average annual salary differences of about $9,000 in 1999 dollars between Assistant and Associate Professors during 1992 – 1999 (or about 15% of the mean Assistant Professor salary; the difference is significant at the 1% confidence level), and about $31,000 between Associate Professors and Professors during the same period in terms of 1999 dollars (or 46% of the average Associate Professor salary; the difference is significant at the 1% level). These differences do not take into account variations in productivity.

In this chapter, I argue that the lag involved in observing academic performance and the existence of other non-observable components warrant the use of an incentive scheme, such as a rank-order tournament. Moreover, I present evidence suggesting that the compensation patterns observed in the economics departments of Big Ten universities are at least partially consistent with schemes of this type. In particular, the empirical evidence demonstrates the importance of rank order in the compensation and promotion practices in academia. There is also evidence that larger salary spreads between grade levels are associated with increased production. Also, given that tournaments within departments would be mixed, the implications of the theory regarding handicapping of high-ability types in the early stages of the contest are observed.

II. Review of Literature

Early sociological studies employed variants of a tournament model to characterize patterns of social mobility. Turner (1960) identified two types of upward social mobility within the American and British societies: the contest and sponsored mobility. Contest mobility, which he attributed to the American society, occurs in the form of an open contest in which the prize is elite status. Sponsored mobility occurs through continuous selection for elite status from an early age. The main difference between the two is that contest mobility minimizes selection and provides opportunities that are more nearly equal, whereas sponsored mobility involves preferential treatment of higher ability types. Features of both models were later incorporated in the tournament model developed by Rosenbaum (1975, 1979). Rosenbaum's model explains careers as a

sequence of contests, in which winners are allowed to continue competing in the next rounds, and losers are dropped from the competition. Therefore, some sorting is combined with competition at each round. Contests for promotions in these studies are identical in nature with the contests described later in the economic literature on tournaments. The main objective of these sociological studies is to explain career paths conditional on early performance and speed of upward mobility. In these models, performance during the first years of work has a decisive influence on the future career path and the probability of future promotion or demotion. The faster one moves through the first stages, the better are the chances for upward career mobility in the later stages.

Economists found the tournament model to be particularly effective in explaining the fact that promotions are sometimes associated with large salary raises that cannot be explained by improvements in performance. Lazear and Rosen (1981) were among the first economists to formalize the principles of tournament theory. In their model, promotions are allocated according to performance-based contests within the firm. The promotions are associated with large increases in compensation, in excess of any increases in the winner's value of marginal product. The prize for the winner of such a contest remains economically efficient because it generates incentives for all contestants. Because prizes are based only on the relative standing of employees (rank order), there is no need for the firm to measure and monitor absolute performance. At the same time, common technological or other external shocks that affect everyone the same way are differenced out, and employees do not have to bear the risk of such variation.[3]

[3] This does not imply that tournament contracts are less risky than piece rates. Theoretical results are ambiguous when employees are risk averse. However, if there are activity-specific measurement errors and/or technology-wide shocks with a large variance that affect everyone the same way, tournaments may be preferred to piece rates by workers who are sufficiently risk-averse. The reason for this is that the

Later developments of the theory focused on comparisons of tournaments with other forms of compensation under various assumptions, such as imperfect information, risk aversion, large-variance common shocks, and non-homogeneous contestants (see, *e.g.*, Nalebuff and Stiglitz, 1983; Green and Stokey, 1983; Malcomson, 1984; Bhattacharya and Guasch, 1988; and Lazear, 1989). Other studies focused on characterizing tournaments that would yield efficient outcomes (O'Keeffe, Viscusi, and Zeckhauser, 1984).

Empirical testing of the implications of tournament theory suffered from limited testing in restricted and often non-business settings (more on this in section V). The main reason for this appears to be the lack of performance data. By definition, an internal labor market involves single organizations or organizational units, and micro data for such settings are rarely widely available. Ironically, the strength of tournament theory lies in formulating a compensation structure that can generate efficient outcomes in the absence of performance data, but, in the absence of such data, it becomes hard to test the validity of the theory itself.

Research on salaries of university professors has for the most part rested on the premise that a piece-rate compensation system is in place. A number of studies focus on attaching a dollar value to research activity.[4] Most of these studies acknowledge the fact that salary structures differ across grade levels (Assistant, Associate, and Professor), but

---

common error portion adds noise to performance measures in the piece-rate structure, whereas with tournaments, this common error is differenced out. (See Green and Stokey, 1983, for a formal discussion of common external shocks and the way they affect the comparison between piece-rates and tournaments.)

[4] See, *e.g.*, Katz (1973), Siegfried and White (1973), Tuckman and Leahey (1975), Tuckman, Gapinski, and Hagemann (1977), Hamermesh, Johnson, and Weisbrod (1982), Diamond (1986), Ragan, Warren, and Bratsberg (1999).

few of them address the issue of why such differences exist. Table 1.1 below summarizes the salary differences among grade levels in the sample.

TABLE 1.1 – SALARY DIFFERENCES AMONG GRADE LEVELS IN ECONOMICS DEPARTMENTS OF BIG TEN UNIVERSITIES

		Salary Differences Among Grade Levels			**Salary Ratios**		
		Assistants	Associates	Professors	Associate vs. Assistant	Professor vs. Associate	Professor vs. Assistant
1991-92	Average real salary (1999$)	$58,917	$65,107	$95,332	1.11	1.46	1.62
	Standard deviation	$10,378	$12,935	$25,296			
	Coefficient of variation	0.18	0.20	0.27			
	Observations	26	35	140			
1995-96	Average real salary (1999$)	$57,028	$69,296	$98,415	1.22	1.42	1.73
	Standard deviation	$10,808	$15,786	$24,270			
	Coefficient of variation	0.19	0.23	0.25			
	Observations	27	44	147			
1998-99	Average real salary (1999$)	$60,101	$69,241	$103,854	1.15	1.50	1.73
	Standard deviation	$6,957	$11,700	$26,285			
	Coefficient of variation	0.12	0.17	0.25			
	Observations	18	41	152			

The treatment of grade level as an explanatory variable in salary determination models varies from study to study. For the most part, the assumption appears to be that grade level is determined by productivity, so that including it in a salary regression in addition to performance measures would result in a misspecified model (Siegfried and White, 1973). Other empirical studies, that lack productivity data, use grade level as a

proxy for performance (see, *e.g.*, Gordon, Morton, and Braden, 1974; Monks, 2000). Another study includes performance indicators along with grade-level dummies in a salary regression, but interprets the latter as measures of the quality of accumulated experience (Koch and Chizmar, 1973). Hamermesh explains the salary differences among grade levels during the 1970s and 1980s as motivators for continuing to publish beyond tenure (Hamermesh, 1988). The focus of his analysis is on the conflict between external market forces that tend to increase entry-level salaries, and senior employee morale, which is assumed to suffer from decreasing wage inequality.[5]

Despite the fact that the literature on academic salaries is quite extensive, it does not entirely succeed in presenting a complete picture of the compensation structure in academia. In particular, the assumption of a piece-rate compensation system is rarely questioned or tested. Few studies ascribe a theoretical model to the structure of earnings in academia. Hansen, Weisbrod, and Strauss (1978) depart from the standard linear model of salaries and incorporate the simultaneity involved in the determination of productivity, salaries, and job quality in their wage model. Gomez-Mejia and Balkin (1992) point out the agency issues in the academic labor market. According to this study, since most faculty members engage in very specialized research, and since their time is largely unstructured, there may be incentives to shirk, or to engage in other activities such as consulting. Tying compensation to publications in top journals and individual citations represents a way to resolve the agency conflict. For the most part, however, this

[5] Hamermesh argues that as universities strive to hire the best young faculty, especially in expanding fields, they do not allow such expansion to bid up the salaries of senior faculty. The latter have non-economic incentives to stay employed (economic rents), associated with living and working in a familiar environment. Administrators capture part of these rents by not paying senior faculty more than what is necessary to retain them, and use the savings to attract new faculty.

literature has focused on estimating more precisely the weights of each individual component of salary, assuming a linear, piece-rate model.[6]

The academic labor market displays clear characteristics of an internal labor market, with internal promotions and considerable pay differentials among grade levels. Recent developments in the labor-economics literature suggest that a tournament-type compensation structure may be an appropriate analytical tool whenever internal promotions are involved. In addition, the salary differences among ranks are very similar to the prizes discussed in the tournament literature. A number of studies of tournaments have suggested that salaries and promotions in academia may resemble rank-order contests, with prizes rising more than proportionate to rank.[7] For the most part, however, information about this market is public and easily accessible.[8] This is an important difference between the academic labor market and most other internal labor markets. Testing the implications of tournament theory in the academic context, therefore, would supplement the evidence on the theoretical implications of this theory, while adding to the understanding of the structure of compensation in the academia.

III. Why Tournaments in Academia?

Just as there are reasons to believe that academic salaries follow a tournament compensation structure, there are reasons to suspect that this may not be the case. Table

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[6] Most studies have focused on diminishing measurement errors by employing increasingly better and bigger data sets.

[7] Bull, Schotter, and Weigelt (1987), Ehrenberg and Bognanno (1990), and Lazear (1995, p. 106), when discussing settings where tournaments are likely to be observed, refer to academic salaries as an example of tournament compensation.

[8] Public universities are required to disclose salary data. In addition, a large number of studies have utilized data from private institutions or otherwise confidential data, suggesting that such information is accessible to a considerable degree. Also, publication data and individual reputation in the market are public information.

1.2 below summarizes the reasons why tournament compensation schemes may or may not explain the compensation structure in academia. What follows is a brief discussion of each argument.

TABLE 1.2 – ARGUMENTS ON TOURNAMENT SCHEMES IN ACADEMIA

Arguments supporting tournament schemes in academia	**Arguments contradicting tournament schemes in academia**
The academic market is an internal labor market, with most hiring occurring at the lowest level and most promotions occurring within the department;	Most departments are characterized by a flat organizational structure, which limits the incentive-generating power of tournaments;
Salary differences between the grade levels cannot be entirely explained in terms of performance differences;	The number of winners is not fixed in advance, and more than one person can win at a time;
Performance is observed with a lag, hence an incentive scheme is necessary to avoid shirking in the short run;	Performance is measurable and data are public information;
There are components of performance that cannot be quantified or measured;	There is no common noise involved in the research production process;
Ordinal rankings are less costly to construct than cardinal rankings.	Researchers can be ranked cardinally as well as ordinally.

One reason why a tournament scheme might be expected in an academic market is that most academic hiring occurs at the lowest level, and most promotions take place within the departmental units. This feature qualifies the academic market as an internal labor market, where incentive schemes, such as tournaments, are likely to emerge. Internal promotions (followed by wage raises) would create an incentive-generating potential for the reward structure, which in turn could be used to reduce shirking and resolve other asymmetric information problems.

The fact that academic performance is potentially observable does not eliminate the incentives to shirk. This is because research performance is observed with a lag.

This lag is considerable and potentially increasing in the quality of research.[9] According to Ellison (2001), the mean time lag between submission and acceptance in top economic journals was 13-28 months as of 1999. In general interest journals, this lag was from 13-19 months, compared with 10-26 months in field journals.[10] Another time lag is involved between the acceptance of an article and its publication. According to Yohe (1980), this time lag ranges between 2- 24 months. Therefore, it is reasonable to assume that it takes on average between 3-4 years to observe research output.[11] A third lag is involved in observing the impact and quality of published research. According to estimates from Lovell (1973), for half of the authors under consideration it takes about 6-7 years between the time their work is published and the time it is cited for the first time. Given that review and publication lags have increased from 20-119% since the 1970s (Yohe, 1980), and the trend has been unchanged over the 1990s (Ellison, 2001), the above lag between publication and their first citation has probably increased since Lovell's study. In light of these statistics, it would be reasonable to assume that there is a time lag of at least ten years dividing research effort from the acknowledgment of its true value and impact on the academic community.

Since output (publications) rather than input (effort) is observed, and since it may take a long time for effort to turn into observable and measurable output, there are considerable incentives to shirk in the short run. These characteristics of academic work

---

[9] The lag most likely differs in importance across academic fields. The evidence that follows applies only to economics.

[10] These data are collected only for articles that eventually get published. The time lag may be much larger for articles that do not get published in their first submission. Also, the data do not say anything about how many times a paper is rejected by a journal, before it is submitted to the journal where it is eventually accepted.

[11] For details on the refereeing process and the determinants of the length of time it takes to review articles, see Hamermesh (1994) and Chong (2001).

would warrant the use of an incentive scheme to motivate people to exert optimal effort during the period when compensation is received while performance is still not observable. A tournament scheme would accomplish this through promotion contests. People would be motivated to put forth optimal effort, because they would want to win such contests. Absolute performance data would not have to be available, because only the ordinal ranking of researchers would be used to determine the winners of promotion contests.

Another reason why rank-order tournaments might be used in academia is that, even though a major part of performance can be measured, there still remain a number of non-measurable components which need to be rewarded. Participation in seminars, committees, advising of students, and other such duties are considered part of the job, but quantifying them for compensation purposes is difficult. Furthermore, given the nature of academic work, such performance aspects would need to be assessed along qualitative dimensions for compensation reasons. For example, the number of graduate students that a professor advises, or the time spent in dissertation committee work, may not be an accurate measure of this aspect of performance. The quality of the comments and extent of support would better characterize this performance component, but such elements cannot easily be observed, let alone quantified.

On the other hand, there are also a number of reasons why a tournament scheme would *not* be expected to emerge in the academic market. A major argument against tournaments is that academic performance data are available and public information. Consequently, for compensation purposes, it is not clear why a tournament using an ordinal ranking (based on relative performance) would be preferred to a payment

structure that employs a cardinal scale (based on absolute performance), since both can be constructed. The issue of the lag involved in observing performance and the shirking incentives this may create could very well be resolved by employing a deferred payment scheme. Such a scheme would base current salary on the past stock of publications. In that case, employees would be motivated by future rewards for today's effort and would not shirk at the present.

Another argument against tournament compensation schemes in academia is that the potential for generating incentives through contests in an academic setting is very limited. The relatively flat organizational structure of a university department does not provide many hierarchical steps to generate the right incentives perpetually. In fact, the first grade level (Assistant Professor) is not earned through a contest within the organization, but rather is granted upon hiring. Most studies of tournaments are based on models involving highly hierarchical organizational structures, with increasingly fewer people at the top ladders. In academia, most people are positioned in the highest ladder (Professor), and the number of winners from one stage to the other is not fixed and most often bigger than one (unlike, for example, the case of CEO's or champion athletes). In contrast to most organizational structures that display a concentration of rewards at the top, rewards in academia are more evenly distributed.[12] Furthermore, after tenure and the last possible promotion (to Professor), standard tournament theory has little to offer in explaining why faculty have any incentives at all to engage in academic research.

[12] Compared to tennis tournaments where the top 4 ranks receive more than 50% of the total prize (as mentioned in Rosen, 1986), the top 4 most highly paid faculty in this sample receive about 25% of the total salary budget.

In addition to the above arguments, tournaments may appear superfluous in an academic market, since there is little industry-wide noise in the research production process that needs to be differenced out. Most noise involved in assessing performance occurs at the individual level. However, one could think of shocks to individual universities or departments that would affect the performance of every faculty member the same way. Examples would be budgetary shocks that result in a reduction of research funding, or a decline in the quality of enrolled students.

Ultimately, the question of whether tournament schemes can explain compensation structures in academia remains to be answered empirically. This chapter will present findings that support some of the implications of tournament theory. After considering all of the above arguments, there are two important reasons why a tournament structure may be preferred as an incentive scheme in an academic market:

1. The costs involved in constructing cardinal rankings based on observable performance exceed those involved in constructing ordinal ones. The time and resource requirements involved in collecting detailed information on all publications, their quality, and the number of individual citations over every employee's academic life or a portion thereof, would render a simple ordinal ranking of employees based on their professional reputation much more attractive.[13] Therefore, in an academic setting, ordinal rankings have an unambiguous cost advantage over cardinal rankings.

[13] As a matter of fact, even though all data used in this study were public information, it took over a year of time and extensive help from colleagues to collect.

2. The previous point assumes that absolute performance data are readily available. In reality, however, it is not clear whether truly absolute measures of performance can be gathered. As a matter of fact, only the *best* publications appear in professional journals, and only the *best* journals and researchers are cited. Therefore, the available productivity measures (publications and citations) may simply be measures of performance relative to other people in the profession, rather than absolute measures of individual performance.

A subtler issue arises with the data requirements for testing the tournament theory. While compensation may be allocated based on the ordinal ranking of faculty, at the time the salary decisions are made, no data are available to prove that this is indeed the case. The same problem would arise in most other settings where a tournament compensation scheme is hypothesized. Ideally, one would need to observe how the administrators in charge of compensation decisions rank their employees. A complete ordinal ranking of all employees may even be impossible, though pair-wise comparisons should be feasible in most cases.[14] This study will use the reputation of the researcher in the market at the time of the salary decision (measured by the total number of citations received) as a proxy for the reputation they enjoy within the department. The number of citations received will be used to rank researchers within each department. This ranking will proxy for the ranking that is made during the process of salary decision-making. In the absence of other data, this method would be the closest approximation to the actual ranking process that may take place. The contradiction of this method rests in the use of absolute performance data to construct ordinal scales. The inability to obtain

[14] For people positioned at the extremes of the performance distribution, the ordinal rank would be easier to deduce. However, it may be very hard to rank people in the middle.

performance data is at the heart of tournament theory. Earlier it was argued that lack of absolute performance data at the time of the salary determination would necessitate a tournament structure. While using such data may be an imperfect method to test the implications of the theory, this may be the only feasible method available to empirical researchers.

IV. Local vs. Global Tournaments

If tournaments are a feature of academic compensation schemes, an issue to be considered concerns their scale—are people ranked at the department level or are they ranked against everyone in the profession for compensation purposes? Robert Frank (1985) suggests that our inherent concern with rank is more relevant to local comparisons, rather than global ones. Most people care about their standing as compared to their immediate associates, or people with whom they need to compete directly for important resources. If this is the case, then local tournaments (at the department level) would be most effective at resolving the motivation issues.[15]

Another reason why local tournaments may be more relevant in this setting is that extensive sorting takes place in the academic market at the hiring stage, with better researchers being hired by more reputable departments. Given such sorting, a global standard of comparison would be infeasible for departments at the extremes of the quality distribution—lower-quality departments would be setting unrealistic standards, whereas higher-quality departments would be setting unchallenging standards, thus negating the incentive-generating power of such comparisons.

Given the considerable level of mobility in academia, it is worth asking why more productive researchers would choose to compete in the higher-level tournaments in better departments. After all, they could choose to move to less reputable departments to improve their rank and, consequently, their compensation. According to Frank (1985), mobility with the intent of improving one's local rank is limited by certain costs. In particular, an individual who moves from a higher-ranked department to a lower-ranked department would lose the extra utility that goes with working at a more prestigious and resourceful department, as well as potentially larger positive externalities generated by more able colleagues. Both of these could offset the salary increment.[16] As a result, the original sorting of researchers into departments (and hence, the homogeneity of department-level tournaments) is likely to be preserved, and few, if any, would move solely to improve their local status.[17]

Lazear and Rosen (1981) argue that heterogeneous tournaments (where people of substantially different abilities compete) are not efficient in generating the necessary incentives. This is because, in such settings, all contestants have incentives to reduce their effort. High-ability contestants have better chances to win and low-ability contestants have better chances to lose, so they all exert sub-optimal levels of effort. Lazear and Rosen suggest that, if worker types can be identified, a handicapping system may be appropriate in such situations. Such a system would give the low-ability types a head start, level the playing field, and help restore the efficient level of incentives.

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[15] The tournaments would not be fully local, because top schools on average offer higher rewards than less reputable institutions.

[16] Hansen, Weisbrod, and Strauss (1978) provide evidence of such a trade-off. According to their estimates, economists employed at more prestigious institutions earn about 12% less than economists with the same qualifications who are employed in less prominent departments.

The initial sorting of researchers into departments would help to homogenize the pool of employees working in individual departments. The fact remains that, even after some initial sorting, not all researchers within a department are identical. Therefore, in order to ensure efficiency of department-level contests, it would be necessary to place all contestants on an approximately equal footing. Since the graduating institution and the quality of the dissertation can reasonably serve as an indicator for the type of worker, it is potentially feasible to use a handicapping system, under which those from less-prestigious institutions are provided with some initial offsetting advantages. Whether handicapping actually takes place will be answered empirically later in the chapter.

The institution of tenure in academia merits some attention. Carmichael (1988) argues that tenure is offered to ensure that the best faculty members are hired. He argues that in the absence of such job security, already employed faculty would not risk hiring members with a higher ability than their own, for fear of losing their jobs. Regardless of the reasons why tenure has become a practice, its incentive–generating power needs to be addressed. Arguably, tenure is the advancement that would be most effective in generating the incentives, rather than promotion to the next grade level. However, for the purpose of this study, the promotion from Assistant to Associate Professor provides a good proxy for the incentives generated by receiving tenure, since most Associate Professors in the sample are tenured faculty. At the junior level, tenure would completely resolve the motivation issues. Other incentives to reduce shirking would not be necessary at this stage. As a result, I expect standard promotion tournaments (promotion to a different grade level followed by a higher average salary) to be most effective and

[17] Such mobility would spoil the homogeneity of the host department and would ruin the efficiency

evident at this stage. Since tenure carries with it increased job security and a continuous and rising earnings stream to be received until the time of retirement, I do not expect the immediate wage raise following this promotion to be very large. Tenure, in a sense, becomes part of the compensation that follows the promotion from Assistant to Associate Professor.[18]

Beyond the junior level, the flat organizational structure of a university department provides few promotion opportunities. Consequently, promotions *per se* will have very limited power as a tool for generating the necessary incentives. After the promotion to Professor, a different type of tournament scheme, which allocates *wage raises* based on rank order (but is not associated with the award of new titles or grade levels), would resolve the motivation issues. In the absence of additional promotion steps, periodical salary increases (similar to bonuses) would achieve the same goals that are achieved by a standard tournament scheme. After all, a key ingredient of a tournament structure is the salary raise ensuing a promotion, not the new title alone. According to Lazear (1995, p.80) such within-job wage growth is an alternative to creating additional promotion steps. In light of this discussion, I expect to find salary increments to be at least partially dependent on rank order at the senior stage.

The empirical support notwithstanding, tournaments will fall short of entirely explaining the compensation structure in academia. Even if all academic departments were to base compensation at least partially on ordinal rank, they are bound to do so to varying degrees. The reason for this, is that the weight that research activity receives

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properties of any compensation tournaments occurring in those departments.

[18] Evidence from Ehrenberg, Pieper, and Willis (1995) supports this argument. They find that departments with smaller tenure probabilities offer higher monetary compensation for the same qualifications.

relative to other performance components depends largely on departmental preferences. Finally, a number of other factors, such as experience, field, market considerations at the time of hiring, and luck, are undoubtedly important in explaining academic wages. As a result, ordinal rank is expected to influence academic salaries only to a certain extent.

V. Data and Methodology

Empirical testing of tournament theory has provided at least some support for most of the implications of the theory. Bull, Schotter, and Weigelt (1987) test the main implications of the theory in an experimental setting. They find more support for the tournament model in symmetric tournaments (with equal players) than in uneven tournaments. Ehrenberg and Bognanno (1990) find that higher total prize levels in Professional Golf Tournaments elicit increased effort from the players. Knoeber and Thurman (1994) find that a tournament-type contract in the case of chicken ranchers is effective in differencing out common shocks, such as weather.[19] They also distinguish between tournaments and other relative compensation schemes, by pointing out that tournaments are based on ordinal rankings, whereas other relative schemes depend on variations from a performance mean.

The strength of the results of these studies lies in the use of performance data. With such data the theory can be tested more accurately. However, the theory was designed primarily for settings where observing absolute performance would be costly or

[19] Inclement weather has a negative impact on the process of broiler production. Therefore, if compensation of chicken ranchers is based on rank order, the effect of weather will be differenced out, and the risk involved in raising chicken will be reduced.

even impossible. This could be one of the reasons why, in the above studies, the theoretical implications are only tested in a very restricted group of settings.

More general testing of tournament theory involved the use of data on executive salaries and promotions. The earliest studies in this strand of the literature did not test the theory itself. Instead, they emphasized the fact that relative performance mattered (Antle and Smith, 1986; Gibbons and Murphy, 1990). Later studies looked more closely at the theoretical implications of the tournament model.

One testable implication of tournament theory involves the relationship between prizes and the chances of winning. If a tournament compensation system is in place, lower probabilities of winning should be associated with higher prizes. However, O'Reilly, Main, and Crystal (1988) fail to find CEO salaries to be positively correlated with the number of vice presidents (contestants for the CEO position). Another implication of the theory involves the size of the wage spread between winners and losers. On one hand, a large wage dispersion may be required to generate the necessary incentives. On the other hand, excessively wide gaps in salaries may intensify competition, and result in self-promotion, non-cooperative behavior, or even sabotage. Main, O'Reilly, and Wade (1993) look at CEO compensation as related to the compensation of vice-presidents, in an attempt to reconcile the seemingly contradictory implications of the theory with respect to the size of salary gaps. Eriksson (1999) uses data on Danish executives to find support for most of the theoretical implications. In particular, he finds that a larger number of contestants is associated with larger wage spreads, and that companies with larger wage gaps between hierarchical levels perform better. According to Rosen (1986), higher level tournaments should be associated with

higher rewards. Bognanno (2001) finds evidence of prizes going up with rank (higher prizes at higher level tournaments) for the case of firm executives.

None of the above studies employs any direct measures of executive performance. This may be one reason why the evidence in these studies is often mixed. However, the theory was motivated by compensation practices in highly hierarchical business organizations where direct performance data were not available. Therefore testing tournament theory in such settings makes more sense.

From the first strand of literature, this study borrows the method of using individual performance data to test the tournament model. However, the setting in which the theory is tested here is arguably more general than the settings in which such analysis has taken place before. University departments are similar to business organizations, in that they make use of internal promotions and wage raises to motivate employees and reduce incentives to shirk. In a certain sense, the academic market may be an ideal setting for testing the tournament model. The availability of performance data allows for accurate testing of the theoretical implications, whereas the setting allows for a sensible generalization of the empirical evidence.

A. THE DATA

This study employs a panel dataset consisting of observations from full-time economics faculty in nine large midwestern universities[20] for the 1991-92, 1995-96, and 1998-99 academic years. These public universities and their economics departments are

similar in many ways, in addition to being located in the same geographical region. If we add to that the high degree of similarity among professors (they have approximately the same amount of education and similar experiences), the differences in compensation and promotion can more sensibly be attributed to our controls. At a minimum, holding many of those fixed mitigates the bias that may arise from omission of important variables. Tables 1.3, 1.4, and 1.5, provide a description of the variables and summary statistics.

B. THE VARIABLES

Publications

Data on publications were collected using a combination of faculty *vitas* and the EconLit database. This database provides a comprehensive list of publications since 1969, and gives full credit to each author for co-authored articles. Publications are measured as number of pages per author over the entire academic life of the researcher. Following the literature, they are divided into two categories:

- *Articles in journals (TOTARTPG).* Articles appearing in all academic journals in the Econlit database, including regional, national, international, and specialty journals. Comments and replies are also included; reviews and corrections are not.
- *Other publications (TOTOTHER).* These include books, textbooks, edited books, articles in collective volumes, book chapters, book reviews, government documents, conference proceedings, and working papers.

The two measures above are combined into one variable to control for the effect of volume of research on salaries (VOLUME).

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[20] Indiana University -Bloomington, University of Illinois at Urbana-Champaign, University of Iowa, Michigan State University, The Ohio State University, Purdue University, University of Michigan,

The nature of academic research requires that some quality measure be incorporated into the above measures of publication volume. Therefore, a publication index was constructed (PUBLINDX) that weighs the article pages (TOTARTPG) by a quality indicator. Unfortunately, data availability does not allow for a quality adjustment for the two other research variables (other publications, and non-ranked publications). For the quality measure, data on professional journals provided by the Journal Citation Reports were used. All journals included in the Social Sciences Citation Index and the Sciences Citation Index were ranked (about 5,000 journals). The Journal Citation Reports (JCR) reports two important measures that can be used to assess the relative quality of the journals. One is the total number of citations a journal has received since it was first published. This measure, while providing a good idea of the quality of the journal, tends to favor older journals or journals that are issued more frequently than others. Another measure that JCR reports is the "two-year impact factor", which measures the frequency with which the "average article" in a journal has been cited in a particular year.[21] The impact factor takes care of publishing differences among journals and, as such, provides a better standard of comparison for journals in the same field. In particular, it tends to discount the advantage of large journals over small ones, of frequently issued journals over less frequently issued ones, and of older journals over newer ones. The absolute citation counts would favor larger, older, and more frequently issued journals, whereas the impact factor corrects for this bias. Table 1.4 shows how a number of journals are ranked based on both the citation counts (left column) and the impact factor (right column) as of 1998.

University of Minnesota, and University of Wisconsin-Madison.

The absolute citation counts and the impact factor are used to incorporate the quality dimension to the publication measure. In both cases the number of article pages per person (for article *n*) is multiplied by the weight of the journal where the article was published (*w*), and summed over all publications in all journals (*j*) to obtain the index:[22]

$$\sum_{n=1}^{N}\sum_{j=1}^{J}\frac{(article\ pages)_n}{authors} * \frac{W_j}{100}$$

where:

n = 1, 2,..., N (published articles)
j = 1, 2, ..., J (journal where article was published)
W = weight of journal

The parameter W_j takes two different values. One equals the ratio of the total citations of the journal where the article is published to the number of citations of the highest ranked journal (in this case, the *American Journal of Public Health*).[23] The other value of weight is equal to the impact factor appearing on the right column of Table 1.4.

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[21] The impact factor is calculated by dividing the number of current citations to articles published in the two previous years by the total number of articles published in the two previous years.

[22] This index takes a different value for each person at each time period, but *i* and *t* subscripts were omitted for ease of presentation. The actual variable used, therefore, is INDEX $_{i,\,t}$.

[23] When only economic journals were considered, the total number of citations received by the *American Economic Review* was used as the denominator to derive W_j. The empirical analysis was not sensitive to such changes.

The two different publication indexes thus constructed appear in Table 1.3 as the citation-based publication index ($PUBLINDX^{cr}$), and the impact-factor publication index ($PUBLINDX^{if}$).

TABLE 1.3 – SUMMARY STATISTICS

VARIABLE NAME	VARIABLE DESCRIPTION	YEAR	N	MEAN	STD. DEV.
SALARY (indexed for 1999 dollars)	Academic year salary received from university sources	1992	202	$85,350	$26,184
		1995	218	$87,412	$26,891
		1999	211	$93,395	$28,546
$PUBLINDX^{if}$ (based on impact factor)	Publication Index with W_j = impact factor (see Table 1.4)	1992	243	126.72	124.08
		1995	243	151.67	141.49
		1999	243	184.64	160.27
$PUBLINDX^{cr}$ (based on citations)	Publication Index with W_j = citation-based weight (see Table 1.4)	1992	243	18.95	20.36
		1995	243	21.74	21.62
		1999	243	25.65	24.06
TOTARTPG	Total published article pages per author	1992	243	131.48	125.70
		1995	243	160.58	145.85
		1999	243	199.56	166.74
TOTOTHER	Total pages per author of other publications	1992	243	179.75	394.40
		1995	232	232.22	545.66
		1999	233	310.27	768.14
VOLUME	Volume of research = TOTARTPG + TOTOTHER	1992	243	311.23	478.38
		1995	232	391.53	642.50
		1999	233	509.47	873.28
EXPER	Experience measured as years since receiving the Ph.D. degree.	1992	226	16.36	9.81
		1995	237	18.51	10.33
		1999	240	22.27	10.50
SELFCITES	Number of self-citations	1992	243	11.49	16.71
		1995	243	14.53	20.60
		1999	243	16.75	24.39
OTHCITES	Number of citations from others	1992	243	216.21	318.57
		1995	243	248.67	345.08
		1999	243	269.05	362.42
SRANK	Simple ordinal rank within department based on citations from others	1992	243	15.09	9.17
		1995	243	15.09	9.16
		1999	243	15.12	9.13
RANK (weighted)	Ordinal rank weighted by the inverse of the total number of faculty in the department by year	1992	243	0.53	0.30
		1995	243	0.52	0.29
		1999	243	0.52	0.29
DISTANCE	Distance (in terms of number of cites) between consecutive ranks	1992	243	91.22	304.98
		1995	243	97.44	317.70
		1999	243	99.75	325.07

TABLE 1.3 (CONT'D)

VARIABLE NAME	VARIABLE DESCRIPTION	YEAR	N	PERCENT/ MEAN	STANDARD DEVIATION
ASSISTANT	=1 if Assistant Professor	1992	36	15.86	—
		1995	30	12.61	—
		1999	18	7.56	—
ASSOCIATE	=1 if Associate Professor	1992	41	18.06	—
		1995	45	18.91	—
		1999	42	17.65	—
PROFESSOR	= 1 if Professor	1992	150	66.08	—
		1995	163	68.49	—
		1999	178	74.79	—
TOP10	=1 if Ph.D. was received from any of the departments ranked in the top ten	1992	131	45.38	—
		1995	126	45.37	—
		1999	127	46.05	—
CHAIR	=1 if chairperson	1992	5	2.06	—
		1995	6	2.47	—
		1999	9	3.70	—
ADMIN	=1 if administrative duties other than chair of department	1992	28	11.52	—
		1995	29	11.93	—
		1999	30	12.35	—
AWARD	=1 if teaching award	1992	27	11.11	—
		1995	26	10.70	—
		1999	30	12.35	—
$\Delta(PUBLINDX^{if})$	Change in publication index (based on impact factor)	'92-'95	243	24.95	41.72
		'95-'99	243	32.97	47.84
$\Delta(PUBLINDX^{cr})$	Change in publication index (based on journal citations)	'92-'95	243	2.80	4.75
		'95-'99	243	3.91	6.55
Δ(VOLUME)	Change in the volume of publications	'92-'95	232	90.09	200.78
		'95-'99	224	104.86	255.42
Δ(OTHCITES)	Change in the number of citations from others	'92-'95	243	32.46	55.74
		'95-'99	243	20.38	36.22
SFLRANK (unweighted)	Simple rank order based on the change in citation counts	'92-'95	243	14.77	9.02
		'95-'99	243	14.77	9.02
FLRANK (weighted)	Rank order based on the change in citation counts, weighted by the inverse of the total number of faculty in the department by year	'92-'95	243	0.55	0.31
		'95-'99	243	0.56	0.31
FLDISTANCE	Distance (in terms of number of cites) between consecutive flow ranks	'92-'95	243	12.04	42.58
		'95-'99	243	7.82	26.90

TABLE 1.3 (CONT'D)

VARIABLE NAME	VARIABLE DESCRIPTION	PERIOD	N	PROPORTION
FEMALE	=1 if female	overall	25	0.10
UNIVERSITY OF ILLINOIS - URBANA	=1 if faculty from University of Illinois	overall	33	0.14
INDIANA UNIVERSITY - BLOOMINGTON	=1 if faculty from Indiana University	overall	19	0.08
UNIVERSITY OF IOWA	=1 if faculty from University of Iowa	overall	25	0.10
THE OHIO STATE UNIVERSITY	=1 if faculty from the Ohio State University	overall	30	0.12
PURDUE UNIVERSITY	=1 if faculty from Purdue University	overall	21	0.09
MICHIGAN STATE UNIVERSITY	=1 if faculty from Michigan State University	overall	36	0.15
UNIVERSITY OF MICHIGAN	=1 if faculty from University of Michigan	overall	36	0.15
UNIVERSITY OF MINNESOTA	=1 if faculty from University of Minnesota	overall	23	0.09
UNIVERSITY OF WISCONSIN-MADISON	=1 if faculty from University of Wisconsin	overall	20	0.08
THEORY	=1 if field of specialization is theoretical economics	overall	26	0.11
ECONOMETRICS	=1 if field of specialization is econometrics	overall	21	0.09
MACRO/ FINANCE	=1 if field of specialization is macroeconomics/finance	overall	37	0.15
PUBLIC/ URBAN	=1 if field of specialization is public/urban economics	overall	31	0.13
LABOR	=1 if field of specialization is labor economics	overall	18	0.09
OTHER	=1 if field of specialization is other than the above (includes I/O, development, experimental, agricultural, economic thought etc.)	overall	10	0.43

TABLE 1.4- RANKING OF ECONOMIC JOURNALS

	Journal Name	Cites		Journal Name	Impact
1	American Journal of Public Health	13,400	1	Journal of Economic Literature	6.41
2	Journal of the American Statistical Association	9,468	2	American Journal of Public Health	3.58
3	American Economic Review	8,999	3	Quarterly Journal of Economics	3.54
4	Econometrica	7,943	4	Journal of Economic Perspectives	3.09
5	Journal of Political Economy	6,697	5	Journal of Political Economy	2.61
6	Biometrica	5,497	6	American Political Science Review	2.28
7	Quarterly Journal of Economics	4,138	7	Health Economics	2.16
8	Journal of Finance	3,791	8	Journal of Finance	2.14
9	American Political Science Review	3,252	9	Journal of Health Economics	2.13
10	Journal of Consumer Research	2,762	10	Econometrica	2.07
11	Journal of Financial Economics	2,676	11	American Economic Review	1.98
12	Economic Journal	2,540	12	Journal of Consumer Research	1.83
13	Journal of Economic Theory	2,514	13	Journal of Financial Economics	1.77
14	Journal of Econometrics	2,479	14	World Bank Economics Review	1.73
15	Review of Economic Studies	2,411	15	Journal of the American Statistical Association	1.44
16	Review of Economics and Statistics	2,331	16	Journal of Productivity Analysis	1.38
17	Journal of Monetary Economics	1,860	17	Journal of Law and Economics	1.34
18	American Journal of Agricultural Economics	1,812	18	Review of Economic Studies	1.33
19	Journal of Economic Perspectives	1,583	19	Economic Journal	1.31
20	Journal of Law and Economics	1,580	20	Industrial Relations	1.27
21	Journal of Economic Literature	1,530	21	Industrial and Labor Relations Review	1.26
22	World Development	1,408	22	Journal of Econometrics	1.19
23	European Economic Review	1,243	23	Journal of International Economics	1.19
24	Journal of Human Resources	1,113	24	Journal of Business	1.18

TABLE 1.4 (CONT'D)

25	International Economic Review	1,113	25	Economic History Review	1.13
26	Journal of Business	1,083	26	Journal of Business and Economics Statistics	1.12
27	Rand Journal of Economics	1,039	27	Journal of Money Credit and Banking	1.12
28	Journal of Business and Economics Statistics	988	28	Review of Economics and Statistics	1.10
29	Industrial and Labor Relations Review	983	29	Journal of Human Resources	1.10
30	Journal of Health Economics	957	30	Urban Studies	1.06
31	Economic Letters	930	31	Journal of Monetary Economics	1.04
32	Public Choice	871	32	Journal of Institutional and Theoretical Economics	1.03
33	Journal of International Economics	838	33	Review of Financial Studies	1.01
34	Urban Studies	836	34	Biometrica	0.94
35	Journal of Money Credit and Banking	834	35	Journal of Urban Economics	0.88
36	Economica	825	36	Rand Journal of Economics	0.88
37	Journal of Urban Economics	787	37	Journal of Policy Analysis and Management	0.87
38	Journal of Economic History	737	38	Journal of Labor Economics	0.87
39	Journal of Labor Economics	733	39	Journal of Economic History	0.86
40	Journal of Development Economics	707	40	Monthly Labor Review	0.85
41	Journal of Economic Behavior and Organization	698	41	Journal of Applied Econometrics	0.84
42	Economic Inquiry	684	42	International Economic Review	0.84
43	Monthly Labor Review	669	43	Journal of International Money and Finance	0.84
44	Southern Economic Journal	646	44	European Economic Review	0.81
45	Journal of Economic Dynamics and Control	636	45	World Development	0.80
46	Oxford Bulletin of Economics and Statistics	617	46	IMF Staff Papers	0.75
47	Journal of Banking and Finance	602	47	Journal of Economic Theory	0.74

TABLE 1.4 (CONT'D)

48	Economic Development and Cultural Change	597	48	Journal of Financial and Quantitative Analysis	0.73
49	Oxford Economic Papers	582	49	Regional Science and Urban Economics	0.71
50	Applied Economics	578	50	Journal of Development Economics	0.69
51	Canadian Journal of Economics	556	51	Journal of Comparative Economics	0.69
52	Review of Financial Studies	547	52	Oxford Economic Papers	0.67
53	Economic History Review	545	53	Cambridge Journal of Economics	0.67
54	Health Economics	544	54	World Economics	0.66
55	Industrial Relations	542	55	Post-Soviet Geography and Economics	0.66
56	National Tax Journal	502	56	Kyklos	0.65
57	Journal of Industrial Economics	497	57	Journal of Development Studies	0.62
58	Journal of International Money and Finance	427	58	Journal of Transport Economics and Policy	0.61
59	Journal of Mathematical Economics	418	59	Journal of Risk and Uncertainty	0.60
60	Journal of Applied Econometrics	412	60	Transportation	0.60
61	Games and Economic Behavior	412	61	Journal of Industrial Economics	0.59
62	IMF Staff Papers	391	62	Journal of Real Estate Finance & Economics	0.59
63	Econometric Theory	390	63	Econ Development Quarterly	0.57
64	Journal of Financial and Quantitative Analysis	388	64	Journal of Economic Dynamics and Control	0.56
65	Journal of Development Studies	388	65	Economic Inquiry	0.55
66	Journal of Risk and Uncertainty	383	66	Oxford Bulletin of Economics and Statistics	0.55
67	International Journal of Game Theory	382	67	Natural Resources Journal	0.55
68	Regional Science and Urban Economics	370	68	Economica	0.54
69	World Bank Economics Review	357	69	International Journal of Game Theory	0.54
70	Journal of Policy Analysis and Management	355	70	Games and Economic Behavior	0.53
71	Journal of Economic Issues	355	71	International Journal of Industrial Organization	0.53
72	Cambridge Journal of Economics	351	72	Economic Development and Cultural Change	0.49
73	Scandinavian Journal of Economics	339	73	National Tax Journal	0.48

TABLE 1.4 (CONT'D)

74	Journal of Forecasting	317	74	Journal of Banking and Finance	0.47
75	Journal of Institutional and Theoretical Economics	313	75	European Review of Agricultural Economics	0.46
76	International Journal of Industrial Organization	313	76	Explorations in Economic History	0.45
77	Journal of Transport Economics and Policy	292	77	Journal of Labor Research	0.43
78	Economic Theory	264	78	Econometric Theory	0.43
79	Explorations in Economic History	261	79	International Review of Law and Economics	0.43
80	Kyklos	254	80	American Journal of Agricultural Economics	0.42
81	International Journal of Forecasting	249	81	Journal of Risk and Insurance	0.42
82	Journal of Comparative Economics	245	82	Journal of Economic Issues	0.42
83	Social Choice and Welfare	239	83	Journal of Forecasting	0.42
84	International Labor Review	235	84	Journal of Housing Economics	0.42
85	Journal of Labor Research	227	85	Journal of Population Economics	0.41
86	Economics of Education Review	224	86	Economic Theory	0.40
87	World Economics	214	87	Journal of Economic Behavior and Organization	0.40
88	Canadian Journal of Agricultural Economics	208	88	Social Choice and Welfare	0.39
89	Journal of Risk and Insurance	193	89	Journal of Agriculture and Resource Economics	0.39
90	Journal of Productivity Analysis	188	90	Review of Industrial Organization	0.38
91	History of Political Economy	183	91	Scandinavian Journal of Economics	0.38
92	Natural Resources Journal	182	92	International Labor Review	0.38
93	Journal of Post Keynesian Economics	170	93	Journal of Post Keynesian Economics	0.35
94	Journal of Policy Modeling	164	94	Weltwirtschaftliches Archiv	0.33
95	Journal of Real Estate Finance & Economics	162	95	Canadian Journal of Agricultural Economics	0.33
96	Transportation	157	96	Southern Economic Journal	0.30

Individual Citations

Citations were collected from the ISI Web of Science site, and drawn from the Social Sciences Citation Index database. Recording practices equally credit every author in multi-authored publications.[24] Efforts were made to include all possible citations for every scholar, by searching for citations under both the name with and without the middle initial.[25] When excluding the middle initial, the risk of including citations of people with the same first and last name was reduced, by considering the journal where the cited work was published, as well as the address of the author.[26]

The citation variable should account for professional recognition in the field and, consequently, serve as a good proxy for the quality of academic work. Citations were divided into two categories: citations from others (OTHCITES) and self-citations (SELFCITES). The number of citations from others is arguably a better measure of the reputation of the researcher in the field. The self-citation variable is used to test one of the implications of tournament theory regarding increased incentives for self-promotion in organizations with large wage dispersals.

Salaries (SALARY)

Data were collected on annual total gross salaries received from the employing institution during 1991-92, 1995-96, and 1998-99 academic years. Information on

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[24] A discrepancy arises from the fact that the number of citations is not divided by the number of authors in the cited article. The reason for this is that citations are more easily obtained for individuals, rather than articles. The main difficulty lies in matching the articles included in the index to the citations of these articles. This process would necessitate getting a different citation count for every article published by each author. This study simplifies the data requirements by counting total citations received by each author under the assumption that the measure of professional reputation in each case would not be very different.

[25] Due either to personal preferences or recording practices, the ratio of citations under the name *without* the middle initial to the name *with* the middle initial varied widely. A note of caution is therefore given to future researchers: using either one search criterion alone is likely to greatly underestimate the citation counts for at least a substantial part of the sample.

outside grants, consulting fees, or other sources of income was not collected, based on the assumption that these do not affect the salary decisions.[27] Salary figures were adjusted for inflation rates using the Consumer Price Index.[28]

Experience (EXPER, $EXPER^2$)

Experience is measured as years since award of highest degree, in the same fashion as in previous studies (all the economists in the sample had a Ph.D.). In most regressions, squared experience ($EXPER^2$) is also included to capture concavities in the payment structure.

Quality of Education (TOP10)

Several rankings of economics departments were combined to construct a list of the 10 universities considered to be the best. Table 1.5 displays the various rankings at different points in time. The variable TOP10 receives a value of one if the Ph.D. degree was received from one of these universities: Chicago, Harvard, MIT, Northwestern, Pennsylvania, Princeton, Stanford, Yale, UC-Berkeley, or Wisconsin-Madison. This variable is intended to capture the effect of the graduating institution, as well as proxy for signaled ability at the time of hiring.

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[26] Most cited works include a reference to the institutional address of the researcher, and, by using CVs, relevant addresses could be distinguished.

[27] The same assumption was made explicitly by Tuckman et al. (1977), and implicitly by all others.

[28] Since in all regressions salary appears in logarithmic form and year dummies are always included, the indexing of salary figures for inflation is superfluous. However, the indexing is useful when making simple inter-temporal comparisons of wages based on sample averages as in Table 1.1.

TABLE 1.5 – RANKING OF ECONOMICS DEPARTMENTS

	US News and World Report '98	Gourman Report '97	Ehrenberg-Hurst '96	Dusansky - Vernon Fall '95	Scott-Mitias Fall '94	Conroy – Dusansky Fall '92	Golden et al. '86	Davis-Papanek 78-81	Graves et al. 74-78
1	**Harvard**	**Chicago**	**Chicago**	**Princeton**	**Harvard**	**Princeton**	**Chicago**	**Chicago**	**Chicago**
2	**MIT**	**MIT**	**Harvard**	**Harvard**	**Chicago**	**MIT**	Johns Hopkins	**Harvard**	**Harvard**
3	**Stanford**	**Harvard**	**MIT**	**MIT**	**Pennsylvania**	**Northwestern**	Rochester	**MIT**	**Stanford**
4	**Princeton**	**Princeton**	**Stanford**	**Pennsylvania**	**MIT**	**Chicago**	**Stanford**	**Stanford**	**Wisconsin-Madison**
5	**UC-Berkeley**	**Stanford**	**Princeton**	**Northwestern**	**Northwestern**	**Harvard**	**Harvard**	**Princeton**	**Pennsylvania**
6	**Chicago**	**Yale**	**Yale**	NYU	**Stanford**	**UC-Berkeley**	Cornell	**Yale**	**MIT**
7	**Yale**	**UC- Berkeley**	**UC-Berkeley**	Boston University	**Princeton**	UC-San Diego	**UC-Berkeley**	**Pennsylvania**	**Yale**
8	**Northwestern**	**Pennsylvania**	**Pennsylvania**	**Yale**	Michigan	Boston University	**MIT**	**Wisconsin-Madison**	UCLA
9	**Pennsylvania**	**Northwestern**	**Northwestern**	**Stanford**	**UC-Berkeley**	**Yale**	Columbia	Columbia	**UC-Berkeley**
10	Minnesota/ **Wisconsin-Madison**	Minnesota	Minnesota	UC-San Diego	UCLA	Pennsylvania NYU	VPI	**UC-Berkeley**	**Princeton**

Note: People receiving their Ph.D. degrees in one of the universities in bold receive a value of one for the variable TOP10. The choice of these particular universities was based on the frequency and position they occupied in various rankings over time.

Ordinal Rank

In most regressions, the ordinal ranking of faculty within each department (RANK) is used as an explanatory variable.[29] Faculty were ranked based on the number of individual citations (excluding self-citations), and the appropriate difference in citation counts was derived for people in subsequent ranks as a measure of the "margin of winning" (DISTANCE). The reason why citations were used as a criterion for ranking (as opposed to other measures of performance, such as the publication index), is that citations are believed to be more directly linked to the quality and reputation of one's work (see, *e.g.*, Hamermesh et al., 1982). For example, not all articles appearing in the *American Economic Review* are of the same quality, but the publication index, by construction, would not be able to make a distinction. In addition, it is easier to perform pair-wise comparisons between different faculty members based on their reputation in the profession (as indicated by cites), rather than by comparing articles they published in different journals and fields. In other words, citation counts may take care of the extensive specialization in the profession. Therefore, citation counts appear to be better indicators of the ordinal rank of the faculty.[30]

Other Controls

A department dummy was used in most regressions, to control for department-specific factors in compensation and promotion practices. All individual-level regressions control for administrative duties (ADMIN) other than being chair of the department. These duties include director of graduate studies, dean of a particular

[29] To standardize the rankings among departments with different numbers of faculty, the rank variable used here is weighted by the inverse of the number of faculty for the department in the given year.

college, director of a research center, etc. A separate dummy variable controls for the effect of chairing the economics department under consideration (CHAIR). The variable AWARD identifies exceptionally good teachers who have received teaching awards. All regressions control for gender differences in earnings and promotion probabilities (FEMALE). Field dummies were also included to control for field-specific factors influencing publications. The field dummies used are: THEORY, ECONOMETRICS, LABOR, MACRO/FINANCE, PUBLIC/URBAN, and OTHER, with the latter being the control group. OTHER includes fields such as industrial organization, development, education, experimental, agricultural economics, and history of economic thought. Finally, year dummies for 1995 and 1999 were included to control for secular changes in the academic labor market during the period under consideration.

VI. Econometric Models and Empirical Results

A number of empirical tests will be carried out to investigate the various implications of tournament theory in the current setting. The hypotheses tested, the models used to test them, and the empirical results are summarized below.

A. Tests of Core Predictions of Tournament Theory

This section discusses testing of the main predictions of tournament theory involving the role of rank order in salary and promotion decisions.

Hypothesis 1: Rank order is a significant determinant of academic salaries.

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[30] Rank-order variables based on the publication index and volume were also derived and used to check the robustness of results.

This hypothesis will be tested in two different ways. The first approach will look at the effect of rank order on salaries, assuming that the game is only played once. Specifically, it will be assumed that researchers are evaluated and ranked ordinally only once, and that the wages are then determined based partly on this information. The second approach will assume that the game is played repeatedly. Every period, researchers are ranked based on their performance during the current period, and then the salaries for the next period are determined using this information and other data. When assuming a one-game model, the performance data will be summed over every researcher's academic life-cycle, and the ranking will be derived from the lifetime accumulation of professional reputation. When assuming a repeated game, the change in performance data will be assessed every period, and the ranking will be based on the increase in professional reputation over the period. Models (1) through (4) will test the first hypothesis employing the first approach. Models (5) – (7) will take the second approach. The estimates of models (1) through (4) are presented in Table 1.6A. To check the robustness of the results, the same models are estimated using the citation-based publication index instead of the impact-factor-based publication index, and the results are displayed in Table 1.6B.

The first specification to be estimated is the following:

$$(1) \quad Log\,(SALARY_{it}) = \beta_0 + \beta_1 PUBLINDX^{j}_{it} + \beta_2 VOLUME_{it} + \beta_3 OTHCITES_{it} + \beta_4 EXPER_{it} + \beta_5 EXPER_{it}^2 + \gamma_0 ASSISTANT_{it} + \gamma_1 ASSOCIATE_{it} + \beta_6 AWARDS_{it} + \beta_7 ADMIN_{it} + \beta_8 CHAIR_{it} + \beta_9 TOP10_i + \beta_{10} FEMALE_i + \delta_k FIELD_i + \theta_l DEPARTMENT_i + \lambda_1 Y95_t + \lambda_2 Y99_t + u_{it}$$

where:

i = individual
t = 92,95, 99
j = cr (citation rank) or if (impact factor)
k = 1, …,5 (FIELD = THEORY, ECONOMETRICS, MACRO/FINANCE, PUBLIC/URBAN, LABOR)
l = 1,…, 8 (corresponding to each department in alphabetical order)

TABLE 1.6A– REGRESSION RESULTS ON THE EFFECT OF RANK ORDER ON SALARIES

Dependent Variable: LOG(SALARY)				
Model	(1)	(2)	(3)	(4)
PUBLICATION INDEX (based on the impact factor)	0.00041 (0.00012)***	–	0.00035 (0.00011)***	0.00031 (0.00011)***
VOLUME OF RESEARCH	0.00002 (0.00002)	–	0.00001 (0.00002)	0.00000 (0.00002)
CITATIONS FROM OTHERS	0.00018 (0.00004)***	–	-0.00000 (0.00006)	0.00003 (0.00007)
RANK WITHIN DEPARTMENT (based on citations)	–	-0.46078 (0.05308)***	-0.37510 (0.07438)***	-0.29197 (0.07877)***
DISTANCE BETWEEN RANKS (based on citations)	–	0.00004 (0.00002)*	0.00003 (0.00004)	0.00002 (0.00004)
EXPERIENCE	0.00809 (0.00761)	0.02715 (0.00518)***	0.02471 (0.00509)***	0.01188 (0.00726)
$(\text{EXPERIENCE})^2$	-0.00011 (0.00019)	-0.00039 (0.00014)***	-0.00034 (0.00013)**	-0.00015 (0.00017)
ASSISTANT	-0.28416 (0.05208)***	–	–	-0.20243 (0.05780)***
ASSOCIATE	-0.21457 (0.02955)***	–	–	-0.17929 (0.02977)***
AWARDS	0.07293 (0.03652)**	0.10150 (0.03501)***	0.08936 (0.03485)**	0.08393 (0.03271)**
ADMINISTRATIVE DUTIES	0.13709 (0.04595)***	0.15164 (0.04675)***	0.13689 (0.04918)***	0.13304 (0.04721)***

TABLE 1.6A (CONT'D)

MODEL	(1)	(2)	(3)	(4)
CHAIR	0.17092	0.13253	0.15293	0.14104
	(0.03682)***	(0.03628)***	(0.03537)***	(0.03471)***
TOP10	0.01725	0.01365	0.00599	0.01071
	(0.02382)	(0.02419)	(0.02439)	(0.02328)
FEMALE	-0.00684	-0.04368	-0.02532	-0.00137
	(0.03453)	(0.03387)	(0.03665)	(0.03278)
THEORY	0.06499	0.05246	0.04831	0.06911
	(0.03349)*	(0.03179)	(0.03388)	(0.03067)**
ECONOMETRICS	0.07845	0.06351	0.06604	0.06538
	(0.04129)*	(0.04017)	(0.04056)	(0.03853)*
MACRO/ FINANCE	0.01482	0.00663	0.00736	0.01683
	(0.02666)	(0.03013)	(0.02834)	(0.02535)
PUBLIC/ URBAN	0.01670	0.01767	0.01768	0.02001
	(0.03774)	(0.03873)	(0.04013)	(0.03669)
LABOR	-0.00637	-0.01556	-0.01813	-0.02266
	(0.03192)	(0.03908)	(0.03671)	(0.03356)
UNIVERSITY OF ILLINOIS – URBANA CHAMPAIGN	0.04850	0.02021	0.03627	0.02537
	(0.04125)	(0.03962)	(0.04028)	(0.03849)
INDIANA UNIVERSITY – BLOOMINGTON	-0.00861	-0.03371	-0.04557	-0.01019
	(0.04501)	(0.03903)	(0.04102)	(0.03984)
UNIVERSITY OF IOWA	0.06677	0.02119	0.04584	0.04083
	(0.04114)	(0.03955)	(0.04102)	(0.03744)

TABLE 1.6A (CONT'D)

Model	(1)	(2)	(3)	(4)
THE OHIO STATE UNIVERSITY	0.10178	0.11414	0.09608	0.10015
	(0.04538)**	(0.04930)**	(0.05199)*	(0.04598)**
PURDUE UNIVERSITY	0.08438	0.01945	0.04552	0.04734
	(0.05239)	(0.05414)	(0.05575)	(0.05410)
UNIVERSITY OF MICHIGAN	0.04108	0.12883	0.09799	0.07547
	(0.03973)	(0.03866)***	(0.04249)**	(0.04166)*
UNIVERSITY OF MINNESOTA	0.14100	0.17111	0.16144	0.15998
	(0.06419)**	(0.06084)***	(0.06097)***	(0.05933)***
UNIVERSITY OF WISCONSIN - MADISON	0.08211	0.19203	0.16273	0.13722
	(0.04085)**	(0.03921)***	(0.04468)***	(0.04367)***
YEAR 1995	0.10306	0.11750	0.10939	0.11435
	(0.01334)***	(0.01327)***	(0.01345)***	(0.01364)***
YEAR 1999	0.20521	0.22668	0.20849	0.22225
	(0.01469)***	(0.01553)***	(0.01603)***	(0.01548)***
CONSTANT	10.89465	10.91572	10.85832	11.02620
	(0.08333)***	(0.06400)***	(0.07057)***	(0.07601)***
Observations	615	615	615	615
R-squared	0.66	0.64	0.65	0.68

Note: Robust standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

TABLE 1.6B– REGRESSION RESULTS ON THE EFFECT OF RANK ORDER ON SALARIES

Dependent Variable: LOG(SALARY)				
Model	(1')	(2')	(3')	(4')
PUBLICATION INDEX (based on journal citations)	0.00255 (0.00087)***	–	0.00221 (0.00087)**	0.00201 (0.00084)**
VOLUME OF RESEARCH	0.00004 (0.00002)*	–	0.00002 (0.00002)	0.00002 (0.00002)
CITATIONS FROM OTHERS	0.00017 (0.00005)***	–	-0.00002 (0.00007)	0.00001 (0.00007)
RANK WITHIN DEPARTMENT (based on citations)	–	-0.46078 (0.05308)***	-0.38451 (0.07336)***	-0.29837 (0.07790)***
DISTANCE BETWEEN RANKS (based on citations)	–	0.00004 (0.00002)*	0.00003 (0.00003)	0.00002 (0.00004)
EXPERIENCE	0.00828 (0.00749)	0.02715 (0.00518)***	0.02507 (0.00499)***	0.01207 (0.00712)*
$(\text{EXPERIENCE})^2$	-0.00012 (0.00018)	-0.00039 (0.00014)***	-0.00035 (0.00013)***	-0.00016 (0.00017)
ASSISTANT	-0.28884 (0.05227)***	–	–	-0.20311 (0.05810)***
ASSOCIATE	-0.21958 (0.02917)***	–	–	-0.18190 (0.02939)***
AWARDS	0.07823 (0.03680)**	0.10150 (0.03501)***	0.09434 (0.03480)***	0.08815 (0.03273)***
ADMINISTRATIVE DUTIES	0.14341 (0.04522)***	0.15164 (0.04675)***	0.14235 (0.04857)***	0.13736 (0.04669)***

TABLE 1.6B (CONT'D)

Model	(1)	(2)	(3)	(4)
CHAIR	0.16352 (0.03817)***	0.13253 (0.03628)***	0.14604 (0.03691)***	0.13531 (0.03574)***
TOP10	0.02742 (0.02313)	0.01365 (0.02419)	0.01451 (0.02392)	0.01823 (0.02261)
FEMALE	0.00300 (0.03392)	-0.04368 (0.03387)	-0.01681 (0.03622)	0.00735 (0.03219)
THEORY	0.06150 (0.03269)*	0.05246 (0.03179)	0.04497 (0.03327)	0.06636 (0.03017)**
ECONOMETRICS	0.06221 (0.04213)	0.06351 (0.04017)	0.05163 (0.04079)	0.05224 (0.03883)
MACRO/ FINANCE	0.01841 (0.02670)	0.00663 (0.03013)	0.01024 (0.02866)	0.01983 (0.02549)
PUBLIC/ URBAN	0.02472 (0.03708)	0.01767 (0.03873)	0.02467 (0.03932)	0.02643 (0.03600)
LABOR	0.00390 (0.03493)	-0.01556 (0.03908)	-0.00962 (0.03874)	-0.01531 (0.03544)
UNIVERSITY OF ILLINOIS – URBANA CHAMPAIGN	0.04493 (0.04116)	0.02021 (0.03962)	0.03282 (0.04020)	0.02233 (0.03819)
INDIANA UNIVERSITY – BLOOMINGTON	0.01152 (0.04466)	-0.03371 (0.03903)	-0.02851 (0.04235)	0.00495 (0.03986)
UNIVERSITY OF IOWA	0.06226 (0.04142)	0.02119 (0.03955)	0.04156 (0.04153)	0.03722 (0.03776)

TABLE 1.6B (CONTINUED)

Model	(1)	(2)	(3)	(4)
THE OHIO STATE UNIVERSITY	0.09738	0.11414	0.09214	0.09556
	(0.04607)**	(0.04930)**	(0.05277)*	(0.04633)**
PURDUE UNIVERSITY	0.08217	0.01945	0.04278	0.04524
	(0.05266)	(0.05414)	(0.05596)	(0.05434)
UNIVERSITY OF MICHIGAN	0.04418	0.12883	0.10199	0.07768
	(0.04058)	(0.03866)***	(0.04273)**	(0.04177)*
UNIVERSITY OF MINNESOTA	0.12379	0.17111	0.14710	0.14644
	(0.06599)*	(0.06084)***	(0.06122)**	(0.05966)**
UNIVERSITY OF WISCONSIN - MADISON	0.08112	0.19203	0.16381	0.13723
	(0.04151)*	(0.03921)***	(0.04450)***	(0.04328)***
YEAR 1995	0.10627	0.11750	0.11237	0.11698
	(0.01324)***	(0.01327)***	(0.01334)***	(0.01351)***
YEAR 1999	0.21062	0.22668	0.21338	0.22630
	(0.01441)***	(0.01553)***	(0.01573)***	(0.01513)***
CONSTANT	10.89058	10.91572	10.85669	11.02502
	(0.08381)***	(0.06400)***	(0.07045)***	(0.07618)***
Observations	615	615	615	615
R-squared	0.66	0.64	0.65	0.68

Note: Robust standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

Model 1 includes most observable components of academic salaries. In particular, it includes the grade levels on the right-hand side. Such a specification is in part intended to estimate the salary differential between the ranks, but it is not sufficient to prove the existence of rank-order contests. However, if grade-level dummies appear significant both statistically and practically, then this would provide some evidence in favor of tournament schemes. If productivity is accounted for, then grade level should not matter in the wage regression in the absence of a tournament-type system.[31]

From the estimates of model (1) in the first column of Table 1.6A (and model (1') in Table 1.6B), it appears that Professors earn on average 28-29% more than Assistants and 21-22% more than Associates, all else equal. These estimates are considerably smaller than the ones presented in Table 1.1, derived from a simple comparison of means, where these differences appear to be about 62-73% for Assistants vs. Professors and 42-50% for Associates vs. Professors. These findings suggest that a substantial part of the salary difference between the grade levels is attributable to variations in performance and other observable controls. However, the fact that there is a premium attached to the grade level, above and beyond the contribution of productivity, suggests that promotions carry with them compensation in excess of the value of marginal product.

[31] The grade-level dummies may be endogenous in a salary regression. The reason for this is that grade level is most likely a deterministic function of publications and experience. If the purpose of the analysis is to estimate the effect of publications on salary, including grade-level dummies would be inappropriate. However, the aim here is to test for a grade-level effect after controlling for publications and experience.

The following models incorporate the rank-order variable, to test the implications of tournament theory. If salaries are allocated in part through tournament-type contests within the department, they would depend on the rank order, but the margin of winning should not matter. Therefore the variable DISTANCE, measuring the difference in performance between people in successive ranks, should appear insignificant. A simple regression of salary on rank would overestimate the effect of this variable, because it most likely interacts with other controls. The second specification, therefore, incorporates a few other variables in addition to rank. No performance variables are included at this point, because, according to tournament theory, absolute performance should not matter at all. The second specification is:

$$(2) \quad Log\,(SALARY_{it}) = \beta_0 + \beta_1 EXPER_{it} + \beta_2\, EXPER_{it}^2 + \beta_3 AWARDS_{it} + \beta_4 ADMIN_{it} + \beta_5 CHAIR_{it} + \beta_6 TOP10_i + \beta_7 FEMALE_i + \beta_8 RANK_{it} + \beta_9 DISTANCE_{it} + \delta_k FIELD_i + \theta_l DEPARTMENT_i + \lambda_1 Y95_t + \lambda_2 Y99_t + u_{it}$$

The coefficients of interest are β_8 and β_9. The first one is expected to be negative (because smaller numbers imply higher rank) and significant, whereas the second coefficient is expected to appear insignificant.

The empirical results are presented in Tables 1.6A and 1.6B (Models (2) and (2')) respectively). Since the rank-order variable has been weighted by the number of contestants, its coefficient does not have a straightforward interpretation. In order to obtain the more interesting effect of moving one step up in rank, the coefficient of the rank variable needs to be multiplied by 0.036[32] (the average change in weighted ranks, or 1/28 since 28 is the average number of faculty per department). This amount represents

the average length of the "step" in the ranking scale. Model (2) in Table 1.6A suggests that the ordinal rank within the department has an effect of about 1.66% (|-0.461| (coefficient) x 0.036 (scaling factor) x 100%) on the salary. (The rank-order variable has a negative coefficient (-0.461) because a smaller number is assigned to people higher up in the ranking). This effect may be overestimated because this regression omits the performance variables. The fact that the distance variable appears significant here, suggests that the absolute measure of performance carries some weight in the salary regression.

Since rank order is only expected to partially explain the allocation of salaries in the present setting, model (3) incorporates the available performance measures:

$$(3) \quad Log\,(SALARY_{it}) = \beta_0 + \beta_1 PUBLINDX^j{}_{it} + \beta_2 VOLUME_{it} + \beta_3 OTHCITES_{it} + \beta_4 EXPER_{it} + \beta_5 EXPER_{it}^2 + \beta_6 AWARDS_{it} + \beta_7 ADMIN_{it} + \beta_8 CHAIR_{it} + \beta_9 TOP10_i + \beta_{10} FEMALE_i + \beta_9 RANK_{it} + \beta_{10} DISTANCE_{it} + \delta_k FIELD_i + \theta_l DEPARTMENT_i + \lambda_1 Y95_t + \lambda_2 Y99_t + u_{it}$$

The coefficients corresponding to rank and distance would be of interest here. When performance measures are included, it is not clear whether the rank variable would still appear significant. If the "absolute" performance measures included are stronger determinants of salaries, the rank variables may very well appear insignificant. However, if, in addition to performance measures, the rank variable still carries some weight, then the implications of tournament theory would receive some credibility. Model (3), therefore, constitutes a more rigorous test of the hypothesis regarding the importance of rank order, than the previous models.

The empirical findings are presented in the third column of Table 1.6A (model (3)) and Table 1.6B (model (3')). It appears that the absolute measure of total citations is

[32] Standard deviation is 0.017. Of course, this number varies across departments.

less important than the rank order that one occupies relative to others. The publication index variable is significant regardless of the way it is derived, suggesting that rank order explains only part of the salary structure, whereas absolute performance and other variables explain the rest. The coefficient of 0.37 in absolute value of the rank-order variable indicates that, on average, there is a 1.33% (|-0.37| x 0.036 x 100%) difference in earnings between faculty members in subsequent ranks due to this ordering alone. The margin of winning, as measured by the distance variable and, to some extent, by the total number of citations, appears insignificant. This result conforms tournament theory.

The above statements need to be interpreted with caution. There is some concern that the absolute measure of citations, the distance variable, and rank order are highly correlated.[33] Most troubling is the high correlation between the distance variable and the total number of citations from others (sample correlation is about 0.79). However, when the distance variable is dropped, the absolute measure of citations remains statistically insignificant (p-value=0.56) and its coefficient is about 7 times smaller than what it appeared in model (1), which did not include the rank-order variable. According to model (1), the coefficient of the absolute citation measure is 0.00018 (standard error = 0.00004, implying a salary increase of 1.8% per 100 citations). When the regression in model (3) is run while omitting the highly correlated distance variable (but including rank order), the coefficient of the citation variable becomes 0.000025 (standard error = 0.00004, implying a 0.25% increase in salary per 100 citations if this coefficient were

[33] Sample correlation matrix:

(Observations = 746)	Citations from others	Rank order (weighted)	Distance between ranks
Citations from others	1.00		
Rank order (weighted)	-0.69	1.00	
Distance between ranks	0.79	0.41	1.00

statistically different from zero). While this finding may in part be due to the correlation between the rank-order variable and the total citation counts, the fact that rank order appears to be the stronger variable between the two grants credibility to the claims on the importance of this variable. At the very least, these findings indicate that rank order does matter to a large extent, but absolute performance may also count marginally.

Another concern with the specification of model (3), which includes rank order and total citations in a linear fashion, deals with the possibility of the results being driven by nonlinearities between these two variables. Section VIII explores non-linear functions of both the rank order and the total citations variables. The results do not appear sensitive to the particular functional form chosen, and as a result the linear model is adopted for the rest of the analysis for simplicity.

A rough comparison of the marginal effects of the publication index and the rank-order variable is of interest here. Model (3) in Table 1.6A suggests a salary increase of 2.2% (0.00035 (coefficient) x 10 (pages) x 6.41 (impact factor) x 100%) for a 10-page paper published in the *Journal of Economic Literature*.[34] Since the impact factor represents the average number of citations per article, it would be reasonable to assume that this same article gets cited, on average, 6.41 times per year. The average annual difference in citation counts between two people in subsequent ranks is approximately 11. So, a typical 10-page article published in the *Journal of Economic Literature* can only generate about half a step up in rank if it is cited at the normal rate that the average article in this journal is cited. More specifically, this same article would cause, in addition to the increase in the index variable, another salary increase due to the

[34] This journal is ranks highest according to the impact factor ranking in Table 1.4, right column. Its impact factor is 6.41.

advancement in rank order. If cited at the average rate per year, this article would be associated with an additional increase in salary 0.80% through the rank-order variable.[35] This comparison suggests that the publication measure carries more weight in the salary equation than the rank-order variable.[36] If the 10-page article had been published in the *American Economic Journal* (impact factor =1.98), the resulting change in salary would be about 0.69%.[37] If the article had been cited at the assumed rate of about twice per year, its effect on salary through the rank-order variable would have been 0.25% (1.98 (average citations per year/11 (average citations per year needed to jump one step up in rank) x |-0.38| (coefficient) x 0.036 (scaling factor) x 100%). In both cases, the weight of the article through the publication index is about three times larger than its effect through the rank-order variable.

The fourth model expands model (1) by incorporating the grade-level variables for comparison reasons:

$$(4) \quad Log\,(SALARY_{it}) = \beta_0 + \beta_1 PUBLINDX^j{}_{it} + \beta_2 VOLUME_{it} + \beta_3 OTHCITES_{it} + \beta_4 EXPER_{it} + \beta_5 EXPER_{it}^2 + \gamma_0 ASSISTANT_{it} + \gamma_1 ASSOCIATE_{it} + \beta_6 AWARDS_{it} + \beta_7 ADMIN_{it} + \beta_8 CHAIR_{it} + \beta_9 TOP10_i + \beta_{10} FEMALE_i + \beta_{11} RANK_{it} + \beta_{12} DISTANCE_{it} + \delta_k FIELD_i + \theta_l DEPARTMENT_i + \lambda_1 Y95_t + \lambda_2 Y99_t + u_{it}$$

The interesting detail that would come out of this specification would be the behavior of the grade-level coefficients when the rank-order variable is included. If these variables

[35] The calculations leading to this figure are as follows: 6.41 (average citations per year/11 (average citations per year needed to jump one step up in rank) x |-0.38| (coefficient of rank) x 0.036 (scaling factor) x 100%.

[36] These are very rough calculations, since the distributions of citations and publications in the sample are quite skewed.

[37] Calculations are as follows: 0.00035 (coefficient) x 10 (pages) x 1.98 (impact factor) x 100%=0.69%.

are positively correlated, as expected, their coefficients should appear smaller in absolute value.

Models (4) in Table 1.6A and (4') in Table 1.6B present the findings from the estimation of the above specification. When comparing models (1) and (4), the coefficients on the grade-level dummies go down in absolute value, from 0.28 to 0.20 for the pay differential between Assistants and Professors, and from 0.21 to 0.18 for the pay differential between Associates and Professors. When comparing models (3) and (4), the coefficient of the rank-order variable goes down (from 0.38 to 0.29, in absolute value) with the inclusion of the grade-level variables. This finding suggests that promotions and rank order may be positively correlated, even after controlling for productivity. An implication of this finding is that promotions may be partly allocated through rank-order contests.

The other variables in these regressions merit some consideration. In all regressions presented in Tables 6A and 6B the indicators for teaching awards and administrative duties (chair or other) appear statistically significant. The estimated effects suggest a salary premium ranging from 7-10% for faculty who have earned teaching awards. Faculty in administrative positions (other than department chair) appear to earn on average 13-15% more, whereas chairs of economics departments in the Big Ten are estimated to earn on average 13-17% more than other faculty.

The other controls included, for the most part appear insignificant. Researchers specializing in theoretical economics or econometrics seem to be earning 6-7% higher salaries, but the effect is quite sensitive to the particular model estimated. The estimations also reveal some differences in pay structures across departments.

The following specifications will assume that the game is played repeatedly over each predetermined period. Specifically, it will be assumed that the performance of the faculty is assessed in each stage and then the salary for the next period is determined based on this information and other variables. Models (5) through (7) will be using flow variables, defined as changes in the measures of interest over the 1992-1995 and 1995-1999 periods. The rank-order variable will be redefined to denote the ranking based on *changes* in citation counts over each of the two periods. The winner of the contest, therefore, is the person who accumulates more citations over the period under consideration, and not necessarily over the academic life (as assumed by (1)-(4)).

$$(5) \quad \Delta Log\,(SALARY_{it}) = \beta_0 + \beta_1 \Delta PUBLINDX^j{}_{it} + \beta_2 \Delta VOLUME_{it} + \beta_3 \Delta OTHCITES_{it} + \beta_4 \Delta AWARDS_{it} + \beta_5 \Delta ADMIN_{it} + \beta_6 \Delta CHAIR_{it} + \beta_7 FLRANK_{it} + \beta_8 FLDISTANCE_{it} + \lambda Y99_t + u_{it}$$

where *FLRANK* and *FLDISTANCE* represent the redefined flow rank-order variables.

The results of the above regression are presented in Tables 1.7A and 1.7B (Models (5) and (5'), respectively). The estimates of model (5) suggest that one step up in rank, holding constant the size of the change in research output, is associated with a salary increase of 0.3% per period (|-0.083| (coefficient) x 0.036 (scaling factor) x 100%). A 10-page paper in the *Journal of Economic Literature* during the period under consideration is predicted to result in a change in salary of 4.5%.[38] An article of the same length in the *American Economic Review* during this same period would result in a salary change of 1.39%.[39]

[38] Figure obtained as 0.0007 (coefficient) x 6.41 (impact factor) x 10 (pages) x 100%.
[39] Calculated as 0.0007 (coefficient) x 1.98 (impact factor) x 10 (pages) x 100%.

TABLE 1.7A – THE EFFECT OF RANK ORDER ON SALARY CHANGES

DEPENDENT VARIABLE: Δ [LOG (SALARY)]			
	(5)	(6)	(7)
Δ (PUBLICATION INDEX) (based on the impact factor)	0.00071 (0.00025)***	0.00065 (0.00025)***	0.00060 (0.00021)***
Δ (VOLUME OF RESEARCH)	-0.00002 (0.00002)	-0.00002 (0.00002)	-0.00002 (0.00002)
Δ (CITATIONS FROM OTHERS)	-0.00025 (0.00021)	-0.00026 (0.00020)	-0.00045 (0.00023)*
FLOW RANK WITHIN DEPARTMENT (based on citations)	-0.08340 (0.02760)***	-0.07601 (0.02746)***	-0.09779 (0.02759)***
FLOW DISTANCE BETWEEN RANKS (based on citations)	0.00021 (0.00022)	0.00024 (0.00021)	0.00032 (0.00023)
Δ(AWARD)	-0.00119 (0.05379)	0.01406 (0.05285)	0.01061 (0.06253)
Δ(ADMINISTRATIVE DUTIES)	0.09265 (0.02757)***	0.09393 (0.02812)***	0.10083 (0.03003)***
Δ(CHAIR)	0.10037 (0.03860)***	0.10068 (0.03891)**	0.10935 (0.04014)***
Δ(GRADE LEVEL)	–	0.07267 (0.01872)***	0.07920 (0.01854)***
UNIVERSITY OF ILLINOIS – URBANA CHAMPAIGN	–	–	0.03873 (0.01747)**
INDIANA UNIVERSITY – BLOOMINGTON	–	–	-0.05338 (0.01332)***
UNIVERSITY OF IOWA	–	–	0.00620 (0.01791)
THE OHIO STATE UNIVERSITY	–	–	-0.00329 (0.01512)
PURDUE UNIVERSITY	–	–	0.02049 (0.01735)
UNIVERSITY OF MICHIGAN	–	–	0.05397 (0.01800)***
UNIVERSITY OF MINNESOTA	–	–	0.13655 (0.07293)*
UNIVERSITY OF WISCONSIN - MADISON	–	–	0.01999 (0.02266)
YEAR 1999	-0.00103 (0.01342)	0.00015 (0.01354)	-0.00093 (0.01286)

TABLE 1.7A – (CONT'D)			
CONSTANT	0.19666 (0.02445)***	0.18340 (0.02467)***	0.17806 (0.02609)***
Observations	382	382	382
R-squared	0.13	0.16	0.24

Note: Robust standard errors in parentheses; * significant at 10%; ** significant at 5%; *** significant at 1%

TABLE 1.7B – THE EFFECT OF RANK ORDER ON SALARY CHANGES

DEPENDENT VARIABLE: Δ [LOG (SALARY)]			
	(5')	(6')	(7')
Δ (PUBLICATION INDEX) (based on journal citations)	0.00676 (0.00280)**	0.00597 (0.00282)**	0.00515 (0.00248)**
Δ (VOLUME OF RESEARCH)	-0.00003 (0.00002)	-0.00002 (0.00002)	-0.00003 (0.00002)
Δ (CITATIONS FROM OTHERS)	-0.00023 (0.00021)	-0.00023 (0.00020)	-0.00039 (0.00022)*
FLOW RANK WITHIN DEPARTMENT (based on citations)	-0.08204 (0.02712)***	-0.07770 (0.02691)***	-0.09972 (0.02750)***
FLOW DISTANCE BETWEEN RANKS (based on citations)	0.00018 (0.00020)	0.00019 (0.00020)	0.00026 (0.00022)
Δ (AWARD)	-0.00170 (0.04539)	0.01185 (0.04565)	0.00902 (0.05499)
Δ (ADMINISTRATIVE DUTIES)	0.09519 (0.02730)***	0.09570 (0.02782)***	0.10190 (0.02966)***
Δ (CHAIR)	0.09935 (0.03913)**	0.09926 (0.03918)**	0.10744 (0.04017)***
Δ (GRADE LEVEL)	–	0.06176 (0.02108)***	0.06997 (0.02016)***
UNIVERSITY OF ILLINOIS – URBANA CHAMPAIGN	–	–	0.03458 (0.01734)**
INDIANA UNIVERSITY – BLOOMINGTON	–	–	-0.05280 (0.01293)***
UNIVERSITY OF IOWA	–	–	-0.00091 (0.01840)
THE OHIO STATE UNIVERSITY	–	–	-0.00204 (0.01523)
PURDUE UNIVERSITY	–	–	0.01706 (0.01705)
UNIVERSITY OF MICHIGAN	–	–	0.04847 (0.01787)***
UNIVERSITY OF MINNESOTA	–	–	0.12972 (0.07033)*
UNIVERSITY OF WISCONSIN - MADISON	–	–	0.01405 (0.02400)
YEAR 1999	-0.00249 (0.01342)	-0.00098 (0.01365)	-0.00133 (0.01292)

TABLE 1.7B – (CONT'D)			
CONSTANT	0.19532 (0.02391)***	0.18581 (0.02364)***	0.18380 (0.02542)***
Observations	382	382	382
R-squared	0.15	0.17	0.24

Note: Robust standard errors in parentheses; * significant at 10%; ** significant at 5%; *** significant at 1%

Model (6) adds the change in grade level ($\Delta STATUS$) to control for the effect of promotion during the time period under consideration:

$$(6) \quad \Delta Log\,(SALARY_{it}) = \beta_0 + \beta_1 \Delta PUBLINDX^j{}_{it} + \beta_2 \Delta VOLUME_{it} + \beta_3 \Delta OTHCITES_{it} + \beta_4 \Delta STATUS_{it} + \beta_5 \Delta AWARDS_{it} + \beta_6 \Delta ADMIN_{it} + \beta_7 \Delta CHAIR_{it} + \beta_8 FLRANK_{it} + \beta_9 FLDISTANCE_{it} + \lambda Y99_t + u_{it}$$

The estimates of Model (6) appear in Table 1.7A. The coefficient on the rank-order variable drops slightly, from 0.083 to 0.076 in absolute value. The publication index coefficient drops from 0.0007 to 0.00065, suggesting a change in salary of 4.2% from a 10-page paper in *JEL*.[40] This drop is to be expected, since a change in the grade level (a promotion) is bound to include productivity considerations. The effect of a grade-level change ($\Delta STATUS$) on salary is estimated to be about 7%, holding productivity and other variables constant.

Model (7) adds department dummies to Model (6) above:

$$(7) \quad \Delta Log\,(SALARY_{it}) = \beta_0 + \beta_1 \Delta PUBLINDX^j{}_{it} + \beta_2 \Delta VOLUME_{it} + \beta_3 \Delta OTHCITES_{it} + \beta_4 \Delta STATUS_{it} + \beta_5 \Delta AWARDS_{it} + \beta_6 \Delta ADMIN_{it} + \beta_7 \Delta CHAIR_{it} + \beta_8 FLRANK_{it} + \beta_9 FLDISTANCE_{it} + \theta_i DEPARTMENT_i + \lambda Y99_t + u_{it}$$

Even though time-invariant dummies may not be needed in a regression involving changes in the variables of interest, the department dummies can explain some of the variation in compensation tournaments across the various departments. In fact, since faculty are ranked ordinally within their own departments, the rank-order variable has more meaning when conditioning upon the particular department.

[40] Where 4.2% = 0.00065 (coefficient) x 6.41 (impact factor) x 10 (pages) x 100%.

The estimates of Model (7) are presented in Table 1.7A. They represent the within-department effects of rank and other controls on salary changes. The partial effect of the rank-order variable suggests that a step up in rank is expected to be associated with a 0.4% change in salary (|-0.098| (coefficient) x 0.036 (scaling factor) x 100%). The partial effect of a change in the publication index drops even further (from 0.00065 in Model (6) to 0.0006 in Model (7)), suggesting a change in salary of 3.85% (0.0006 (coefficient) x 6.41 (impact factor) x 10 (pages) x 100%) as a result of a 10-page article published in the *Journal of Economic Literature*. Models (5'), (6'), and (7') in Table 1.7B yield similar results.

Since promotions are a key ingredient of tournament theory, the next step is to see whether rank order affects the allocation of promotions.

Hypothesis 2: The probability of promotion depends on rank order.

To test this hypothesis, the probability of promotion to the next grade level is estimated using rank order as one of the explanatory variables. Model (8) performs this estimation without distinguishing between the two different promotions. This model assumes that the promotion from Assistant to Associate Professor is equivalent to the promotion from Associate to Professor. This assumption is relaxed later.

$$(8)\quad P(Grade\text{-}level_{it} = n|\boldsymbol{x}) = \Phi(\beta_0 + \beta_1 PUBLINDX^j{}_{it} + \beta_2 VOLUME_{it} + \beta_3 OTHCITES_{it} + \beta_4 EXPER_{it} + \beta_5 EXPER_{it}^2 + \beta_6 AWARDS_{it} + \beta_7 ADMIN_{it} + \beta_8 TOP10_i + \beta_9 FEMALE_i + \beta_{10} RANK_{it} + \beta_{11} DISTANCE_{it} + \delta_k FIELD_i + \theta_l DEPARTMENT_i + \lambda_1 Y95_t + \lambda_2 Y99_t + u_{it})$$

n = Assistant, Associate, or Professor.

The partial effect of the RANK variable is of most interest in this specification.

The first column in Tables 1.8A and 1.8B present the estimates of model (8).

TABLE 1.8A – REGRESSION RESULTS ON THE EFFECT OF RANK ORDER ON THE PROBABILITY OF PROMOTION

	Dependent Variable				
	GRADE LEVEL	ASSOCIATE VS. ASSISTANT		PROFESSOR VS. ASSOCIATE	
Model	(8) (Ordered Probit)	(9) (Probit)	Partial effects from model (9)	(10) (Probit)	Partial effects from (10)
PUBLICATION INDEX (based on the impact factor)	0.00274 (0.00134)**	0.00811 (0.00463)*	0.00287 (0.00163)*	0.00291 (0.00156)*	0.00013 (0.00007)*
VOLUME OF RESEARCH	0.00103 (0.00048)**	0.00176 (0.00153)	0.00062 (0.00054)	0.00091 (0.00055)*	0.00004 (0.00003)
CITATIONS FROM OTHERS	-0.00065 (0.00109)	-0.00272 (0.00304)	-0.00096 (0.00106)	-0.00186 (0.00134)	-0.00008 (0.00006)
RANK WITHIN DEPARTMENT (based on citations)	-2.94130 (0.67817)***	-3.92194 (1.79135)**	-1.38602 (0.62347)**	-4.05785 (0.91303)***	-0.17467 (0.07536)**
DISTANCE BETWEEN RANKS (based on citations)	-0.00014 (0.00088)	-0.00142 (0.00241)	-0.00050 (0.00086)	0.00015 (0.00107)	0.00001 (0.00005)
EXPERIENCE	0.40657 (0.04279)***	0.46449 (0.16630)***	0.16415 (0.06830)**	0.45169 (0.06787)***	0.01944 (0.00789)**
$(\text{EXPERIENCE})^2$	-0.00501 (0.00100)***	-0.00702 (0.00718)	-0.00248 (0.00269)	-0.00542 (0.00141)***	-0.00023 (0.00011)**
TOP10	-0.37009 (0.16480)**	-0.73451 (0.36663)**	-0.25926 (0.12661)**	-0.06485 (0.21631)	-0.00281 (0.00960)
AWARDS	0.46087 (0.34431)	-0.22436 (1.12688)	-0.08281 (0.43056)	0.47941 (0.41962)	0.01456 (0.01008)

TABLE 1.8A (CONT'D)

ADMINISTRATIVE DUTIES	-0.05243	-2.15976	-0.65828	0.63260	0.01745
	(0.32839)	(0.88213)**	(0.12055)***	(0.45978)	(0.01058)*
FEMALE	-0.45705	-0.05543	-0.01974	-0.36720	-0.02191
	(0.24467)*	(0.50122)	(0.18004)	(0.37225)	(0.03175)
THEORY	-0.47477	0.78714	0.23645	-1.23219	-0.14490
	(0.24985)*	(0.55031)	(0.13222)*	(0.33398)***	(0.07266)**
ECONOMETRICS	-0.19204	-0.74296	-0.28542	-0.17319	-0.00868
	(0.31014)	(0.60869)	(0.23457)	(0.41531)	(0.02395)
MACRO/ FINANCE	-0.25221	0.30315	0.10147	-0.60023	-0.03993
	(0.21853)	(0.48557)	(0.15567)	(0.29113)**	(0.02834)
PUBLIC/ URBAN	-0.20837	-0.21079	-0.07723	-0.36752	-0.02110
	(0.24979)	(0.56228)	(0.21188)	(0.32003)	(0.02383)
LABOR	0.24012	-0.41379	-0.15619	-0.04158	-0.00186
	(0.32966)	(0.78335)	(0.30895)	(0.44386)	(0.02038)
UNIVERSITY OF ILLINOIS – URBANA CHAMPAIGN	0.21359	0.10149	0.03513	0.36694	0.01208
	(0.31709)	(0.94257)	(0.31928)	(0.38964)	(0.01068)
INDIANA UNIVERSITY – BLOOMINGTON	-1.63454	-2.26762	-0.70282	-1.49513	-0.22631
	(0.34445)***	(0.64645)***	(0.10592)***	(0.53712)***	(0.15017)
UNIVERSITY OF IOWA	-0.42010	-0.96085	-0.36728	-0.13131	-0.00630
	(0.30958)	(0.72206)	(0.26515)	(0.40801)	(0.02167)

TABLE 1.8A (CONT'D)

THE OHIO STATE UNIVERSITY	-0.59424 (0.27726)**	-0.76052 (0.63982)	-0.28929 (0.24606)	-0.51937 (0.33861)	-0.03392 (0.03197)
PURDUE UNIVERSITY	-0.74118 (0.32174)**	-1.64408 (0.80534)**	-0.58055 (0.20408)***	-0.69380 (0.39303)*	-0.05474 (0.04944)
UNIVERSITY OF MICHIGAN	0.74320 (0.33337)**	0.01781 (0.66648)	0.00627 (0.23399)	1.50913 (0.44664)***	0.02977 (0.01358)**
UNIVERSITY OF MINNESOTA	-0.50939 (0.38228)	-0.99996 (0.74335)	-0.38173 (0.27000)	-0.37713 (0.51711)	-0.02295 (0.04261)
UNIVERSITY OF WISCONSIN - MADISON	0.70437 (0.36798)*	0.40899 (0.74056)	0.13037 (0.20789)	1.36082 (0.48462)***	0.02308 (0.01100)**
YEAR 1995	-0.21672 (0.18288)	0.12402 (0.35374)	0.04341 (0.12249)	-0.46071 (0.25280)*	-0.02404 (0.01718)
YEAR 1999	-0.55673 (0.20230)***	0.13236 (0.42333)	0.04606 (0.14466)	-1.00346 (0.28632)***	-0.06528 (0.02941)**
CUT 1	0.21529 (0.58320)	–	–	–	–
CUT 2	2.66109 (0.61849)	–	–	–	–
CONSTANT		-0.06445 (1.75848)		-2.34764 (0.79487)***	
Observations	665	212		594	
Pseudo R-squared	0.65	0.66		0.64	
Log-Likelihood	-189.247	-48.70		-113.28	

Note: Standard errors in parentheses.
* significant at 10%; ** significant at 5%; *** significant at 1%

TABLE 1.8B – REGRESSION RESULTS ON THE EFFECT OF RANK ORDER ON THE PROBABILITY OF PROMOTION

	Dependent Variable				
	GRADE LEVEL	ASSOCIATE VS. ASSISTANT		PROFESSOR VS. ASSOCIATE	
Model	(8') (Ordered Probit)	(9') (Probit)	Partial effects from model (9')	(10') (Probit)	Partial effects from (10')
PUBLICATION INDEX (based on the impact factor)	0.01291 (0.00818)	0.05606 (0.02847)**	0.01975 (0.01002)**	0.01093 (0.00950)	0.00052 (0.00046)
VOLUME OF RESEARCH	0.00125 (0.00047)***	0.00242 (0.00141)*	0.00085 (0.00049)*	0.00114 (0.00054)**	0.00005 (0.00003)*
CITATIONS FROM OTHERS	-0.00047 (0.00108)	-0.00236 (0.00305)	-0.00083 (0.00107)	-0.00160 (0.00133)	-0.00008 (0.00007)
RANK WITHIN DEPARTMENT (based on citations)	-2.86786 (0.68707)***	-3.16276 (1.80151)*	-1.11402 (0.62959)*	-4.01425 (0.91873)***	-0.18925 (0.07951)**
DISTANCE BETWEEN RANKS (based on citations)	-0.00024 (0.00086)	-0.00145 (0.00242)	-0.00051 (0.00086)	0.00002 (0.00106)	0.00000 (0.00005)
EXPERIENCE	0.40762 (0.04239)***	0.48713 (0.16505)***	0.17158 (0.06799)**	0.44520 (0.06645)***	0.02099 (0.00814)***
$(\text{EXPERIENCE})^2$	-0.00511 (0.00098)***	-0.00754 (0.00707)	-0.00266 (0.00265)	-0.00542 (0.00138)***	-0.00026 (0.00012)**
TOP10	-0.33637 (0.16624)**	-0.78832 (0.37410)**	-0.27702 (0.12789)**	-0.04481 (0.22211)	-0.00212 (0.01066)
AWARDS	0.44994 (0.34579)	0.03919 (1.10930)	0.01368 (0.38358)	0.42470 (0.41946)	0.01473 (0.01142)

TABLE 1.8B (CONT'D)

ADMINISTRATIVE DUTIES	0.02831	-1.66728	-0.57743	0.71205	0.02067
	(0.32908)	(0.78242)**	(0.17555)***	(0.46475)	(0.01117)*
FEMALE	-0.48309	-0.06279	-0.02232	-0.42746	-0.02920
	(0.24596)**	(0.49453)	(0.17746)	(0.37218)	(0.03761)
THEORY	-0.50918	0.64293	0.19911	-1.24802	-0.15777
	(0.24944)**	(0.54150)	(0.14089)	(0.33329)***	(0.07668)**
ECONOMETRICS	-0.24184	-0.91893	-0.35215	-0.22786	-0.01307
	(0.31270)	(0.63199)	(0.23423)	(0.41580)	(0.02846)
MACRO/ FINANCE	-0.24244	0.28078	0.09404	-0.56370	-0.03957
	(0.21699)	(0.48865)	(0.15730)	(0.28548)**	(0.02864)
PUBLIC/ URBAN	-0.18213	-0.16750	-0.06079	-0.32309	-0.01950
	(0.24934)	(0.56637)	(0.21066)	(0.31740)	(0.02410)
LABOR	0.29577	-0.30292	-0.11258	0.06395	0.00286
	(0.32603)	(0.80478)	(0.31195)	(0.43500)	(0.01862)
UNIVERSITY OF ILLINOIS – URBANA CHAMPAIGN	0.21090	0.00225	0.00079	0.36950	0.01336
	(0.31472)	(0.90002)	(0.31674)	(0.38568)	(0.01153)
INDIANA UNIVERSITY – BLOOMINGTON	-1.55956	-2.18440	-0.69191	-1.35083	-0.19668
	(0.34141)***	(0.64889)***	(0.11274)***	(0.52448)**	(0.13967)
UNIVERSITY OF IOWA	-0.41027	-0.95117	-0.36347	-0.10639	-0.00547
	(0.30893)	(0.72188)	(0.26637)	(0.40449)	(0.02256)

TABLE 1.8B (CONT'D)

THE OHIO STATE UNIVERSITY	-0.56835 (0.27562)**	-0.68058 (0.63934)	-0.25795 (0.24794)	-0.46791 (0.33630)	-0.03182 (0.03178)
PURDUE UNIVERSITY	-0.73456 (0.32290)**	-1.70218 (0.81471)**	-0.59519 (0.19850)***	-0.67850 (0.39246)*	-0.05701 (0.05186)
UNIVERSITY OF MICHIGAN	0.71366 (0.33312)**	-0.07660 (0.67964)	-0.02737 (0.24577)	1.47815 (0.44775)***	0.03241 (0.01423)**
UNIVERSITY OF MINNESOTA	-0.53826 (0.38734)	-1.32792 (0.80857)	-0.49284 (0.25215)*	-0.34491 (0.51514)	-0.02215 (0.04359)
UNIVERSITY OF WISCONSIN - MADISON	0.63661 (0.37044)*	0.20770 (0.76511)	0.06970 (0.24316)	1.28625 (0.48510)***	0.02500 (0.01151)**
YEAR 1995	-0.21190 (0.18241)	0.09239 (0.35572)	0.03231 (0.12344)	-0.43122 (0.25003)*	-0.02420 (0.01778)
YEAR 1999	-0.53104 (0.20089)***	0.00765 (0.43523)	0.00269 (0.15303)	-0.92472 (0.27802)***	-0.06277 (0.02882)**
CUT 1	0.26595 (0.59056)	–	–	–	–
CUT 2	2.69744 (0.62531)	–	–	–	–
CONSTANT		-0.74337 (1.79227)		-2.26920 (0.79768)***	
Observations	665	212		594	
Pseudo R-squared	0.65	0.66		0.64	
Log-Likelihood	-190.13	-48.23		-114.43	

Note: Standard errors in parentheses.
* significant at 10%; ** significant at 5%; *** significant at 1%

Model (8) in Table 1.8A shows the results from an ordered probit estimation, where grade level is the dependent variable.[41] Rank order appears to be significant in the promotion decision whereas the margin of winning is not (models (8) and (8')). It should be noted that the up-or-out tenure feature of academic contracts induces some selection bias in these results. More specifically, the Assistant Professors who were not promoted would not be the sample. Also, Assistant Professors who left to join higher-ranked departments would not be in the sample. In the case of such selection bias, the model would *underestimate* the effect of rank order if most junior moves were due to non-promotion, and would *overestimate* this same effect if most junior faculty move to more prominent departments. A closer look at the mobility patterns of junior faculty shows that this particular sample may not be affected by these problems. Only four junior faculty leave the sample, of which one moves to a higher-ranked department and three move to lower ranked departments.[42] These issues are revisited in more detail in section VII.II when discussing the third hypothesis of the tournament model (handicapping).

The assumption of equivalence between the different types of promotions is now relaxed. From the discussion in the second section of this paper, the promotion from Assistant to Associate Professor may be more important relative to the respective raise in salary, whereas a salary raise may be more important than the promotion from Associate to Professor. Models (9) and (10) separately estimate the effect of rank order on the two types of promotion.

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[41] The grade-level variable takes 3 values here: =1 for Assistants, =2 for Associates, =3 for Professors.

[42] They eventually continue moving to higher-ranked departments after 2000.

$$(9)\quad P(Associate = 1|\mathbf{x}, Assistant) = \Phi(\beta_0 + \beta_1 PUBLINDX^j_{it} + \beta_2 VOLUME_{it} + \beta_3 OTHCITES_{it} + \beta_4 EXPER_{it} + \beta_5 EXPER_{it}^2 + \beta_6 AWARDS_{it} + \beta_7 ADMIN_{it} + \beta_8 TOP10_i + \beta_9 FEMALE_i + \beta_{10} RANK_{it} + \beta_{11} DISTANCE_{it} + \delta_k FIELD_i + \theta_l DEPARTMENT_i + \lambda_1 Y95_t + \lambda_2 Y99_t + u_{it})$$

$$(10)\quad P(Professor = 1|\mathbf{x}, Associate) = \Phi(\beta_0 + \beta_1 PUBLINDX^j_{it} + \beta_2 VOLUME_{it} + \beta_3 OTHCITES_{it} + \beta_4 EXPER_{it} + \beta_5 EXPER_{it}^2 + \beta_6 AWARDS_{it} + \beta_7 ADMIN_{it} + \beta_8 TOP10_i + \beta_9 FEMALE_i + \beta_{10} RANK_{it} + \beta_{11} DISTANCE_{it} + \delta_k FIELD_i + \theta_l DEPARTMENT_i + \lambda_1 Y95_t + \lambda_2 Y99_t + u_{it})$$

Since most Associate Professors are also tenured faculty, estimating model (9) is similar to estimating the effect of rank order on the probability of receiving tenure.

The probit estimates of Models (9) and (10) are presented in Tables 1.8A and 1.8B. They display the probability of promotion to the next rank, given that one's placement is at the preceding rank. Model (9) suffers from the same selection bias mentioned above, so the importance of rank order on the probability of promotion at this stage may be underestimated. The third column in Table 1.8A displays the partial effects from model (9). The partial effect of the rank-order variable, indicates that one step up in rank is associated with a 0.05 (|-1.39| (partial effect) x 0.036 (scaling factor)) increase in the probability of being promoted from Assistant to Associate Professor, all else equal. At this stage, it appears that rank order and experience are the most important determinants of promotion (although the publication index is marginally significant).[43]

[43] The experience variable points toward the selection bias mentioned above. At the junior stage, the experience variable is significant, but the squared term is not. In theory, the effect of experience should be turning around quite rapidly at the junior stage. The reason for this is that, if people are not promoted after a certain number of years, they are probably less qualified than the rest. The up-or–out feature, however, ensures that people who are not promoted, leave the sample. This may be the reason why the estimates indicate that the effect of experience does not turn around at all at this stage.

Model (10) does not suffer from the selection bias mentioned above, since almost all Associates in the sample have a chance to be considered for promotion to the next level.[44] In this model, the estimates suggest that one step up in rank results in a 0.006 (|-0.17| (partial effect) x 0.036 (scaling factor)) increase in the probability of promotion to Professor. According to the partial effects calculated in the last column of Table 1.8A, a 10-page paper published in the *Journal of Economic Literature* would result in a 0.008 increase in the probability of being promoted to the next level.[45] The estimates in Table 1.8B yield similar results.

B. Tests of Other Implications of Tournament Theory

This section will focus on testing implications of the theory involving handicapping, wage vs. productivity spreads, and self-promotion. A testable implication of the tournament theory concerns mixed (heterogeneous) contests. The theory maintains that all participants will reduce their effort in heterogeneous contests, because the low-ability types believe they will lose anyway, whereas the high-ability types believe will win anyway. In such situations, a handicapping system (which gives the low-ability types a head start), is assumed to restore the efficient level of incentives. This implication will be tested next:

Hypothesis 3: In mixed tournaments, handicapping of high-ability types is needed to ensure efficiency.

With the data available for this study, it may not be possible to test this hypothesis directly. Ideally, one would need to have additional individual-level data about people at

[44] There may be a small number of untenured Associate Professors in the sample, but the data do not allow distinguishing between them and the rest.

[45] Calculated as: 0.00013 (coefficient) x 6.41 (impact factor) x 10 (pages).

the time of hiring that would better reveal the quality of the applicants. Then, the junior faculty members could be followed over the period of time necessary to receive the promotions. Finally, an analysis of who had better chances to obtain the promotion (conditional on productivity) could show whether higher ability people were disadvantaged in the promotion contest. However, the only variable available in this dataset that carries any information about the signaled ability at the time of hiring is TOP10, which is a very crude measure of individual ability. The third hypothesis will be tested by looking at the estimate of the variable TOP10 in model (9), but this will only be a crude test, because of the imprecision with which TOP10 measures ability. If the department is actively engaging in a process aiming at homogenizing the pool of contestants, the people signaling higher ability at the time of hiring will be set back to level the playing field, and therefore, the estimate of TOP10 should be negative. It should be noted that this phenomenon most likely would not manifest itself after the junior stage. The main reason for this is that more information about the ability of the person can be obtained during the first years of work. Therefore, the relevance of the TOP10 variable as a measure of ability falls beyond the junior stage. If the handicapping phenomenon is present beyond this stage, the TOP10 variable would be unsuitable to reveal it.

In models (7), (8), (7'), and (8') in Tables 1.8A and 1.8B, the variable TOP10, which serves as a proxy for high-ability types, seems to affect the chances of promotion negatively (and significantly so). Since these results emerge in regressions, which account for productivity, experience, rank order, field, and department, the TOP10 variable only displays the effect of the graduating institution. These findings suggest that

in promotion tournaments, people graduating from top universities may be disadvantaged in the early stages with the intention of leveling the playing field. This result is corroborated by the finding that the group graduating from top-10 institutions, appears on average more productive then the rest. Table 1.9A provides a comparison of means for the most important variables for the two groups. With respect to experience, grade level, salaries, (weighted and simple) rank order, and number of citations, the two groups do not differ significantly from each other. However, the group graduating from top institutions has significantly higher average publication indexes and volume of publications. In contrast, the group graduating from less prominent departments appears to have accumulated more teaching awards. Table 1.9B restricts attention to junior faculty only. The same observations hold in this case, with the exception that junior faculty graduating from top ten institutions appear to accumulate significantly more citations in addition to publications, and as a result, tend to occupy higher rank order spots within their respective departments. The only non-significant differences between the two groups are in experience, administrative duties, and grade level.

Since Big Ten departments are primarily research institutions, it seems unlikely that promotions are primarily based on teaching performance and less so on research performance. This would be the only explanation to the observed lower probability of promotion associated with faculty graduating from top institutions. Even if this were the case, since both sets of performance variables are included in the regressions (teaching awards and research activity), the additional effect of TOP10 cannot be attributed to the apparent superior teaching performance of faculty graduating from less prominent

departments. Therefore, the estimated negative coefficient of the variable TOP10 seems to confirm tournament-theory implications with respect to heterogeneous contests.

TABLE 1.9A– COMPARISON OF KEY VARIABLES BY QUALITY OF GRADUATING INSTITUTION

	Graduating from top 10 institutions		Graduating from other institutions		Statistically different
	Sample Mean	Standard Deviation	Sample Mean	Standard Deviation	
Publication index (based on the impact factor)	186.24	159.94	129.78	126.89	Yes
Publication index (based on journal citations)	24.06	21.32	20.25	22.57	Yes
Volume of publications	507.04	935.48	324.00	364.78	Yes
Citations from others	240.91	273.55	234.23	372.91	No
Total citations	256.09	289.08	247.24	383.47	No
Teaching awards	0.08	0.28	0.15	0.36	Yes
Administrative duties (including chair)	0.16	0.37	0.15	0.35	No
Experience	18.24	10.08	19.29	10.31	No
Grade level	2.57	0.70	2.56	0.71	No
Departmental rank (simple)	15.08	8.55	15.50	9.66	No
Departmental rank (weighted)	0.51	0.28	0.54	0.30	No
Salaries (indexed for inflation)	83,295	26,207	80,902	27,283	No
Percent Assistant Professors	12.34%		12.53%		
Percent Associate Professors	18.51%		19.07%		

TABLE 1.9B– COMPARISON OF KEY VARIABLES BY QUALITY OF GRADUATING INSTITUTION (ASSISTANTS AND ASSOCIATES ONLY)

	Graduating from top 10 institutions		Graduating from other institutions		Statistically different
	Sample Mean	Standard Deviation	Sample Mean	Standard Deviation	
Publication index (based on the impact factor)	90.92	86.30	59.40	62.25	Yes
Publication index (based on citation counts)	12.69	12.11	8.89	11.10	Yes
Volume of publications	203.22	198.45	132.65	148.72	Yes
Citations from others	119.78	161.09	74.74	108.44	Yes
Total citations	126.79	166.41	81.80	116.89	Yes
Teaching awards	0.00	0.00	0.07	0.26	Yes
Administrative duties	0.04	0.19	0.02	0.15	No
Experience	9.38	5.98	9.22	6.44	No
Grade level	1.60	0.49	1.60	0.49	No
Departmental rank (simple)	19.08	8.28	21.02	7.58	Yes*
Departmental rank (weighted)	0.67	0.26	0.73	0.21	Yes
Salaries (indexed for inflation)	60,038	12,113	58,755	13,685	No
Percent Assistant Professors	40.00%		39.66%		

* p-value = 0.07

Another concern with the observed handicapping phenomenon relates to the selection bias discussed earlier in the chapter. It could be the case that more productive junior faculty graduating from top ten institutions have moved out of the sample and joined more prominent departments. The faculty sample used in this study includes 19 junior faculty who have entered or exited the sample during the 1992-1999 period. Fourteen of them have entered the sample after 1992 and have remained with their original department as of 1999. Three faculty members, who were employed by one of the Big Ten departments as of 1992, have exited the sample before 1999, and two faculty members have both entered and exited the sample during the 1992-1999 period. Out of all nineteen junior faculty who enter or exit the sample, only six have graduated from top 10 departments. Table 1.10 below details the moves for faculty exiting the sample (origin and destination departments) and evaluates whether the move is up, down, or horizontal, based on rankings from the National Research Council (1995).

TABLE 1.10 – EXITS OF JUNIOR FACULTY FROM THE SAMPLE

Origin Department	Destination Department (1)	Destination Department (2)	Move up/ down	Graduated from top ten department
University of Minnesota	New York University (1998)	Yale (2000)	(1) down; 10 to 17 (2) up; 17 to 6	No
University of Minnesota	New York University (1998)	University of Pennsylvania (2000)	(1) down; 10 to 17 (2) up; 17 to 8	No
University of Minnesota	University of Wisconsin (1998)	University of Pennsylvania (2002)	(1) down; 10 to 15 (2) up; 15 to 8	Yes
University of Minnesota	University of Pennsylvania (1997)	Arizona State (2001)	(1) up; 10 to 8 (2) down; 8 to 67	No

In order to avoid the selection bias discussed above, the effect of graduating from a top ten department on the probability of promotion is re-estimated by restricting the sample in various ways. First the sample is restricted to exclude all junior faculty who enter or exit during the 1992-1999 period or for whom salary data are missing for any year during this period (balanced panel). The first column in Table 1.11A below reproduces the results from model (9) previously presented in Table 1.8A. These are the original probit estimates of the promotion probability from Assistant to Associate Professor which evidence a negative impact associated with having graduated from a top ten department. Since the field dummies are jointly insignificant (p-value = 0.36), they are dropped and the model is re-estimated. The results appear in the second column of Table 1.11A under model (12). From the partial effects of these regressions (shown in Table 1.11B), it appears that the partial effect of the TOP10 variable increases slightly from 0.26 to 0.29 after the exclusion of the field dummies. The estimates of regressions that use the balanced panel are displayed in Table 1.11A models (13) and (14). Model (13) shows that the effect of TOP10 remains similar in nature to previous estimations, although its partial effect is much smaller (only 0.12 compared to 0.26-0.29 estimated in models (11) and (12)). In model (14) the effect of TOP10 ceases to be significant. Interestingly, all other productivity variables (including the rank variable) are insignificant in the regressions that use the balanced panel. The reason for this most likely is the reduced sample (from 212 to 135 observations). Experience remains significant, but its estimated partial effect is much smaller compared to the case when all observations from junior faculty are used (0.04 – 0.07 vs. 0.16 – 0.17).

Employing a balanced panel for this analysis may be unnecessarily restrictive. For once, this method drops observations from individuals who have been in the sample during the entire period under consideration, but for external reasons salary information is missing for given years. As a result, the analysis is repeated with a less restrictive sample, which excludes junior faculty who enter or exit the sample during 1992-1999 (but includes faculty with missing observations for a particular year). The previous models are re-estimated and the regression results appear in Table 1.12A, models (15) and (16). The difference between the two is that model (16) excludes field dummies, which appear jointly insignificant. The sample size has increased slightly in these estimations as compared to the case where the balanced panel was used (from 135 to 163). The variable of interest (TOP10) appears insignificant in both estimations with and without field dummies. However, the p-values on this variable are 0.17 and 0.14 for models (15) and (16) respectively, so the variable could be considered significant against the 10% one-sided alternative. The partial effects (tabulated in the first two columns of Table 1.12B), are closer to the partial effects estimated earlier using the entire sample (0.21-0.22 vs. 0.26-0.29 with the full sample). The sample used here, however, may still be too restrictive for the purpose of the analysis at hand. The main issue would be that this sample excludes new entering faculty who have obtained their degrees after 1992 and who are competing for promotions just like other junior faculty who have graduated before 1992.

As a result, the restrictions on the sample used are relaxed further to include entering junior faculty and exclude junior faculty who exit the sample. This sample should be more relevant to the analysis, since it deals with the possibility that the effect

of TOP10 is driven by junior faculty graduating from top ten schools who move to better departments. The new estimates are presented in Table 1.12A, models (17) and (18). Here the TOP10 variable appears statistically significant and its partial effect (shown on Table 1.12B) ranges from 0.24 to 0.27, which is quite close to the estimated partial effect of TOP10 when using the entire sample (ranging from 0.26 to 0.29). Rank and experience remain significant and their partial effect is similar to the base case. From these results it appears that entry and exit from the sample do not appear to drive the main results.

There is another way to approach the question of whether moves in and out of the sample influence the estimated effect of the TOP10 variable. That would be to include interactions of TOP10 with a dummy variable indicating whether a particular junior faculty member enters or exits the sample. Table 1.13 presents regression estimates, which include interactions of TOP10 with YMOVER (junior faculty entering or exiting the sample) and AMOVER (junior faculty exiting the sample). Both sets of interactions appear insignificant, further corroborating the finding that the effect of TOP10 does not appear to be influenced by moves in and out of the sample.

TABLE 1.11A – REGRESSION RESULTS ON THE EFFECT OF GRADUATING FROM A TOP TEN SCHOOL ON THE PROBABILITY OF PROMOTION

Dependent Variable: ASSOCIATE vs. ASSISTANT

ESTIMATION METHOD: PROBIT

SAMPLE	ALL	ALL	BALANCED PANEL	BALANCED PANEL
Model	(11)	(12)	(13)	(14)
PUBLICATION INDEX	0.00811	0.00554	0.00150	-0.00109
(based on the impact factor)	(0.00463)*	(0.00430)	(0.00779)	(0.00623)
VOLUME OF RESEARCH	0.00176	0.00153	0.00282	0.00183
	(0.00153)	(0.00139)	(0.00259)	(0.00203)
CITATIONS FROM OTHERS	-0.00272	-0.00225	0.00274	0.00225
	(0.00304)	(0.00294)	(0.00602)	(0.00542)
RANK WITHIN DEPARTMENT	-3.92194	-3.45862	-1.25938	-2.13148
(based on citations)	(1.79135)**	(1.64626)**	(2.95007)	(2.57802)
DISTANCE BETWEEN RANKS	-0.00142	-0.00105	-0.00348	-0.00213
(based on citations)	(0.00241)	(0.00229)	(0.00715)	(0.00388)
EXPERIENCE	0.46449	0.48190	0.63590	0.34961
	(0.16630)***	(0.15078)***	(0.26311)**	(0.22013)
$(\text{EXPERIENCE})^2$	-0.00702	-0.00782	-0.01444	-0.00141
	(0.00718)	(0.00623)	(0.01097)	(0.01008)
TOP10	-0.73451	-0.80027	-1.00670	-0.65327
	(0.36663)**	(0.34096)**	(0.61236)*	(0.49986)
AWARDS	-0.22436	-0.24617	-3.04265	-2.08435
	(1.12688)	(0.99437)	(3.16697)	(2.24587)
ADMINISTRATIVE DUTIES	-2.15976	-1.89665	-1.29607	-0.94775
	(0.88213)**	(0.82303)**	(1.28364)	(1.02781)

TABLE 1.11A (CONT'D)

FEMALE	-0.05543 (0.50122)	-0.35476 (0.44697)	-0.83041 (0.79154)	-0.52876 (0.57417)
THEORY	0.78714 (0.55031)	–	0.53041 (0.76314)	–
ECONOMETRICS	-0.74296 (0.60869)	–	-0.50609 (1.00014)	–
MACRO/ FINANCE	0.30315 (0.48557)	–	1.96628 (0.92879)**	–
PUBLIC/ URBAN	-0.21079 (0.56228)	–	0.94810 (0.80430)	–
LABOR	-0.41379 (0.78335)	–	-1.06078 (1.19347)	–
UNIVERSITY OF ILLINOIS – URBANA CHAMPAIGN	0.10149 (0.94257)	0.30178 (0.80093)	2.65777 (3.17765)	2.22472 (2.27981)
INDIANA UNIVERSITY – BLOOMINGTON	-2.26762 (0.64645)***	-1.96246 (0.58838)***	-2.93416 (1.05944)***	-2.25000 (0.81508)***
UNIVERSITY OF IOWA	-0.96085 (0.72206)	-0.90722 (0.67400)	-1.09471 (1.11873)	-1.46382 (0.95670)
THE OHIO STATE UNIVERSITY	-0.76052 (0.63982)	-0.55060 (0.58247)	0.59266 (1.19725)	0.07599 (1.02901)
PURDUE UNIVERSITY	-1.64408 (0.80534)**	-1.38492 (0.77161)*	-3.09260 (1.26343)**	-2.71953 (1.22112)**

TABLE 1.11A (CONT'D)

UNIVERSITY OF MICHIGAN	0.01781	0.21026	0.06081	-0.14207
	(0.66648)	(0.63133)	(0.94438)	(0.83654)
UNIVERSITY OF MINNESOTA	-0.99996	-0.89047	-3.45423	-1.74524
	(0.74335)	(0.68915)	(1.63965)**	(1.07173)
UNIVERSITY OF WISCONSIN - MADISON	0.40899	0.25516	-0.90350	-0.36667
	(0.74056)	(0.68791)	(1.19120)	(0.94154)
YEAR 1995	0.12402	0.14872	0.39552	0.36474
	(0.35374)	(0.33839)	(0.49195)	(0.42348)
YEAR 1999	0.13236	0.12718	1.98622	1.20073
	(0.42333)	(0.40059)	(0.98263)**	(0.70667)*
CONSTANT	-0.06445	-0.26146	-3.11509	-0.62287
	(1.75848)	(1.58216)	(2.97473)	(2.52466)
Observations	212	212	135	135
Field dummies jointly insignificant ($\chi^2(5)$)	5.47		7.00	
Prob> χ^2	0.36		0.22	
Pseudo R-squared	0.66	0.64	0.67	0.61
Log-Likelihood	-48.69	-51.64	-26.48	-31.29

Note: Standard errors in parentheses.
* significant at 10%; ** significant at 5%; *** significant at 1%

TABLE 1.11B – PARTIAL EFFECTS FROM REGRESSIONS ON TABLE 1.11A

Dependent Variable: ASSOCIATE vs. ASSISTANT

MARGINAL EFFECTS FROM PROBIT REGRESSIONS

SAMPLE	ALL	ALL	BALANCED PANEL	BALANCED PANEL
Model	(11')	(12')	(13')	(14')
PUBLICATION INDEX	0.00287	0.00201	0.00016	-0.00013
(based on the impact factor)	(0.00163)*	(0.00155)	(0.00085)	(0.00073)
VOLUME OF RESEARCH	0.00062	0.00056	0.00031	0.00021
	(0.00054)	(0.00050)	(0.00032)	(0.00025)
CITATIONS FROM OTHERS	-0.00096	-0.00082	0.00030	0.00026
	(0.00106)	(0.00106)	(0.00071)	(0.00069)
RANK WITHIN DEPARTMENT	-1.38602	-1.25320	-0.13706	-0.24755
(based on citations)	(0.62347)**	(0.59823)**	(0.32179)	(0.28909)
DISTANCE BETWEEN RANKS	-0.00050	-0.00038	-0.00038	-0.00025
(based on citations)	(0.00086)	(0.00083)	(0.00087)	(0.00050)
EXPERIENCE	0.16415	0.17461	0.06921	0.04060
	(0.06830)***	(0.06181)***	(0.06158)**	(0.04393)
$(\text{EXPERIENCE})^2$	-0.00248	-0.00283	-0.00157	-0.00016
	(0.00269)	(0.00238)	(0.00188)	(0.00124)
TOP10	-0.25926	-0.28813	-0.11727	-0.07810
	(0.12661)**	(0.11827)**	(0.10178)*	(0.07112)
AWARDS	-0.08281	-0.09291	-0.85549	-0.60231
	(0.43056)	(0.38670)	(0.54408)	(0.73443)
ADMINISTRATIVE DUTIES	-0.65828	-0.61047	-0.30430	-0.19807
	(0.12055)**	(0.13735)**	(0.45105)	(0.31835)

TABLE 1.11B (CONT'D)

FEMALE	-0.01974 (0.18004)	-0.13326 (0.17227)	-0.12977 (0.18019)	-0.07700 (0.10950)
THEORY	0.23645 (0.13222)*	–	0.04582 (0.05701)	–
ECONOMETRICS	-0.28542 (0.23457)	–	-0.07852 (0.21415)	–
MACRO/ FINANCE	0.10147 (0.15567)	–	0.12802 (0.10171)**	–
PUBLIC/ URBAN	-0.07723 (0.21188)	–	0.06283 (0.05585)	–
LABOR	-0.15619 (0.30895)	–	-0.22447 (0.38191)	–
UNIVERSITY OF ILLINOIS – URBANA CHAMPAIGN	0.03513 (0.31928)	0.10283 (0.25390)	0.10425 (0.07864)	0.10103 (0.07144)
INDIANA UNIVERSITY – BLOOMINGTON	-0.70282 (0.10592)***	-0.64480 (0.11736)***	-0.81413 (0.22273)***	-0.63174 (0.24722)***
UNIVERSITY OF IOWA	-0.36728 (0.26515)	-0.34908 (0.24815)	-0.22576 (0.35567)	-0.36176 (0.34219)
THE OHIO STATE UNIVERSITY	-0.28929 (0.24606)	-0.21071 (0.22828)	0.04482 (0.06392)	0.00842 (0.10833)
PURDUE UNIVERSITY	-0.58055 (0.20408)**	-0.50734 (0.22453)*	-0.86424 (0.19834)**	-0.79342 (0.26074)**

TABLE 1.11B (CONT'D)

UNIVERSITY OF MICHIGAN	0.00627	0.07314	0.00638	-0.01793
	(0.23399)	(0.21062)	(0.09534)	(0.11521)
UNIVERSITY OF MINNESOTA	-0.38173	-0.34299	-0.91563	-0.48889
	(0.27000)	(0.25508)	(0.15583)**	(0.41983)
UNIVERSITY OF WISCONSIN - MADISON	0.13037	0.08754	-0.17117	-0.05386
	(0.20789)	(0.22217)	(0.33944)	(0.17584)
YEAR 1995	0.04341	0.05333	0.03889	0.03862
	(0.12249)	(0.11954)	(0.05494)	(0.04866)
YEAR 1999	0.04606	0.04547	0.13218	0.09553
	(0.14466)	(0.14081)	(0.09098)**	(0.06749)*
Observations	212	212	135	135
Observed P	0.60	0.60	0.73	0.73
Predicted P(at x-bar)	0.69	0.67	0.95	0.94

Note: Standard errors in parentheses.
* significant at 10%; ** significant at 5%; *** significant at 1%

TABLE 1.12A – REGRESSION RESULTS ON THE EFFECT OF GRADUATING FROM A TOP TEN SCHOOL ON THE PROBABILITY OF PROMOTION (RESTRICTED SAMPLES)

Dependent Variable: ASSOCIATE vs. ASSISTANT

ESTIMATION METHOD: PROBIT

SAMPLE	EXCLUDES JUNIOR MOVERS		EXCLUDES EXITING JUNIOR FACULTY	
Model	(15)	(16)	(17)	(18)
PUBLICATION INDEX (based on the impact factor)	0.00342 (0.00567)	0.00191 (0.00512)	0.00710 (0.00476)	0.00515 (0.00438)
VOLUME OF RESEARCH	0.00152 (0.00190)	0.00127 (0.00171)	0.00193 (0.00155)	0.00151 (0.00140)
CITATIONS FROM OTHERS	-0.00184 (0.00381)	-0.00244 (0.00351)	-0.00267 (0.00301)	-0.00225 (0.00292)
RANK WITHIN DEPARTMENT (based on citations)	-3.83940 (2.40738)	-4.15838 (2.21342)*	-3.79841 (1.78946)**	-3.39863 (1.64373)**
DISTANCE BETWEEN RANKS (based on citations)	-0.00176 (0.00312)	-0.00050 (0.00261)	-0.00137 (0.00238)	-0.00098 (0.00227)
EXPERIENCE	0.63700 (0.24310)***	0.46458 (0.20729)**	0.48934 (0.17046)***	0.48343 (0.15588)***
$(\text{EXPERIENCE})^2$	-0.01004 (0.01030)	-0.00317 (0.00946)	-0.00801 (0.00721)	-0.00795 (0.00640)
TOP10	-0.67391 (0.48641)	-0.65550 (0.44100)	-0.70711 (0.36823)*	-0.77420 (0.34255)**
AWARDS	-0.61489 (1.25372)	-0.32410 (1.09731)	-0.15321 (1.13812)	-0.22581 (0.99498)

TABLE 1.12A (CONT'D)

ADMINISTRATIVE DUTIES	-1.26745 (1.06650)	-1.22826 (0.93531)	-2.10131 (0.88083)**	-1.83771 (0.82285)**
FEMALE	-0.64688 (0.66861)	-0.65234 (0.55096)	-0.09362 (0.50396)	-0.36546 (0.44669)
THEORY	0.68054 (0.65582)	–	0.74184 (0.55084)	–
ECONOMETRICS	-0.94634 (0.92254)	–	-0.90319 (0.65288)	–
MACRO/ FINANCE	1.10528 (0.66025)*	–	0.28978 (0.48654)	–
PUBLIC/ URBAN	0.37893 (0.66136)	–	-0.23544 (0.56490)	–
LABOR	-0.62511 (1.02571)	–	-0.40169 (0.77016)	–
UNIVERSITY OF ILLINOIS – URBANA CHAMPAIGN	0.57166 (1.06033)	0.45315 (0.93834)	0.08135 (0.94341)	0.29819 (0.79784)
INDIANA UNIVERSITY – BLOOMINGTON	-2.89902 (0.91839)***	-2.43921 (0.80558)***	-2.27896 (0.65083)***	-1.94006 (0.58622)***
UNIVERSITY OF IOWA	-1.63722 (1.03881)	-1.63302 (0.91708)*	-0.97097 (0.72198)	-0.91190 (0.67064)

TABLE 1.12A (CONT'D)

THE OHIO STATE UNIVERSITY	-0.53439	-0.47439	-0.76260	-0.53878
	(0.92298)	(0.87423)	(0.63888)	(0.57995)
PURDUE UNIVERSITY	-3.85965	-3.46308	-1.64817	-1.37549
	(1.34063)***	(1.26984)***	(0.80496)**	(0.76840)*
UNIVERSITY OF MICHIGAN	0.30592	0.28590	0.02298	0.21648
	(0.77367)	(0.71445)	(0.66613)	(0.63050)
UNIVERSITY OF MINNESOTA	-1.53563	-1.08811	-1.21656	-0.97732
	(1.03331)	(0.96091)	(0.80379)	(0.74387)
UNIVERSITY OF WISCONSIN - MADISON	0.21414	0.24451	0.45180	0.26602
	(0.91598)	(0.80570)	(0.74251)	(0.68419)
YEAR 1995	0.19359	0.19116	0.08703	0.13713
	(0.45858)	(0.41390)	(0.35800)	(0.34065)
YEAR 1999	0.29291	0.14143	0.11312	0.12254
	(0.58007)	(0.51169)	(0.42324)	(0.39901)
CONSTANT	-1.20706	0.29470	-0.19361	-0.28590
	(2.47483)	(2.24247)	(1.75873)	(1.59882)
Observations	163	163	206	206
Field dummies jointly insignificant ($\chi^2(5)$)	6.87		5.75	
Prob> χ^2	0.23		0.33	
Pseudo R-squared	0.70	0.66	0.65	0.63
Log-Likelihood	-33.04	-36.96	-48.24	-51.34

Note: Standard errors in parentheses;.
* significant at 10%; ** significant at 5%; *** significant at 1%

TABLE 1.12B – PARTIAL EFFECTS FROM REGRESSIONS ON TABLE 1.12A

Dependent Variable: ASSOCIATE vs. ASSISTANT

MARGINAL EFFECTS FROM PROBIT REGRESSIONS

SAMPLE	EXCLUDES JUNIOR MOVERS		EXCLUDES EXITING JUNIOR FACULTY	
Model	(15')	(16')	(17')	(18')
PUBLICATION INDEX	0.00116	0.00061	0.00245	0.00183
(based on the impact factor)	(0.00191)	(0.00163)	(0.00163)	(0.00154)
VOLUME OF RESEARCH	0.00052	0.00041	0.00067	0.00053
	(0.00065)	(0.00055)	(0.00053)	(0.00050)
CITATIONS FROM OTHERS	-0.00062	-0.00078	-0.00092	-0.00080
	(0.00127)	(0.00110)	(0.00103)	(0.00103)
RANK WITHIN DEPARTMENT	-1.29960	-1.33320	-1.31297	-1.20647
(based on citations)	(0.79221)	(0.68150)	(0.60850)	(0.58415)
DISTANCE BETWEEN RANKS	-0.00060	-0.00016	-0.00047	-0.00035
(based on citations)	(0.00108)	(0.00084)	(0.00083)	(0.00081)
EXPERIENCE	0.21562	0.14895	0.16915	0.17161
	(0.10607)	(0.08653)	(0.06915)	(0.06315)
$(\text{EXPERIENCE})^2$	-0.00340	-0.00102	-0.00277	-0.00282
	(0.00390)	(0.00318)	(0.00267)	(0.00241)
TOP10	-0.22550	-0.20832	-0.24416	-0.27329
	(0.15954)	(0.13515)	(0.12434)	(0.11648)
AWARDS	-0.23121	-0.11276	-0.05480	-0.08365
	(0.49382)	(0.40462)	(0.41929)	(0.38092)
ADMINISTRATIVE DUTIES	-0.47373	-0.45885	-0.66050	-0.60918
	(0.34689)	(0.32446)	(0.13344)	(0.15355)
FEMALE	-0.23363	-0.22674	-0.03282	-0.13516
	(0.25349)	(0.20185)	(0.17931)	(0.17041)
THEORY	0.20002	–	0.21919	–
	(0.16004)		(0.13122)	
ECONOMETRICS	-0.36085	–	-0.34523	–
	(0.34497)		(0.24525)	
MACRO/ FINANCE	0.29049	–	0.09478	–
	(0.15410)		(0.15223)	

TABLE 1.12B (CONT'D)

PUBLIC/ URBAN	0.11701	–	-0.08500	–
	(0.18632)		(0.21097)	
LABOR	-0.23446	–	-0.14922	–
	(0.40744)		(0.30113)	
UNIVERSITY OF ILLINOIS –	0.16754	0.12840	0.02763	0.09919
URBANA CHAMPAIGN	(0.26441)	(0.23402)	(0.31460)	(0.24608)
INDIANA UNIVERSITY –	-0.77981	-0.75196	-0.71332	-0.64805
BLOOMINGTON	(0.10496)	(0.13175)	(0.10827)	(0.12292)
UNIVERSITY OF IOWA	-0.58336	-0.58551	-0.36910	-0.34938
	(0.27417)	(0.25689)	(0.26739)	(0.24965)
THE OHIO STATE UNIVERSITY	-0.19671	-0.16704	-0.28697	-0.20355
	(0.35870)	(0.33135)	(0.24582)	(0.22647)
PURDUE UNIVERSITY	-0.80942	-0.81995	-0.58523	-0.50685
	(0.10009)	(0.10352)	(0.20963)	(0.23139)
UNIVERSITY OF MICHIGAN	0.09645	0.08508	0.00791	0.07343
	(0.22800)	(0.19857)	(0.22815)	(0.20427)
UNIVERSITY OF MINNESOTA	-0.55511	-0.40731	-0.45699	-0.37437
	(0.28563)	(0.35469)	(0.26707)	(0.26939)
UNIVERSITY OF WISCONSIN - MADISON	0.06837	0.07261	0.13812	0.08878
	(0.27338)	(0.22045)	(0.19537)	(0.21343)
YEAR 1995	0.06426	0.05997	0.02986	0.04816
	(0.14875)	(0.12653)	(0.12181)	(0.11784)
YEAR 1999	0.09530	0.04441	0.03857	0.04292
	(0.18097)	(0.15560)	(0.14203)	(0.13735)
Observations	163	163	206	206
Observed P	0.61	0.61	0.61	0.61
Predicted P(at x-bar)	0.72	0.75	0.70	0.69

Note: Standard errors in parentheses; * significant at 10%; ** significant at 5%; *** significant at 1%

TABLE 1.13 – REGRESSION RESULTS ON THE EFFECT OF GRADUATING FROM A TOP TEN SCHOOL ON THE PROBABILITY OF PROMOTION (WITH INTERACTIONS FOR MOVERS)

Dependent Variable: ASSOCIATE vs. ASSISTANT				
ESTIMATION METHOD: PROBIT				
SAMPLE	EXCLUDES JUNIOR MOVERS		EXCLUDES EXITING JUNIOR FACULTY	
Model	(19)	PARTIAL EFFECTS FROM (19)	(20)	PARTIAL EFFECTS FROM (20)
PUBLICATION INDEX (based on the impact factor)	0.00504 (0.00435)	0.00185 (0.00159)	0.00535 (0.00435)	0.00193 (0.00156)
VOLUME OF RESEARCH	0.00116 (0.00143)	0.00043 (0.00053)	0.00143 (0.00141)	0.00052 (0.00051)
CITATIONS FROM OTHERS	-0.00147 (0.00307)	-0.00054 (0.00113)	-0.00275 (0.00295)	-0.00099 (0.00106)
RANK WITHIN DEPARTMENT (based on citations)	-3.01691 (1.68877)*	-1.10946 (0.62105)	-3.90543 (1.67972)**	-1.40644 (0.60455)
DISTANCE BETWEEN RANKS (based on citations)	-0.00146 (0.00235)	-0.00054 (0.00087)	-0.00087 (0.00229)	-0.00031 (0.00083)
TOP10	-0.53270 (0.40630)	-0.19585 (0.14635)	-0.87976 (0.35280)**	-0.31410 (0.12010)
TOP10 * YMOVER	-0.45226 (0.81128)	-0.17508 (0.32197)	–	–
YMOVER	0.87229 (0.56895)	0.27862 (0.15564)	–	–
TOP10 * AMOVER	–	–	2.06034 (1.41239)	0.33290 (0.07171)
AMOVER	–	–	0.25598 (1.30774)	0.08649 (0.41084)

TABLE 1.13 (CONT'D)

EXPERIENCE	0.54195 (0.16027)***	0.19930 (0.06688)	0.49715 (0.15582)***	0.17904 (0.06353)
$(\text{EXPERIENCE})^2$	-0.00905 (0.00647)	-0.00333 (0.00252)	-0.00793 (0.00639)	-0.00286 (0.00243)
AWARDS	-0.14658 (1.01766)	-0.05522 (0.39094)	-0.38705 (1.02727)	-0.14781 (0.40571)
ADMINISTRATIVE DUTIES	-1.61319 (0.85112)*	-0.55233 (0.18327)	-1.79477 (0.82479)**	-0.59584 (0.15534)
FEMALE	-0.27312 (0.45739)	-0.10315 (0.17633)	-0.37666 (0.45442)	-0.14106 (0.17497)
UNIVERSITY OF ILLINOIS – URBANA CHAMPAIGN	0.66510 (0.84764)	0.21067 (0.22008)	0.34710 (0.83338)	0.11608 (0.25502)
INDIANA UNIVERSITY – BLOOMINGTON	-2.16114 (0.61085)***	-0.67024 (0.10212)	-2.03121 (0.59926)***	-0.65917 (0.11457)
UNIVERSITY OF IOWA	-0.90825 (0.70018)	-0.35006 (0.25498)	-1.24294 (0.72031)*	-0.46516 (0.23097)
THE OHIO STATE UNIVERSITY	-0.82585 (0.62201)	-0.31847 (0.23452)	-0.54996 (0.59485)	-0.20976 (0.23299)
PURDUE UNIVERSITY	-1.67472 (0.79941)**	-0.57793 (0.18338)	-1.43996 (0.78469)*	-0.52381 (0.22211)
UNIVERSITY OF MICHIGAN	0.38785 (0.65188)	0.13204 (0.20360)	0.28364 (0.63969)	0.09632 (0.20424)

TABLE 1.13 (CONT'D)

UNIVERSITY OF MINNESOTA	-0.93522	-0.35987	-1.01433	-0.38773
	(0.69968)	(0.25209)	(0.76380)	(0.27186)
UNIVERSITY OF WISCONSIN - MADISON	0.31271	0.10781	0.34462	0.11467
	(0.70783)	(0.22644)	(0.69577)	(0.21146)
YEAR 1995	0.14254	0.05193	0.16306	0.05802
	(0.34537)	(0.12410)	(0.34172)	(0.11959)
YEAR 1999	0.06546	0.02393	0.07572	0.02705
	(0.40569)	(0.14712)	(0.40207)	(0.14220)
CONSTANT	-1.24397		0.04959	
	(1.74493)		(1.63126)	
Observations	212	212	212	212
Pseudo R-squared	0.65		0.65	
Log-Likelihood	-50.33		-50.51	

Note: Standard errors in parentheses.
* significant at 10%; ** significant at 5%; *** significant at 1%

One other concern with the analysis of the handicapping phenomenon is that it hinges upon the particular list of top ten schools. The list used here does not depend on any particular ranking method since it was generated by combining a number of top-ten lists over time and across various studies. However, since the TOP10 variable is a dummy, it does not take into account varying degrees of graduating department quality. For example, this variable will treat equally someone graduating from a school ranked 11^{th} and someone graduating from a department ranked 31^{st}. The handicapping phenomenon would make more sense to emerge when comparing people graduating from very differently-ranked institutions. To deal with these concerns, and at the same time to establish the robustness of the results with respect to the handicapping hypothesis, the TOP10 variable is replaced by the continuous variable PHDRANK. This variable is constructed from the 1982 National Research Council (NRC) ranking of economics Ph.D. departments. This particular ranking was chosen because it is one of the most comprehensive from the different available rankings of economics departments and the source is more credible.[46]

The same models are estimated and the results with respect to the handicapping phenomenon remain the same. Models (21) and (22) in Table 1.14A estimate the base promotion model which includes all observations from junior faculty. The variable PHDRANK has a positive sign, indicating that junior faculty graduating from lower-ranked departments (departments for which PHDRANK receives higher numbers) are more likely to be promoted. Table 1.14B presents the partial effects which suggest that

[46] The 1993 NRC rankings of graduate departments are more accurate and comprehensive according to the source. However, the 1982 rankings appeared as a more relevant alternative for the current analysis, since most junior faculty under consideration graduated before 1993. The 1993 NRC rankings were also used, but the results did not change dramatically.

for each subsequent lower rank of graduating department, the probability of promotion increases by two percentage points (0.017-0.018). These results remain qualitatively the same when excluding faculty with missing salary observations (models (23) and (24)). The partial effect of PHDRANK, however, drops in this case to 0.005 for each step in graduating school rank.

When junior faculty who move in or out of the sample are excluded (models (25) and (26) in Table 1.15A), the effect of graduating school quality on the probability of promotion disappears. When only exiting junior faculty are excluded from the analysis, the effect of PHDRANK appears similar to the base case including all junior faculty. The estimates suggest a partial effect of 0.016 – 0.018 associated with each additional step in the ranking of Ph.D. university.

Table 1.16 presents results from regressions involving interaction terms between PHDRANK and a dummy indicating entering or exiting faculty (YMOVER) or only exiting faculty (AMOVER). Here the first interaction appears significant and positive, suggesting that the lower the department rank of junior faculty who enter or exit the sample (or do both) during 1992-1999, the higher the promotion probability is. The partial effect of this interaction is about 0.06. This result could be due to the fact that more mobile faculty command better employment terms (wages, promotions etc.). The second interaction, however, does not reveal any such effects.

TABLE 1.14A – REGRESSION RESULTS ON THE EFFECT OF GRADUATING SCHOOL QUALITY ON THE PROBABILITY OF PROMOTION

Dependent Variable: ASSOCIATE vs. ASSISTANT

ESTIMATION METHOD: PROBIT

SAMPLE	ALL	ALL	BALANCED PANEL	BALANCED PANEL
Model	(21)	(22)	(23)	(24)
PUBLICATION INDEX (based on the impact factor)	0.01130 (0.00533)**	0.00878 (0.00496)*	0.01210 (0.00983)	0.00900 (0.00810)
VOLUME OF RESEARCH	0.00151 (0.00164)	0.00161 (0.00148)	0.00090 (0.00231)	0.00081 (0.00215)
CITATIONS FROM OTHERS	-0.00640 (0.00356)*	-0.00618 (0.00353)*	-0.00158 (0.00716)	-0.00172 (0.00703)
RANK WITHIN DEPARTMENT (based on citations)	-5.67603 (2.08000)***	-5.48419 (2.01669)***	-3.68445 (3.54857)	-3.96759 (3.40202)
DISTANCE BETWEEN RANKS (based on citations)	-0.00023 (0.00240)	0.00042 (0.00235)	-0.00226 (0.00523)	-0.00097 (0.00453)
EXPERIENCE	0.41724 (0.15906)***	0.40979 (0.15433)***	0.34532 (0.30106)	0.15731 (0.26095)
$(\text{EXPERIENCE})^2$	-0.00570 (0.00677)	-0.00501 (0.00652)	-0.00053 (0.01457)	0.00757 (0.01327)
PHDRANK	0.04955 (0.02708)*	0.05234 (0.02630)**	0.07726 (0.04295)*	0.07624 (0.03865)**
AWARDS	-0.04890 (1.31356)	0.17115 (1.13065)	-2.13340 (2.53768)	-2.01538 (2.47366)
ADMINISTRATIVE DUTIES	-2.60135 (0.99405)***	-2.44674 (0.92683)***	-2.56455 (1.54738)*	-2.09948 (1.25129)*

TABLE 1.14A (CONT'D)

FEMALE	0.56992 (0.58247)	0.36249 (0.52438)	1.16838 (1.05617)	1.22045 (0.88755)
THEORY	0.40264 (0.57287)	–	-0.04313 (0.81861)	–
ECONOMETRICS	-0.69657 (0.68010)	–	-0.97280 (1.06348)	–
MACRO/ FINANCE	0.39240 (0.54175)	–	0.82181 (0.87467)	–
PUBLIC/ URBAN	-0.45544 (0.60840)	–	0.08805 (0.81976)	–
LABOR	-0.67258 (0.85034)	–	-1.90234 (1.17900)	–
UNIVERSITY OF ILLINOIS – URBANA CHAMPAIGN	-0.43913 (1.13078)	-0.55718 (0.95599)	1.75275 (2.55339)	1.84791 (2.48515)
INDIANA UNIVERSITY – BLOOMINGTON	-2.55606 (0.71951)***	-2.29946 (0.66572)***	-2.84902 (1.07779)***	-2.18968 (0.92332)**
UNIVERSITY OF IOWA	-1.23695 (0.74820)*	-1.25959 (0.71770)*	-1.03492 (1.08898)	-1.00290 (1.00675)
THE OHIO STATE UNIVERSITY	-0.85929 (0.65328)	-0.70330 (0.61674)	-0.37617 (1.20982)	-0.08360 (1.08588)
PURDUE UNIVERSITY	-2.02975 (0.90395)**	-1.85828 (0.88007)**	-3.73837 (1.51994)**	-3.43355 (1.51614)**

TABLE 1.14A (CONT'D)

UNIVERSITY OF MICHIGAN	0.60335	0.61834	1.49797	1.27339
	(0.77443)	(0.73492)	(1.24667)	(1.05687)
UNIVERSITY OF MINNESOTA†	-1.18959	-1.14982		
	(0.82259)	(0.76261)		
UNIVERSITY OF WISCONSIN - MADISON	0.64197	0.46029	0.19263	0.45681
	(0.79825)	(0.72976)	(1.27038)	(1.09742)
YEAR 1995	0.30309	0.27148	0.75480	0.61599
	(0.37148)	(0.35755)	(0.59441)	(0.52421)
YEAR 1999	0.32797	0.31071	1.84050	1.18946
	(0.45156)	(0.43075)	(1.01350)*	(0.75473)
CONSTANT	0.63556	0.61244	-1.88236	-0.58912
	(1.80555)	(1.69059)	(3.11724)	(2.92751)
Observations	197	197	120	120
Field dummies jointly insignificant ($\chi^2(5)$)	4.41		5.39	
Prob> χ^2	0.49		0.37	
Pseudo R-squared	0.66	0.65	0.64	0.60
Log-Likelihood	-44.44	-46.78	-23.82	-26.821

Note: Standard errors in parentheses; * significant at 10%; ** significant at 5%; *** significant at 1%

†There is only one associate professor from the University of Minnesota who has been in the sample during the entire period, but this observation is dropped because the person has graduated from a foreign university with undetermined rank (PHDRANK).

TABLE 1.14B – PARTIAL EFFECTS FROM REGRESSIONS ON TABLE 1.14A

Dependent Variable: ASSOCIATE vs. ASSISTANT

MARGINAL EFFECTS FROM PROBIT REGRESSIONS

SAMPLE	ALL	ALL	BALANCED PANEL	BALANCED PANEL
Model	(21')	(22')	(23')	(24')
PUBLICATION INDEX	0.00389	0.00301	0.00071	0.00053
(based on the impact factor)	(0.00183)	(0.00167)	(0.00075)	(0.00058)
VOLUME OF RESEARCH	0.00052	0.00055	0.00005	0.00005
	(0.00056)	(0.00050)	(0.00015)	(0.00014)
CITATIONS FROM OTHERS	-0.00220	-0.00212	-0.00009	-0.00010
	(0.00119)	(0.00117)	(0.00038)	(0.00038)
RANK WITHIN DEPARTMENT	-1.95349	-1.87854	-0.21561	-0.23468
(based on citations)	(0.69699)	(0.67058)	(0.23163)	(0.23921)
DISTANCE BETWEEN RANKS	-0.00008	0.00014	-0.00013	-0.00006
(based on citations)	(0.00083)	(0.00080)	(0.00038)	(0.00029)
EXPERIENCE	0.14360	0.14037	0.02021	0.00930
	(0.06364)	(0.06143)	(0.03593)	(0.02344)
$(\text{EXPERIENCE})^2$	-0.00196	-0.00172	-0.00003	0.00045
	(0.00246)	(0.00235)	(0.00088)	(0.00051)
PHDRANK	0.01705	0.01793	0.00452	0.00451
	(0.00927)	(0.00882)	(0.00481)	(0.00450)
AWARDS	-0.01703	0.05595	-0.48858	-0.44665
	(0.46239)	(0.35212)	(0.87720)	(0.84035)
ADMINISTRATIVE DUTIES	-0.71291	-0.70329	-0.65819	-0.49031
	(0.09444)	(0.10210)	(0.47488)	(0.43532)
FEMALE	0.17484	0.11583	0.04113	0.04285
	(0.15511)	(0.15422)	(0.04536)	(0.04643)
THEORY	0.12741	–	-0.00259	–
	(0.16440)		(0.05058)	
ECONOMETRICS	-0.26465	–	-0.12797	–
	(0.26549)		(0.25753)	

TABLE 1.14B (CONT'D)

MACRO/ FINANCE	0.12480	–	0.03472	–
	(0.15989)		(0.05286)	
PUBLIC/ URBAN	-0.16862	–	0.00487	–
	(0.23571)		(0.04325)	
LABOR	-0.25523	–	-0.41410	–
	(0.33560)		(0.43297)	
UNIVERSITY OF ILLINOIS –	-0.16256	-0.20809	0.04067	0.04228
URBANA CHAMPAIGN	(0.44021)	(0.37424)	(0.05203)	(0.05133)
INDIANA UNIVERSITY –	-0.74382	-0.71493	-0.71493	-0.49183
BLOOMINGTON	(0.09612)	(0.11032)	(0.30209)	(0.30105)
UNIVERSITY OF IOWA	-0.46322	-0.47065	-0.13243	-0.12659
	(0.25166)	(0.23967)	(0.24469)	(0.22117)
THE OHIO STATE UNIVERSITY	-0.32299	-0.26242	-0.02912	-0.00526
	(0.24857)	(0.23855)	(0.12411)	(0.07287)
PURDUE UNIVERSITY	-0.66891	-0.63598	-0.92047	-0.87568
	(0.17325)	(0.19663)	(0.15678)	(0.21968)
UNIVERSITY OF MICHIGAN	0.17599	0.17830	0.03665	0.03448
	(0.18675)	(0.17352)	(0.04393)	(0.04113)
UNIVERSITY OF MINNESOTA	-0.44761	-0.43368		
	(0.28078)	(0.26558)		
UNIVERSITY OF WISCONSIN - MADISON	0.18352	0.13883	0.00968	0.01902
	(0.17977)	(0.18952)	(0.05144)	(0.03151)
YEAR 1995	0.10151	0.09075	0.03648	0.03084
	(0.12061)	(0.11558)	(0.04644)	(0.03922)
YEAR 1999	0.10771	0.10178	0.06455	0.04567
	(0.13903)	(0.13177)	(0.06513)	(0.05005)
Observations	197	197	120	120
Observed P	0.61	0.61	0.76	0.76
Predicted P(at x-bar)	0.71	0.71	0.97	0.97

Note: Standard errors in parentheses.

TABLE 1.15A – REGRESSION RESULTS ON THE EFFECT OF GRADUATING SCHOOL QUALITY ON THE PROBABILITY OF PROMOTION (RESTRICTED SAMPLES)

Dependent Variable: ASSOCIATE vs. ASSISTANT

ESTIMATION METHOD: PROBIT

SAMPLE	EXCLUDES JUNIOR MOVERS		EXCLUDES EXITING JUNIOR FACULTY	
Model	(25)	(26)	(27)	(28)
PUBLICATION INDEX (based on the impact factor)	0.00990 (0.00731)	0.00768 (0.00625)	0.01054 (0.00547)*	0.00863 (0.00505)*
VOLUME OF RESEARCH	0.00076 (0.00215)	0.00113 (0.00185)	0.00164 (0.00167)	0.00150 (0.00150)
CITATIONS FROM OTHERS	-0.00576 (0.00451)	-0.00651 (0.00452)	-0.00628 (0.00352)*	-0.00608 (0.00350)*
RANK WITHIN DEPARTMENT (based on citations)	-5.63622 (2.68416)**	-5.97131 (2.66840)**	-5.47635 (2.07131)***	-5.32180 (2.01659)***
DISTANCE BETWEEN RANKS (based on citations)	-0.00079 (0.00310)	0.00087 (0.00279)	-0.00014 (0.00238)	0.00047 (0.00233)
EXPERIENCE	0.56644 (0.24150)**	0.37183 (0.21540)*	0.42620 (0.16177)***	0.40660 (0.15868)**
$(\text{EXPERIENCE})^2$	-0.00742 (0.01045)	0.00120 (0.01056)	-0.00606 (0.00681)	-0.00502 (0.00668)
PHDRANK	0.03960 (0.03039)	0.03649 (0.02750)	0.04896 (0.02725)*	0.05276 (0.02632)**
AWARDS	-0.38146 (1.44546)	0.04755 (1.27146)	0.03575 (1.31810)	0.16931 (1.12769)

TABLE 1.15A (CONT'D)

ADMINISTRATIVE DUTIES	-2.02022 (1.25181)	-2.00913 (1.12275)*	-2.56585 (0.98956)***	-2.37435 (0.92595)**
FEMALE	0.26269 (0.76939)	0.39296 (0.68007)	0.56844 (0.58272)	0.35463 (0.52409)
THEORY	0.26038 (0.68962)	–	0.35783 (0.57478)	–
ECONOMETRICS	-0.85736 (0.95612)	–	-0.83705 (0.73533)	–
MACRO/ FINANCE	1.23640 (0.77626)	–	0.30669 (0.55465)	–
PUBLIC/ URBAN	0.25593 (0.71509)	–	-0.49361 (0.61031)	–
LABOR	-0.88612 (1.16671)	–	-0.67628 (0.83582)	–
UNIVERSITY OF ILLINOIS – URBANA CHAMPAIGN	-0.02985 (1.24159)	-0.12526 (1.11006)	-0.43975 (1.12223)	-0.53639 (0.94994)
INDIANA UNIVERSITY – BLOOMINGTON	-3.23539 (0.99653)***	-2.74234 (0.89781)***	-2.50751 (0.71707)***	-2.25823 (0.66335)***
UNIVERSITY OF IOWA	-1.55718 (1.02028)	-1.52504 (0.93999)	-1.20771 (0.74450)	-1.23553 (0.71363)*

TABLE 1.15A (CONT'D)

THE OHIO STATE UNIVERSITY	-0.79390	-0.70453	-0.84822	-0.68782
	(1.00886)	(0.95708)	(0.64990)	(0.61332)
PURDUE UNIVERSITY	-4.19935	-3.86244	-1.98752	-1.84268
	(1.47894)***	(1.48173)***	(0.89568)**	(0.87481)**
UNIVERSITY OF MICHIGAN	1.08177	1.05078	0.63357	0.62393
	(0.95706)	(0.87306)	(0.77427)	(0.73305)
UNIVERSITY OF MINNESOTA	-0.61880	-0.58878	-1.42098	-1.32486
	(1.25036)	(1.17067)	(0.89937)	(0.84323)
UNIVERSITY OF WISCONSIN - MADISON	0.49361	0.70059	0.71086	0.47582
	(0.99583)	(0.89227)	(0.80389)	(0.72633)
YEAR 1995	0.48398	0.31917	0.27055	0.27104
	(0.52381)	(0.46540)	(0.37631)	(0.35968)
YEAR 1999	0.48085	0.18704	0.29585	0.30271
	(0.62399)	(0.56140)	(0.45254)	(0.42906)
CONSTANT	-0.67214	0.80537	0.51318	0.53191
	(2.49897)	(2.37617)	(1.80330)	(1.71702)
Observations	150	150	191	191
Field dummies jointly insignificant ($\chi^2(5)$)	5.69		4.39	
Prob> χ^2	0.34		0.49	
Pseudo R-squared	0.71	0.68	0.65	0.63
Log-Likelihood	-29.04	-32.36	-44.09	-46.40

Note: Standard errors in parentheses; * significant at 10%; ** significant at 5%; *** significant at 1%

TABLE 1.15B – PARTIAL EFFECTS FROM REGRESSIONS ON TABLE 1.15A

Dependent Variable: ASSOCIATE vs. ASSISTANT

MARGINAL EFFECTS FROM PROBIT REGRESSIONS

SAMPLE	EXCLUDES JUNIOR MOVERS		EXCLUDES EXITING JUNIOR FACULTY	
Model	(25')	(26')	(27')	(28')
PUBLICATION INDEX	0.00324	0.00227	0.00351	0.00287
(based on the impact factor)	(0.00245)	(0.00182)	(0.00180)	(0.00164)
VOLUME OF RESEARCH	0.00025	0.00033	0.00055	0.00050
	(0.00070)	(0.00054)	(0.00055)	(0.00049)
CITATIONS FROM OTHERS	-0.00188	-0.00192	-0.00209	-0.00202
	(0.00141)	(0.00124)	(0.00113)	(0.00112)
RANK WITHIN DEPARTMENT	-1.84322	-1.76552	-1.82367	-1.76718
(based on citations)	(0.83848)	(0.74083)	(0.66734)	(0.64434)
DISTANCE BETWEEN RANKS	-0.00026	0.00026	-0.00005	0.00016
(based on citations)	(0.00103)	(0.00081)	(0.00079)	(0.00077)
EXPERIENCE	0.18524	0.10994	0.14193	0.13502
	(0.10493)	(0.08635)	(0.06341)	(0.06189)
$(\text{EXPERIENCE})^2$	-0.00243	0.00036	-0.00202	-0.00167
	(0.00378)	(0.00305)	(0.00241)	(0.00234)
PHDRANK	0.01295	0.01079	0.01630	0.01752
	(0.00988)	(0.00795)	(0.00898)	(0.00850)
AWARDS	-0.13609	0.01383	0.01179	0.05348
	(0.54682)	(0.36435)	(0.43048)	(0.33836)
ADMINISTRATIVE DUTIES	-0.66812	-0.68038	-0.72582	-0.70945
	(0.23130)	(0.24282)	(0.10095)	(0.11498)
FEMALE	0.08208	0.10687	0.16777	0.10962
	(0.22800)	(0.16939)	(0.14811)	(0.14865)
THEORY	0.08050	–	0.11017	–
	(0.19943)		(0.16178)	
ECONOMETRICS	-0.32326	–	-0.31639	–
	(0.37053)		(0.28407)	
MACRO/ FINANCE	0.29812	–	0.09569	–
	(0.16207)		(0.16271)	

TABLE 1.15B (CONT'D)

PUBLIC/ URBAN	0.07863	–	-0.17945	–
	(0.20693)		(0.23453)	
LABOR	-0.33175	–	-0.25259	–
	(0.45653)		(0.33004)	
UNIVERSITY OF ILLINOIS –	-0.00983	-0.03832	-0.15908	-0.19604
URBANA CHAMPAIGN	(0.41165)	(0.35026)	(0.43129)	(0.36835)
INDIANA UNIVERSITY –	-0.80360	-0.79960	-0.75137	-0.71851
BLOOMINGTON	(0.10520)	(0.12798)	(0.10206)	(0.11748)
UNIVERSITY OF IOWA	-0.56365	-0.55021	-0.45100	-0.46047
	(0.29703)	(0.29772)	(0.25900)	(0.24566)
THE OHIO STATE UNIVERSITY	-0.29169	-0.24181	-0.31356	-0.25164
	(0.39341)	(0.36492)	(0.24788)	(0.23592)
PURDUE UNIVERSITY	-0.83248	-0.85788	-0.66848	-0.63763
	(0.10390)	(0.10599)	(0.18707)	(0.20669)
UNIVERSITY OF MICHIGAN	0.25495	0.21704	0.17507	0.17231
	(0.16917)	(0.14043)	(0.17299)	(0.16421)
UNIVERSITY OF MINNESOTA	-0.22899	-0.20435	-0.52233	-0.49227
	(0.49486)	(0.45154)	(0.27272)	(0.27204)
UNIVERSITY OF WISCONSIN - MADISON	0.13784	0.15761	0.18946	0.13723
	(0.22752)	(0.14755)	(0.16122)	(0.17780)
YEAR 1995	0.14955	0.09047	0.08770	0.08760
	(0.15213)	(0.12801)	(0.11834)	(0.11219)
YEAR 1999	0.14564	0.05350	0.09430	0.09608
	(0.17249)	(0.15091)	(0.13579)	(0.12702)
Observations	150	150	191	191
Observed P	0.62	0.62	0.62	0.62
Predicted P(at x-bar)	0.74	0.78	0.73	0.73

Note: Standard errors in parentheses

TABLE 1.16 – REGRESSION RESULTS ON THE EFFECT OF GRADUATING SCHOOL QUALITY ON THE PROBABILITY OF PROMOTION

Dependent Variable: ASSOCIATE vs. ASSISTANT

ESTIMATION METHOD: PROBIT

SAMPLE	EXCLUDES JUNIOR MOVERS		EXCLUDES EXITING JUNIOR FACULTY	
Model	(29)	PARTIAL EFFECTS FROM (29)	(30)	PARTIAL EFFECTS FROM (30)
PUBLICATION INDEX (based on the impact factor)	0.00801 (0.00567)	0.00262 (0.00179)	0.00898 (0.00503)*	0.00307 (0.00168)
VOLUME OF RESEARCH	0.00192 (0.00169)	0.00063 (0.00054)	0.00152 (0.00150)	0.00052 (0.00051)
CITATIONS FROM OTHERS	-0.00698 (0.00408)*	-0.00228 (0.00125)	-0.00617 (0.00352)*	-0.00211 (0.00117)
RANK WITHIN DEPARTMENT (based on citations)	-6.50913 (2.36502)***	-2.12634 (0.73703)	-5.39995 (2.01711)***	-1.84525 (0.66668)
DISTANCE BETWEEN RANKS (based on citations)	0.00028 (0.00260)	0.00009 (0.00084)	0.00045 (0.00234)	0.00015 (0.00080)
PHDRANK	0.03235 (0.02832)	0.01057 (0.00900)	0.05361 (0.02644)**	0.01832 (0.00881)
PHDRANK * YMOVER	0.19848 (0.08026)**	0.06484 (0.02674)	–	–
YMOVER	-0.69483 (0.89020)	-0.24648 (0.32800)	–	–
PHDRANK * AMOVER	–	–	-0.12111 (0.28494)	-0.04139 (0.09694)
AMOVER	–	–	2.34469 (4.36048)	0.31148 (0.10099)

TABLE 1.16 (CONT'D)

EXPERIENCE	0.46418	0.15163	0.41347	0.14129
	(0.19258)**	(0.08096)	(0.15820)***	(0.06265)
$(EXPERIENCE)^2$	-0.00200	-0.00065	-0.00521	-0.00178
	(0.00902)	(0.00303)	(0.00666)	(0.00239)
AWARDS	0.26800	0.08036	0.16909	0.05516
	(1.27738)	(0.34968)	(1.13499)	(0.35287)
ADMINISTRATIVE DUTIES	-2.31325	-0.70759	-2.43628	-0.70344
	(1.03880)**	(0.14615)	(0.92395)***	(0.10365)
FEMALE	0.41870	0.12487	0.37859	0.12021
	(0.61696)	(0.16389)	(0.52519)	(0.15261)
UNIVERSITY OF ILLINOIS – URBANA CHAMPAIGN	-0.44236	-0.15803	-0.54162	-0.20171
	(1.10400)	(0.41970)	(0.95742)	(0.37439)
INDIANA UNIVERSITY – BLOOMINGTON	-3.38450	-0.81949	-2.28726	-0.71397
	(0.84925)***	(0.08721)	(0.66445)***	(0.11152)
UNIVERSITY OF IOWA	-1.70991	-0.60685	-1.24116	-0.46434
	(0.80023)**	(0.21560)	(0.71651)*	(0.24166)
THE OHIO STATE UNIVERSITY	-1.31418	-0.48193	-0.70140	-0.26135
	(0.73516)*	(0.25407)	(0.61702)	(0.23862)
PURDUE UNIVERSITY	-3.69284	-0.82612	-1.85371	-0.63543
	(1.20509)***	(0.09169)	(0.87846)**	(0.19771)
UNIVERSITY OF MICHIGAN	0.93623	0.22525	0.63978	0.18256
	(0.81241)	(0.14068)	(0.73533)	(0.17017)
UNIVERSITY OF MINNESOTA	-1.90802	-0.65324	-1.34322	-0.49816
	(0.87006)**	(0.19571)	(0.84794)	(0.26911)

TABLE 1.16 (CONT'D)

UNIVERSITY OF WISCONSIN - MADISON	0.62373	0.16684	0.47771	0.14282
	(0.81064)	(0.16971)	(0.73012)	(0.18643)
YEAR 1995	0.31088	0.09840	0.28189	0.09389
	(0.41023)	(0.12419)	(0.35801)	(0.11523)
YEAR 1999	0.37120	0.11412	0.31160	0.10178
	(0.49712)	(0.13555)	(0.43071)	(0.13136)
CONSTANT	0.81680		0.51276	
	(2.08295)		(1.71883)	
Observations	197	197	197	197
Pseudo R-squared	0.70		0.65	
Log-Likelihood	-39.83		-46.58	

Note: Standard errors in parentheses;
* significant at 10%; ** significant at 5%; *** significant at 1%

To summarize, the handicapping phenomenon emerges in a number of specifications and is, for the most part, robust to sample selection and the particular variable used to capture the quality of graduating department. What appears to be the case is that people graduating from prominent institutions signal higher ability levels. In order to encourage people graduating from less highly-ranked departments to perform optimally, employers give them a head start by raising their chances of promotion. People from less highly-ranked departments are expected to have a higher chance of promotion of 0.26, according to the partial effect of TOP10 in the third column of Table 1.8A, and 0.28 according to the respective estimate in Table 1.8B.[47] The fact that this result appears only in the early stages (the promotion to Associate) further supports this theory. The reason for this is that the need to level the playing field most likely disappears after the beginning stages of the contest. After the junior stage, more information becomes available about the true abilities of the faculty members, and mobility and up-or-out contract features work to homogenize the pool of contestants for the next stages of the tournament.

The next hypothesis has been widely used to test the effect of tournaments on behavior:

Hypothesis 4: Larger salary spreads induce larger productivity spreads.

The problem with testing this hypothesis is that a simple regression of productivity spreads on salary spreads would establish correlation between the two measures but not

[47] Intuitively, the way the observed handicapping phenomenon occurs in practice could be as follows: when two similar Assistant Professors are hired, one of which has graduated from a top-ten institution and the other from a less highly ranked department, the employer's expectations about their performance may differ. The Assistant graduating from the better school is expected to perform better, so if his/her performance only equals the performance of the other contestant, chances are that this will reflect negatively on his/her promotion probability. This story is consistent with the fact that the group graduating

causality. It could very well be the case that large productivity spreads induce large salary spreads, in which case nothing can be said about the incentive effects of tournaments. If, however, the productivity spreads are regressed on the salary spreads of the previous period, something can be said about the causality aspect of the relationship. In such a regression, it would be unreasonable to conclude that the future productivity spreads will have caused the current salary spreads. The only reasonable interpretation would involve salary differences driving productivity changes. Model (11) below tests this:

$$(11)\ \text{Spread}\ (P^{k}_{j,\,t+\Delta t}) = \beta_0 + \beta_1\ \text{Spread}\ (W^{k}_{j,\,t})$$

where: P = productivity
W = wage
j = department
t = 1992, 1995, 1999
k = Assistant vs. Associate, Associate vs. Professor, Assistant vs. Professor.

The coefficient of interest in (11) is β_1, which, if confirmed positive, lends support to the incentive effects of compensation tournaments. Model (11) is estimated both by pooled OLS and by the fixed effects method. The latter estimation is intended to hold constant department-specific factors influencing publications and salaries (such as department quality, the relative importance placed on research vs. other duties, department funding in general and funding of research activity, etc.). The spread variables are measured as the logarithm of the ratio of average salaries or performance variables between two grade levels. The dependent variable here is the spread in productivity, since, according to the theory, wage spreads would be driving productivity

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from top-ten institutions appears to have higher publication measures but similar salaries and promotion rates.

differences, not the other way around. Five different productivity spreads are calculated, based on the volume of publications, the two different publication indexes, total article pages (since this is the measure of volume most often used in the literature) and citations from others. In order to be able to infer some causality in the results, the salary spreads are lagged one period. Thus, the productivity spreads in 1995 and 1999 are analyzed with respect to the salary spreads in 1992 and 1995, respectively. An additional reason for lagging the salary spreads this way is that, if these spreads really induce greater effort, they would only do so with a lag. The data seem to reveal a significant correlation between larger lagged spreads in salaries and larger production differentials. Tables 1.17, 1.18, and 1.19 present regression results from model (11) for the case of Assistants vs. Associates, Associates vs. Professors and Assistant vs. Professors, respectively. It appears that the incentive effects are present in the case of Assistants vs. Associates, and Assistants vs. Professors. The elasticity in the case of Assistant vs. Associate Professors, ranges from 2.81 to 6.89 (Table 1.17). With the exception of the case when productivity is measured by volume of research where the correlation appears insignificant, the results are quite consistent across productivity measures. The fixed effects method, interestingly estimates larger elasticities, but with expected larger standard errors. These findings suggest that a one-percent change in the salary spread is expected to generate a 2.81- to 6.89-percent change in the productivity spread.

The elasticity estimates in the case of Associates vs. Professors are smallest and insignificant for the most part. The only exception is the case where productivity is measured by number of citations and the estimated elasticity for this case is 2.3-3.7, depending on the method used.

The elasticity of response when comparing Assistants to Professors ranges from 2.48 to 6.03, and is statistically significant only when the estimation is performed using OLS. Given the very small sample sizes in this part of the analysis (17-18), the fixed effects method is expected to yield large standard errors.

TABLE 1.17 –DEPARTMENTAL- LEVEL REGRESSION RESULTS ON THE EFFECT OF SALARY SPREADS ON PERFORMANCE (ASSISTANT PROFESSORS VS. ASSOCIATE PROFESSORS)

Dependent Variable: $Log\left(\frac{\overline{P}_{assoc}}{\overline{P}_{assist}}\right)_{i,t+\Delta t}$

	P = Impact Publication Index		P = Citation Publication Index		P = Volume of Publications		P = Total Article Pages		P = Cites From Others	
	OLS	FE	OLS	FE	OLS	FE	OLS	FE	OLS	FE
$Log\left(\frac{\overline{W}_{assoc}}{\overline{W}_{assist}}\right)_{i,t}$	4.12 (1.59)**	5.19 (2.11)**	4.98 (1.97)**	6.89 (3.33)*	2.98 (1.72)	3.11 (2.04)	2.81 (1.19)**	4.65 (2.00)*	5.22 (2.73)*	4.38 (4.03)
Constant	0.46 (0.32)	0.29 (0.35)	0.32 (0.39)	0.03 (0.55)	0.35 (0.34)	0.33 (0.34)	0.45 (0.24)*	0.16 (0.33)	0.43 (0.55)	0.56 (0.67)
Observations	17	17	17	17	17	17	17	17	17	17
R-squared	0.31	0.46	0.30	0.38	0.17	0.25	0.27	0.43	0.20	0.14

Note: Standard errors in parentheses.
* significant at 10%; ** significant at 5%; *** significant at 1%

TABLE 1.18 –DEPARTMENTAL- LEVEL REGRESSION RESULTS ON THE EFFECT OF SALARY SPREADS ON PERFORMANCE (ASSOCIATE PROFESSORS VS. PROFESSORS)

Dependent Variable: $Log\left(\frac{\overline{P}_{prof}}{\overline{P}_{assoc}}\right)_{i,t+\Delta t}$

	P = Impact Publication Index		P = Citation Publication Index		P = Volume of Publications		P = Total Article Pages		P = Cites From Others	
	OLS	FE	OLS	FE	OLS	FE	OLS	FE	OLS	FE
$Log\left(\frac{\overline{W}_{prof}}{\overline{W}_{assoc}}\right)_{i,t}$	0.75 (1.13)	-0.62 (1.07)	0.15 (1.12)	-0.23 (1.43)	0.97 (1.47)	0.81 (1.39)	0.44 (0.89)	-0.69 (0.80)	3.67 (1.55)**	2.32 (1.02)*
Constant	0.25 (0.43)	0.76 (0.40)*	0.52 (0.43)	0.67 (0.53)	0.42 (0.56)	0.48 (0.52)	0.40 (0.34)	0.82 (0.30)**	-0.19 (0.59)	0.31 (0.38)
Observations	17	17	17	17	17	17	17	17	17	17
R-squared	0.03	0.05	0.00	0.00	0.03	0.05	0.02	0.10	0.27	0.43

Note: Standard errors in parentheses.
* significant at 10%; ** significant at 5%; *** significant at 1%

TABLE 1.19 –DEPARTMENTAL- LEVEL REGRESSION RESULTS ON THE EFFECT OF SALARY SPREADS ON PERFORMANCE (ASSISTANT PROFESSORS VS. PROFESSORS)

Dependent Variable: $Log\left(\frac{\overline{P}_{prof}}{\overline{P}_{assist}}\right)_{i,t+\Delta t}$

	P = Impact Publication Index		P = Citation Publication Index		P = Volume of Publications		P = Total Article Pages		P = Cites From Others	
	OLS	FE	OLS	FE	OLS	FE	OLS	FE	OLS	FE
$Log\left(\frac{\overline{W}_{prof}}{\overline{W}_{assist}}\right)_{i,t}$	2.85 (1.19)**	2.50 (2.55)	3.40 (1.49)**	2.59 (3.44)	3.21 (1.21)**	1.25 (2.14)	2.48 (1.04)**	1.88 (2.02)	6.03 (1.92)***	3.99 (3.81)
Constant	0.14 (0.65)	0.32 (1.35)	-0.08 (0.81)	0.35 (1.83)	-0.07 (0.66)	0.96 (1.14)	0.12 (0.57)	0.43 (1.07)	-0.75 (1.05)	0.32 (2.02)
Observations	18	18	18	18	18	18	18	18	18	18
R-squared	0.26	0.11	0.25	0.07	0.31	0.04	0.26	0.10	0.38	0.12

Note: Standard errors in parentheses.
* significant at 10%; ** significant at 5%; *** significant at 1%

One problem with Model (11) is that it does not control for the original spreads in productivity, which may be due to ability differences alone, and not to tournament effects. Model (12) corrects this by adding the spread in the previous period as an explanatory variable:

$$(12)\ \text{Spread}\ (P^{k}_{j,\,t+\Delta t}) = \beta_0 + \beta_1\ \text{Spread}\ (W^{k}_{j,\,t}) + \beta_2\ \text{Spread}\ (P^{k}_{j,\,t})$$

By conditioning the correlation between productivity and salary spreads on past productivity spreads, the future spreads in productivity can be more accurately interpreted as a behavioral change as a result of the tournament structure. Tables 1.20, 1.21, and 1.22 display the findings. As expected, the elasticities estimated in this case are smaller in magnitude than those presented in Tables 1.17, 1.18, and 1.19 respectively, confirming the conjecture that part of the correlation is actually due to the underlying population variation. However, the incentive effects still come through for the cases of Assistants vs. Associates (with elasticities varying from 2.31 to 11.12) and Assistants vs. Professors (with elasticities varying from 1.21 to 4.57).

There may be a simple explanation of why the incentive effects only appear in the case of Assistants versus each of the other two grade levels. The institution of tenure and the up-or-out contract feature in the academic market enhance the importance of the first promotion. Therefore, the theoretical implications will be more apparent at the junior stage. The up-or-out rule, in addition, ensures that people with sub-standard performance leave the pool of contestants. Therefore, beyond the junior stage, the faculty employed in a department are a more homogeneous group. As a result, the spreads in performance and salaries may not vary enough to estimate the desired correlation. This does not mean

that tournament incentives no longer exist beyond the junior stage. At this stage the necessary incentives are generated by periodical raises in salary, rather than promotions.

Another reason why the incentive effect is not observed for the case of Associates vs. Professors, may be related to the fact that wage compression is one of the by-products of tournaments. A relative compensation system may create incentives to reduce cooperation and even engage in sabotage. In such situations, a wage compression may be necessary. If the observed salaries are conditioned on this potential adverse effect, their variation will be smaller, and it may not be possible to estimate the relationship of interest. This hypothesis is tested next.

TABLE 1.20 –DEPARTMENTAL- LEVEL REGRESSION RESULTS ON THE EFFECT OF SALARY SPREADS ON PERFORMANCE (ASSISTANT PROFESSORS VS. ASSOCIATE PROFESSORS)

Dependent Variable: $Log\left(\frac{\overline{P}_{assoc}}{\overline{P}_{assist}}\right)_{i,t+\Delta t}$

	P = Impact Publication Index		P = Citation Publication Index		P = Volume of Publications		P = Total Article Pages		P = Cites From Others	
	OLS	FE	OLS	FE	OLS	FE	OLS	FE	OLS	FE
$Log\left(\frac{\overline{W}_{assoc}}{\overline{W}_{assist}}\right)_{i,t}$	3.25 (1.71)*	5.79 (1.99)**	4.24 (2.08)*	11.12 (3.09)**	2.31 (1.96)	3.32 (2.35)	2.26 (1.29)	5.21 (1.84)**	2.84 (3.51)	6.38 (2.82)*
$Log\left(\frac{\overline{P}_{assoc}}{\overline{P}_{assist}}\right)_{i,t}$	0.24 (0.20)	0.31 (0.21)	0.20 (0.19)	0.67 (0.28)*	0.15 (0.20)	0.05 (0.19)	0.15 (0.14)	0.27 (0.17)	0.45 (0.46)	0.13 (0.37)
Constant	0.33 (0.33)	-0.13 (0.43)	0.22 (0.40)	-1.38 (0.72)	0.31 (0.35)	0.25 (0.47)	0.41 (0.24)	-0.14 (0.35)	0.36 (0.62)	0.15 (0.68)
Observations	17	17	17	17	17	17	17	17	16	16
R-squared	0.38	0.61	0.35	0.69	0.20	0.26	0.33	0.61	0.22	0.51

Note: Standard errors in parentheses.
* significant at 10%; ** significant at 5%; *** significant at 1%

TABLE 1.21 –DEPARTMENTAL- LEVEL REGRESSION RESULTS ON THE EFFECT OF SALARY SPREADS ON PERFORMANCE (ASSOCIATE PROFESSORS VS. PROFESSORS)

Dependent Variable: $Log\left(\frac{\overline{P}_{prof}}{\overline{P}_{assoc}}\right)_{i,t+\Delta t}$

	P = Impact Publication Index		P = Citation Publication Index		P = Volume of Publications		P = Total Article Pages		P = Cites From Others	
	OLS	FE	OLS	FE	OLS	FE	OLS	FE	OLS	FE
$Log\left(\frac{\overline{W}_{prof}}{\overline{W}_{assoc}}\right)_{i,t}$	-0.17 (1.08)	-0.73 (1.20)	-0.35 (1.03)	-0.29 (1.67)	0.86 (1.52)	0.34 (1.71)	0.41 (0.94)	-0.58 (0.92)	0.48 (0.94)	2.12 (1.13)
$Log\left(\frac{\overline{P}_{prof}}{\overline{P}_{assoc}}\right)_{i,t}$	0.40 (0.17)**	-0.05 (0.16)	0.30 (0.14)**	-0.02 (0.19)	0.10 (0.18)	-0.07 (0.13)	0.02 (0.11)	0.02 (0.07)	0.74 (0.12)***	0.43 (0.27)
Constant	0.35 (0.38)	0.83 (0.49)	0.50 (0.38)	0.70 (0.68)	0.38 (0.58)	0.71 (0.70)	0.39 (0.35)	0.75 (0.37)*	0.30 (0.31)	-0.03 (0.37)
Observations	17	17	17	17	17	17	17	17	16	16
R-squared	0.29	0.06	0.25	0.01	0.05	0.09	0.02	0.11	0.82	0.70

Note: Standard errors in parentheses.
* significant at 10%; ** significant at 5%; *** significant at 1%

TABLE 1.22 –DEPARTMENTAL- LEVEL REGRESSION RESULTS ON THE EFFECT OF SALARY SPREADS ON PERFORMANCE (ASSISTANT PROFESSORS VS. PROFESSORS)

Dependent Variable: $Log\left(\frac{\overline{P}_{prof}}{\overline{P}_{assist}}\right)_{i,t+\Delta t}$

	P = Impact Publication Index		P = Citation Publication Index		P = Volume of Publications		P = Total Article Pages		P = Cites From Others	
	OLS	FE	OLS	FE	OLS	FE	OLS	FE	OLS	FE
$Log\left(\frac{\overline{W}_{prof}}{\overline{W}_{assist}}\right)_{i,t}$	2.44 (1.26)*	2.62 (2.73)	3.21 (1.55)*	2.35 (3.61)	2.41 (1.27)*	1.21 (2.29)	1.66 (1.08)	1.89 (2.17)	4.57 (2.19)*	4.14 (4.08)
$Log\left(\frac{\overline{P}_{prof}}{\overline{P}_{assist}}\right)_{i,t}$	0.21 (0.20)	-0.13 (0.42)	0.15 (0.23)	-0.27 (0.47)	0.32 (0.21)	-0.07 (0.35)	0.35 (0.19)*	-0.00 (0.46)	0.49 (0.37)	-0.18 (0.60)
Constant	0.01 (0.66)	0.47 (1.52)	-0.24 (0.87)	0.97 (2.18)	-0.22 (0.64)	1.10 (1.40)	0.01 (0.54)	0.44 (1.30)	-0.95 (1.03)	0.59 (2.33)
Observations	18	18	18	18	18	18	18	18	18	18
R-squared	0.31	0.12	0.27	0.11	0.40	0.05	0.39	0.10	0.45	0.13

Note: Standard errors in parentheses.
* significant at 10%; ** significant at 5%; *** significant at 1%

According to Lazear (1989), large salary spreads may increase the incentives for non-cooperative behavior or sabotage. The reason for this is that workers can increase their productivity by exerting more effort and/or by making themselves look better relative to the other contestants. Since sabotage may be practically difficult in academia, given the nature of research, engaging in self-promotion may be one way that this phenomenon can be observed. The present dataset has measures of self-citations, which would serve as a good proxy for engaging in self-promotion with the intent of improving one's relative standing in the group. Model (13) tests the following hypothesis:

Hypothesis 5: Large salary spreads create incentives for self-promotion.

This hypothesis is tested by looking at spreads in self citations as related to spreads in salaries:

$$(13)\ \text{Spread}\,(S^{k}_{j,\,t+\Delta t}) = \beta_0 + \beta_1\ \text{Spread}\,(W^{k}_{j,\,t}) + \beta_2\ \text{Spread}\,(S^{k}_{j,\,t}) + \beta_3\ \text{Spread}\,(P^{k}_{j,\,t})$$

where:

S = self-citations
W = wage
P = productivity
j = department
t = 1992, 1995, 1999
k = Assistant vs. Associate, Associate vs. Professor, and Assistant vs. Professor.

Tables 1.23, 1.24, and 1.25 display the results. Five sets of regressions are run for each case: the first relates contemporaneous spreads in self citations to spreads in salaries; the second relates lagged salary spreads to self citation spreads (setting $\beta_2 = \beta_3 = 0$ in model (13)); the third regression conditions this relation on the past spread in self citations (setting $\beta_3 = 0$); the fourth and the fifth regressions further condition the relation on the publication spreads in addition to past spreads in self-citations (they use two

different measures of publications, namely volume and citations). Each model is estimated using OLS and the fixed effects method.

Model 13-1 in Tables 1.23-1.25 estimates the contemporaneous correlation between wage and self-citation spreads. The correlations are significant only in the case of Assistants vs. Associates and Assistant vs. Professors. The estimated elasticity ranges between 1.41-1.45. The fixed effects method does not yield significant results in this case. Model 13-2 in Tables 1.23-1.25 relates current self-citation spreads to past salary spreads. The estimated elasticities in this case are much larger, ranging from 2.33 for the case of Assistants vs. Professors (Table 1.25, OLS estimation), to 8 for the case of Assistants vs. Associates (Table 1.23, fixed effects estimation).

Since citing one's own work is often necessary, and certain researchers may need to do this more than others, depending on the field or nature of research, certain spreads in self citations may be built into the relationship that is being observed here. To control for this, it becomes necessary to condition on past spreads (model 13-3, Tables 1.23 – 1.25). The estimates reveal a similar relationship between salary spreads and self citations in the case of Assistants vs. Associates (Table 1.23) and Assistants vs. Professors (Table 1.25) as before. The estimated elasticities are similar and range from 2.37 for the case of Assistants vs. Professors (model 13-3, Table 1.25, OLS estimates) to 6.95 (model 13-3, Table 1.23, fixed effects estimates).

Since citations and self-citations are necessarily positively correlated with the amount of research published and/or cited, the fourth and fifth set of regressions condition the relationship between salary spreads and self-citation spreads on spreads of two measures of publications, namely volume and total citations from others (in addition

to lagged spreads in self-citations). The estimates appear in tables 1.23-1.25 under models 13-4 and 13-5. The relationship remains strong for the case of Assistants vs. Associates with elasticities ranging from 5.26 to 7.03. The fixed effects method yields significant results for this case as well. For the case of Assistants vs. Professors, the only significant elasticity estimate is obtained through the OLS estimation with the relationship being conditioned upon volume of publications (the elasticity is 3.24). It is interesting to note that the estimated elasticities increase consistently from left to right in Tables 1.23 and 1.25 with the addition of more controls. While it would be expected to obtain less significant and smaller estimates as past self-citation spreads and past research activity are factored in, the opposite is observed. This emphasizes the fact that there appears to be a strong association between self-citations and salaries.

TABLE 1.23 – DEPARTMENTAL- LEVEL REGRESSION RESULTS ON THE EFFECT OF SALARY SPREADS ON SELF PROMOTION (ASSISTANT PROFESSORS VS. ASSOCIATE PROFESSORS)

Dependent Variable: $Log\left(\frac{\overline{S}_{assoc}}{\overline{S}_{assist}}\right)_{i,t}$			Dependent Variable: $Log\left(\frac{\overline{S}_{assoc}}{\overline{S}_{assist}}\right)_{i,t+\Delta t}$							
	(13-1)		(13-2)		(13-3)		(13-4) P = Volume		(13-5) P = Cites	
	OLS	FE	OLS	FE	OLS	FE	OLS	FE	OLS	FE
$Log\left(\frac{\overline{W}_{assoc}}{\overline{W}_{assist}}\right)_{i,t}$	1.45 (0.71)*	0.32 (1.07)	4.67 (1.41)***	8.00 (2.16)***	4.97 (1.57)***	6.95 (2.04)**	5.26 (1.77)**	7.03 (2.37)**	5.33 (1.90)**	7.01 (2.27)**
$Log\left(\frac{\overline{S}_{assoc}}{\overline{S}_{assist}}\right)_{i,t}$	–	–	–	–	-0.18 (0.29)	-0.36 (0.23)	-0.12 (0.34)	-0.37 (0.26)	-0.09 (0.38)	-0.31 (0.33)
$Log\left(\frac{\overline{P}_{assoc}}{\overline{P}_{assist}}\right)_{i,t}$	–	–	–	–	–	–	-0.10 (0.25)	0.04 (0.29)	-0.11 (0.30)	-0.10 (0.37)
Constant	0.37 (0.18)**	0.53 (0.22)**	-0.28 (0.29)	-0.83 (0.38)*	-0.25 (0.35)	-0.50 (0.43)	-0.22 (0.37)	-0.56 (0.64)	-0.22 (0.37)	-0.42 (0.56)
Observations	24	24	16	16	15	15	15	15	15	15
R-squared	0.16	0.01	0.44	0.70	0.45	0.80	0.46	0.80	0.46	0.80

Note: Standard errors in parentheses; * significant at 10%; ** significant at 5%; *** significant at 1%

TABLE 1.24– DEPARTMENTAL- LEVEL REGRESSION RESULTS ON THE EFFECT OF SALARY SPREADS ON SELF PROMOTION (ASSOCIATE PROFESSORS VS. PROFESSORS)

Dependent Variable: $Log\left(\frac{\overline{S}_{prof}}{\overline{S}_{assoc}}\right)_{i,t}$	Dependent Variable: $Log\left(\frac{\overline{S}_{prof}}{\overline{S}_{assoc}}\right)_{i,t+\Delta t}$									
	(13-1)		(13-2)		(13-3)		(13-4) P = Volume		(13-5) P = Cites	
	OLS	FE	OLS	FE	OLS	FE	OLS	FE	OLS	FE
$Log\left(\frac{\overline{W}_{prof}}{\overline{W}_{assoc}}\right)_{i,t}$	-0.29 (0.92)	-0.20 (0.70)	-1.72 (1.12)	-0.56 (1.31)	-1.12 (0.97)	2.64 (1.64)	-1.45 (0.92)	2.84 (1.39)	-1.43 (1.41)	2.71 (1.82)
$Log\left(\frac{\overline{S}_{prof}}{\overline{S}_{assoc}}\right)_{i,t}$	–	–	–	–	0.50 (0.20)**	-0.84 (0.31)**	0.38 (0.19)*	-0.77 (0.27)**	0.44 (0.27)	-0.93 (0.46)
$Log\left(\frac{\overline{P}_{prof}}{\overline{P}_{assoc}}\right)_{i,t}$	–	–	–	–	–	–	0.34 (0.19)	0.59 (0.34)	0.06 (0.19)	0.14 (0.46)
Constant	0.83 (0.35)**	0.79 (0.27)***	1.38 (0.42)***	0.94 (0.49)*	0.81 (0.42)*	0.41 (0.47)	0.77 (0.39)*	-0.14 (0.51)	0.91 (0.55)	0.32 (0.58)
Observations	25	25	17	17	16	16	16	16	16	16
R-squared	0.00	0.01	0.14	0.03	0.42	0.60	0.54	0.77	0.42	0.61

Note: Standard errors in parentheses; * significant at 10%; ** significant at 5%; *** significant at 1%

TABLE 1.25– DEPARTMENTAL- LEVEL REGRESSION RESULTS ON THE EFFECT OF SALARY SPREADS ON SELF PROMOTION
(ASSISTANT PROFESSORS VS. PROFESSORS)

Dependent Variable: $Log\left(\frac{\overline{S}_{prof}}{\overline{S}_{assist}}\right)_{i,t}$			Dependent Variable: $Log\left(\frac{\overline{S}_{prof}}{\overline{S}_{assist}}\right)_{i,t+\Delta t}$							
	(13-1)		(13-2)		(13-3)		(13-4) P = Volume		(13-5) P = Cites	
	OLS	FE	OLS	FE	OLS	FE	OLS	FE	OLS	FE
$Log\left(\frac{\overline{W}_{prof}}{\overline{W}_{assist}}\right)_{i,t}$	1.41 (0.68)**	1.29 (0.83)	2.33 (1.21)*	3.88 (2.06)	2.37 (1.19)*	3.42 (2.56)	3.24 (1.23)**	3.59 (2.76)	2.39 (1.54)	4.14 (3.01)
$Log\left(\frac{\overline{S}_{prof}}{\overline{S}_{assist}}\right)_{i,t}$	–	–	–	–	0.43 (0.35)	-0.19 (0.55)	0.57 (0.34)	-0.08 (0.63)	0.43 (0.45)	0.02 (0.70)
$Log\left(\frac{\overline{P}_{prof}}{\overline{P}_{assist}}\right)_{i,t}$	–	–	–	–	–	–	-0.36 (0.21)	-0.19 (0.40)	-0.00 (0.31)	-0.23 (0.42)
Constant	0.56 (0.37)	0.63 (0.44)	0.03 (0.67)	-0.79 (1.10)	-0.52 (0.80)	-0.30 (1.80)	-0.50 (0.75)	-0.19 (1.94)	-0.52 (0.85)	-0.49 (1.95)
Observations	26	26	17	17	17	17	17	17	17	17
R-squared	0.15	0.13	0.20	0.34	0.28	0.35	0.41	0.38	0.28	0.39

Note: Standard errors in parentheses; * significant at 10%; ** significant at 5%; *** significant at 1%

VII. THE COMPENSATION STRUCTURE IN ACADEMIA

Throughout the study, the findings have suggested that the structure of compensation in academia depends largely on the grade level. At the Assistant Professor stage, the department needs to make sure that people of the right ability have been hired. At the same time, given the lag involved in observing productivity, the department needs to make sure that the newly hired faculty are indeed performing to a certain departmental standard. The up-or-out tenure decision is designed to solve both problems and provide the necessary incentives. The regression results indicate that the effect of rank order is most important for the promotion decision at the junior stage. When analyzing the effect of rank order on promotion probabilities (Table 1.8A), it appears that the partial effect of the rank variable is much larger in magnitude at the junior stage. The estimates in the third and the fifth columns of Table 1.8A show that one step up in ordinal rank within a department increases the probability of promotion from Assistant to Associate Professor by about 0.06 (|-1.76| (coefficient) x 0.036 (scaling factor)), whereas the probability of being promoted from Associate to Professor only increases by about 0.006 (|-0.17| (coefficient) x 0.036 (scaling factor)) for one step up in ordinal rank.

However, the publication measures do not seem to matter at the junior stage (they all appear statistically insignificant). At the senior stage, both the impact factor publication index and the volume of publications appear significant. Looking at the same partial-effect estimates in Table 1.8A, the marginal effect of the publication index suggests that a 10-page paper published in the *Journal of Economic Literature* increases the probability of being promoted to the next level by about 0.015. In addition, the ten published pages would be increasing the probability of promotion by another 0.0013

through the VOLUME variable. However, both of these variables appear statistically insignificant when analyzing the probability of promotion from Assistant to Associate Professor.

Comparing the above marginal effects of the rank-order and publications variables across the grade levels, it is clear that rank-order tournaments are much stronger at the early stage. Not only is the effect of rank order about 10 times larger at the Assistant level, but also the absolute publication measures do not seem to matter. All these findings point toward a weak link between absolute performance and salary at the Assistant Professor level.

It was argued earlier that beyond the Assistant Professor level, salary raises are more important in generating incentives than the promotion to the next grade level. Regressions similar to models (5) through (7) are run separately for the two groups (Assistants, and all others) to see whether salary raises affect one group more than the other as argued. Table 1.26 displays the results. A simple comparison between the estimates in the first and second columns of this table shows that salary raises do not generate incentives at the junior stage. All explanatory variables in the regression involving Assistants only appear statistically insignificant. At the senior stage, however, the coefficient of the rank-order variable suggests an effect of 0.7% (|-0.12| (coefficient) x 0.036 (scaling factor)) of one step up in ordinal rank on the salary increase during the period under consideration. This finding corroborates the argument that a different kind of tournament takes place, where prizes equal periodical wage raises. Such periodical salary increases, based partially on rank order, would provide the incentives not to shirk at the later stages of academic life-cycle.

TABLE 1.26 –COMPARISON OF SALARY CHANGES BETWEEN ASSISTANTS AND ALL OTHERS

Dependent Variable: Δ[Log (salary)]		
	Assistants	Associates and Professors
Δ (PUBLICATION INDEX) (based on the impact factor)	0.00390 (0.00332)	0.00060 (0.00023)**
Δ VOLUME OF RESEARCH	-0.00013 (0.00048)	-0.00003 (0.00002)
Δ (CITATIONS FROM OTHERS)	-0.00068 (0.00071)	-0.00053 (0.00028)*
FLOW RANK WITHIN DEPARTMENT (based on citations)	-0.43409 (0.36743)	-0.11683 (0.02770)***
FLOW DISTANCE BETWEEN RANKS	-0.00127 (0.00456)	0.00039 (0.00028)
Δ(AWARDS)	–	-0.00125 (0.05894)
Δ(ADMINISTRATIVE DUTIES)	–	0.09823 (0.02975)***
Δ(CHAIR)	–	0.10475 (0.03859)***
UNIVERSITY OF ILLINOIS	0.06366 (0.08875)	0.01646 (0.01861)
INDIANA UNIVERSITY-BLOOMINGTON	-0.00156 (0.15208)	-0.06786 (0.01566)***
UNIVERSITY OF IOWA	-0.01369 (0.15520)	-0.00791 (0.02037)
THE OHIO STATE UNIVERSITY	-0.01073 (0.08296)	-0.01644 (0.01691)
PURDUE UNIVERSITY	0.20449 (0.20243)	0.00358 (0.01917)
UNIVERSITY OF MICHIGAN	-0.05640 (0.15104)	0.04466 (0.02133)**
UNIVERSITY OF MINNESOTA	0.62070 (0.38359)	0.05215 (0.05164)
UNIVERSITY OF WISCONSIN-MADISON	0.01826 (0.07749)	0.02793 (0.02898)
YEAR 1999	-0.08897 (0.15817)	-0.00309 (0.01137)
CONSTANT	0.33043 (0.30403)	0.21333 (0.02597)***
OBSERVATIONS	26	356
R-squared	0.55	0.22

Note: Robust standard errors in parentheses;
* significant at 10%; ** significant at 5%; *** significant at 1%

VIII. ROBUSTNESS CHECK

The main implications of tournament theory were tested by conjecturing a linear relationship between salaries and the rank-order variable. Estimates in Tables 1.7A and 1.7B showed that, in salary regressions, the rank-order variable was significant and the total-citations variable was not. This observation was interpreted to indicate the relative importance of rank order compared to absolute performance. The concern arising from such a specification, however, was that the results might be driven by nonlinearities between the rank order and the total citations variable. Therefore, the models were estimated with functions of rank order instead and this section will present the findings. As a general conclusion, it does not appear that the particular form of the rank-order variable influences the previous findings.

Table 1.27 presents the findings from using powers of the rank-order variable. All regressions include the full set of controls used in the previous analysis (same as column (3) Table 1.6). The first regression simply reproduces the previous results for ease of comparison. The second regression introduces $(RANK)^2$ whereas the third one adds $(RANK)^3$. These modifications to the basic model do not produce any major changes. The absolute citation measure remains insignificant, the estimated partial effects of other control variables remain the same (not shown here), and the R-squared remains the same. In addition, the Ramsey RESET test for functional form specification does not reveal any misspecifications. The rank order variable becomes insignificant when its powers are added to the regressions, but this is mostly due to the multicollinearity introduced. Jointly, rank order and its powers are largely significant.

TABLE 1.27 –POWER FUNCTIONS OF RANK ORDER

Dependent Variable: LOG(SALARY)			
Model	(1)	(2)	(3)
RANK	-0.37510 (0.07438)***	-0.29451 (0.21769)	-0.14486 (0.56579)
$(RANK)^2$	–	-0.06942 (0.20001)	-0.39723 (1.26747)
$(RANK)^3$	–	–	0.20409 (0.83184)
CITATIONS FROM OTHERS	-0.00000 (0.00006)	0.00001 (0.00006)	0.00001 (0.00006)
DISTANCE BETWEEN RANKS (based on citations)	0.00003 (0.00004)	0.00003 (0.00004)	0.00003 (0.00004)
OTHER CONTROLS (INCLUDED IN MODEL 3)	Yes	Yes	Yes
CONSTANT	10.85832 (0.07057)***	10.84056 (0.08417)***	10.82083 (0.10021)***
Observations	615	615	615
R-squared	0.65	0.65	0.65
Ramsey RESET test	0.88	1.01	0.99
Prob>F	0.453	0.386	0.397
Test for joint significance of RANK and its powers (Prob>F)		0.000	0.000

Note: Robust standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 1.28 introduces three other functions of RANK, namely, the inverse, the exponential, and the logarithmic. Regressions are run with and without the linear version of RANK to check for the relative importance of RANK versus f(RANK). The only case when the absolute measure of citations appears significant is when the inverse of RANK is used (model (1), Table 1.28). While this finding may appear to cast doubt upon the functional choice, the Ramsey RESET test reveals functional form misspecification in this case (p-value = 0.000). In fact, as the linear form of RANK is added to the first regression, RANK appears significant whereas 1/RANK and total cites are not (Model (2), Table 1.28. In addition, the R-squared is lowest for the first regression.

The exponential and logarithmic functions also appear to work the same way as the linear function of RANK. They appear significant and negative as expected, whereas the absolute measure of total citations is insignificant (models (3) and (5), Table 1.28). When RANK is added to these regressions (models (4) and (6)), both functions of rank become insignificant. Because of this tendency and the relative simplicity of a linear function, all models in the chapter include RANK in a linear fashion.

TABLE 1.28 – INVERSE, EXPONENTIAL, AND LOGARITHMIC FUNCTIONS OF RANK ORDER

Dependent Variable: LOG(SALARY)						
Model	(1)	(2)	(3)	(4)	(5)	(6)
RANK	–	-0.38835 (0.07911)***	–	-0.23889 (0.41523)		-0.43562 (0.13846)***
1/RANK	0.00498 (0.00429)	-0.00268 (0.00436)	–	–	–	–
EXP(RANK)	–	–	-0.20182 (0.04284)***	-0.07473 (0.23895)	–	–
LOG(RANK)	–	–	–	–	-0.12246 (0.02670)***	0.02964 (0.04873)
CITATIONS FROM OTHERS	0.00024 (0.00007)***	0.00001 (0.00007)	0.00003 (0.00006)	0.00001 (0.00006)	0.00002 (0.00006)	0.00001 (0.00007)
DISTANCE BETWEEN RANKS (based on citations)	-0.00013 (0.00007)*	0.00005 (0.00004)	0.00003 (0.00004)	0.00003 (0.00004)	-0.00007 (0.00004)*	0.00004 (0.00004)
OTHER CONTROLS (INCLUDED IN MODEL 3)	Yes	Yes	Yes	Yes	Yes	Yes
CONSTANT	10.58651 (0.05767)***	10.87075 (0.07256)***	11.01539 (0.09681)***	10.91838 (0.20901)***	10.54363 (0.05665)***	10.91301 (0.11661)***
Observations	615	615	615	615	615	615
R-squared	0.62	0.65	0.65	0.65	0.64	0.65
Ramsey RESET test	6.66	0.96	1.29	1.01	2.23	1.05
Prob>F	0.000	0.411	0.277	0.389	0.083	0.371
Test for joint significance of RANK and *f*(RANK) (Prob>F)				0.000		

Note: Robust standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

IX. CONCLUSION

This chapter tests the implications of tournament theory, using data from a portion of the academic market for economists. The practice of internal promotions and the considerable pay differentials among grade levels qualify the academic labor market as an internal labor market, where compensation schemes such as tournaments are likely to emerge. The existence of detailed performance and salary data for the academic market make it a prime setting for testing the implications of tournament theory.

There seems to be ample evidence that rank-order tournaments explain part of the compensation structure in this market, and more so at the junior level. All regressions show a significant effect of rank order, even after controlling for measures of productivity. A summary of the estimated partial effects of productivity and rank order on salaries and promotion probabilities is presented in Table 1.29. These effects are stronger early in the career, when promotion appears to be a major motivator. According to this table, the effect of rank order on salaries ranges from 0.7% to 1%, holding constant productivity measures. The effect of rank on promotion probabilities varies widely with the type of promotion. At the junior stage, the effect of rank order on the probability of promotion is about ten times larger than its effect at the senior stage (the partial effect ranges from 0.05 to 0.06 for the promotion to Associate, and from 0.006 to 0.007 for the promotion to Professor).

Other explanatory variables (such as experience) and other measures of productivity display a practically and statistically significant effect, suggesting that the tournament theory falls short of explaining fully the structure of compensation in the profession. The next chapter will further investigate the effect of experience on salaries.

Implications of the tournament theory regarding the correlation of higher compensation spreads with larger production differentials are confirmed by the data. In particular, all measures of productivity show a significantly positive response to higher spreads in salaries in the previous period. This effect is observed even after conditioning on the initial spread in productivity, which may be due to inherent ability differences within individual departments.

There is some indication of handicapping of higher-ability types, for the purpose of improving the efficiency of mixed tournaments. In particular, it appears that having graduated from one of the top institutions (signaling higher ability at the time of first hire) negatively affects the chances of promotion from Assistant to Associate Professor, holding productivity and other variables constant.

Finally, the data show some evidence that the competitiveness induced by a tournament scheme may induce self promotion for the purpose of improving relative status.

At first sight, reliance on tournament-type schemes to allocate compensation and promotions would not seem reasonable in an academic setting, since research productivity can be measured. Besides, the lack of a highly hierarchical organizational structure and the fact that these contests may have multiple winners may undermine the incentives that would emerge under a pure tournament scheme. However, since performance can only be observed with a considerable lag, it is necessary to use an incentive system to motivate people in the short run. One such incentive system would be a delayed payment scheme, which employs cardinal scales of performance. The reason why a tournament structure is preferred to a delayed payment scheme lies in the

cost effectiveness of the former. Ordinal rankings are less costly to construct, relative to cardinal rankings. In addition, certain components of performance, such as advising of students, committee work, etc., cannot be easily quantified and measured. However, people can be ranked ordinally with respect to their involvement and performance along these dimensions.

The limited number of grade levels, the institution of tenure, and the up-or-out contract features in academia are bound to induce differences in the salary and incentive structures across grade levels. At the junior stage, promotions carry considerably more weight than salary raises, whereas at the senior stage, the very lack of additional promotion steps would give periodical salary raises more importance, and greater potential for generating incentives. One could think of salary increases and promotion titles as carrying different weights in the compensation package at different points in the academic life-cycle. The data indicate that promotions (based partly on rank order) are more powerful at generating performance incentives at the junior stage. Salary increases (also based partly on rank order) seem to be better motivators later in the academic life-cycle.

TABLE 1.29 – SUMMARY OF ESTIMATED PARTIAL EFFECTS OF PUBLICATIONS AND RANK ORDER

Marginal Effects						
		Impact Factor Publication Index ($PUBLINDX^{if}$)		Citation Ratio Publication Index ($PUBLINDX^{cr}$)		Rank order
		10-page JEL paper (impact factor = 6.41)	10-page AER paper (impact factor = 1.98)	10-page JEL paper (weight =1,530/13,400)	10-page AER paper (weight =8,999/13,400)	
Effect on Salary		1.67 – 1.92%	0.51 – 0.59%	0.20 – 0.21%	1.15 – 1.27%	0.07 – 1.04%
Effect on Salary Changes		3.85 – 4.49%	1.19 – 1.39%	0.63 – 0.82%	3.71 – 4.84%	0.25 – 0.30%
Effect on Promotion Probability	Assistant to Associate	0.07†	0.02†	0.03	0.16	0.05 – 0.06
	Associate to Professor	0.015	0.005	0.001	0.008	0.006 – 0.007

† statistically insignificant

Note: Numbers appearing in this table were derived from the estimates of models (1) – (10) presented in Tables 6A, 6B, 7A, 7B, 8A, and 8B using calculations similar to the ones presented in the text.

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ESSAYS ON COMPENSATION STRUCTURES AND HUMAN CAPITAL MIGRATION

VOLUME II

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Elda Pema

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CHAPTER TWO

RISING WAGE PROFILES – AN EVALUATION OF ALTERNATIVE THEORIES BASED ON EVIDENCE FROM THE ACADEMIC LABOR MARKET

I. INTRODUCTION

According to human-capital theory, upward-sloping wage-experience profiles reflect human capital accumulation (Ben-Porath, 1967; Mincer, 1974; Becker, 1975). Empirical work has documented rising wage profiles, and the human-capital theory explanation has often been confirmed in the literature. According to this theory, investment in general human capital results in increased productivity, and consequently in higher wages. In recent years, alternative explanations of increasing wage-experience profiles have emerged in response to a number of empirical and theoretical challenges to the human-capital theory.[48] These theories maintain that such profiles may be adopted to (1) reduce shirking (Lazear, 1979, 1983); (2) decrease adverse selection (Guasch and Weiss, 1980); or (3) respond to workers' preferences for increasing consumption profiles (Loewenstein and Sicherman, 1991; Frank and Hutchens, 1993). Lack of performance data has prevented extensive testing and comparison of these theories. However, such an analysis, becomes feasible in the context of academic labor markets, where information on productivity is publicly available and the setup favors the formation of implicit contracts. Interestingly, the fact that detailed information on the ability of employees is available for this market does not affect the general finding that experience remains one of the best predictors of salaries.

[48] See Hutchens (1989) for a literature review.

This chapter investigates the effect of experience on academic salaries, and claims that it is attributable to incentives built into the compensation system. The long lag involved in observing performance warrants the use of an incentive scheme to motivate faculty to perform in the short run. The study tests and compares the implications of various theories, and produces evidence that favors the hypothesis that employees prefer rising wage profiles. The study finds weak evidence in support of Lazear's theory of delayed-payment schemes, and evidence refuting the adverse-selection theory. Section II will summarize the findings on wage profiles in academia; Section III will introduce and discuss competing theories; Section IV will present the empirical analysis and discuss the findings; Section V will provide a few concluding remarks.

II. Review of Literature on Academic Salaries

As discussed in Chapter 1, most research on faculty salaries has focused on estimating the effect of publishing activity on salaries, under the implicit assumption of a piece-rate compensation structure. All studies that involve a salary regression include experience[49] and all of them report positive and significant returns to experience even after controlling for productivity. The inclination in the literature is to interpret this experience effect as representing all of the omitted or unobserved performance components, assuming implicitly that all of these components improve with experience. This study departs from this line of explanation and from the assumption of a piece-rate compensation scheme for faculty, and explains the premium to experience as evidence of an underlying incentive-payment scheme. Table 2.1 summarizes the partial effect of

experience on faculty salaries reported by various studies over time. The striking element emerging from Table 2.1 is that the majority of these studies involve performance measures ranging from satisfactory to exceptionally detailed. However, the effect of experience is always present, and comparable in magnitude across studies.

[49] Experience in all of these studies is defined as the number of years since receipt of Ph.D.

TABLE 2.1 – SUMMARY OF ESTIMATED EXPERIENCE PARTIAL EFFECTS FROM VARIOUS STUDIES

Study	Effect of Experience and (experience^2)	Sample	Other variables included
Siegfried and White (1973)	$253 per additional year	45 faculty members during 1971-1972 academic year	Publications, teaching, administrative duties.
Tuckman and Leahey (1975)	1% for Professors 5% for Associates	American Council of Education data on 973 male economists during 1972-1973	Dummy variables for number of articles and books published, for race, Ph.D., department quality, administrative duties, and age.
Tuckman and Hagemann (1976)	$411 (for economists) (exper^2 = -7)	Male faculty in economics and education during 1972-1973.	Articles, books, teaching awards, administrative duties (past and present), public service, quality of department, region, and race.
Tuckman, Gapinski and Hagemann (1977)	$528 for social science faculty (exper^2 = -8)	American Council of Education (ACE) data for 1972-1973; five disciplines; about 12,000 full time faculty	Dummy variables for number of articles and books published, teaching awards, public service, administrative duties, Ph.D., department quality, region, and race.
Hansen, Weisbrod, and Strauss (1978)	3%[50] (exper^2 = 0.0005)	National Register of Scientific and Technical Personnel; 863 economists during 1966.	Number of articles and books published, ranking of employing and graduating departments, gender, and age.
Weiss and Lillard (1978)	4.7% - 6.3% depending on discipline (exper^2 = 0.0004 -0.0008)	American scientists during 1960-1970 (about 11,000).	Type of employer, gender, quality of Ph.D.-granting department, pre-degree experience.
Hamermesh, Johnson, and Weisbrod (1982)	1.1% (exper^2 = 0.00001)	Seven large public universities, 1979-1980; full professors only	Citations, published articles (for two departments only), administrative duties.

[50] This effect is obtained by their estimates of a "typical" wage equation. The study, however, estimates a system of equations, which does not generate a direct relationship between salary and experience.

TABLE 2.1 (CONTINUED)

Diamond Jr. (1986)	4% ($exper^2$ = 0.0006)	Longitudinal data over the 1965-1977 period; mathematics faculty at the University of California.	Various measures of citations and cumulative number of published articles.
Sauer (1988)	1.8% ($exper^2$ = 0.00003)	140 academic economists (associates or full professors) employed in top 40 departments during 1982-1983.	Quality-weighted article pages in top 100 journals, books, and papers, data on co-authorship, citations over 1976-1982, and years of administrative service.
Ransom (1993)	3.9% - 4.5% overall ($exper^2$ = 0.00006 - 0.00007); 2%-6% by discipline ($exper^2$ = 0.0007 - 0.001)	ACE data (1972-73), about 30,000 observations	Degree held, seniority, race, sex, region, SMSA type, city size, contract length.

It should be noted that when discussing human capital in the academic market, reference is being made primarily to general human capital. There is very little, if any, institution-specific knowledge that a faculty member may possess. Research and teaching skills of faculty are easily transferable from one department to another. An indication of the relevance of this claim is the often-cited negative returns to seniority for faculty (Ransom, 1993; Brown and Woodbury, 1998).[51] According to human-capital theory, rising wage-seniority profiles would represent returns to firm-specific investment by workers, which may be shared by the employer (Becker, 1975; Oi, 1962; Hashimoto, 1981). While the negative seniority effect may be due to selection (more mobile people are better able than others, or better able at bidding up their salaries through frequent moves), its existence provides some support for the claim that human capital in the academic labor market is mostly general rather than department-specific.

The discussion in Section III, Chapter 1 suggests that the long lag involved in observing research output in academia warrants the use of an incentive scheme. Based on available research on the publishing process, it is claimed that there is a lag of at least ten years involved in observing and evaluating research productivity. During the period when research effort has not yet materialized in the form of published work or has not yet been evaluated by the academic community by being cited, there are considerable incentives to shirk. A payment scheme that incorporates incentives to reduce shirking

[51] Moore, Newman, and Turnbull (1998) conclude that these two studies find negative seniority effects because they fail to include performance variables. With the dataset used for this study, I replicate the results from Moore et al., and find that the negative seniority effect indeed is not very robust when including performance data and only using a cross section. However, the seniority effect persists after correcting for heteroskedasticity and autocorrelation among observations from the same individual in a panel dataset. Also, the conclusions of Moore et al. are based on a regression where both seniority and experience are included and appear insignificant (Table 2, p. 360). Their insignificance in this particular regression may be due to the high collinearity between experience and seniority (in my sample, the correlation amounts to 0.75).

during the period that productivity cannot be assessed would be necessary to mitigate the agency problem. While a tournament compensation scheme would help alleviate this problem, the empirical analysis of the first chapter showed that it does not entirely explain all the features of the compensation system for Big Ten faculty. In particular, in all the regressions presented in the first chapter, the effect of experience was quite robust. To explain this feature of the compensation system, the present chapter will test theories on incentive-based compensation systems that generate implications on the wage-experience relationship. At the same time the analysis may shed light on this relationship in other markets where information is less readily available. The academic market is a prime setting for exploring these issues since (1) human-capital accumulation is quite intensive in this market, (2) performance data are publicly available, (3) most contracts are long-term, and (4) there are agency problems that can be remedied by incentive-based compensation schemes.

III. Review of Literature on Incentive-Based Compensation Schemes

A. Lazear's Model of Delayed Payment Contracts

Lazear's work on mandatory retirement and pensions (1979, 1981, 1983) suggests that upward-sloping wage-experience profiles could arise because of incentives incorporated into the compensation scheme. According to his original model, junior workers are offered a wage below the value of marginal product (VMP), in return for wages above VMP later in their working life. This compensation scheme creates incentives for workers not to shirk because, if caught and fired, they forego the rents that

they are promised to receive later in the life cycle. Reliable workers remain with the firm and collect wages higher than the spot VMP at the end of their career. The existence of mandatory retirement would facilitate the implementation of such a compensation scheme, since workers may have incentives to renege on the promise to retire when they are earning more than their reservation wage.[52] Reputation effects would provide incentives to firms to pay the promised rents to their older workers, at a time when they would have incentives to replace older workers with younger employees who would earn less than their reservation wage. It should be noted that such contracts would give rise to upward-sloping wage-experience profiles, even in the absence of productivity increases.

The Age Discrimination in Employment Act (ADEA) enacted in 1968 introduced a series of age-discrimination laws intended to protect workers between the ages of 40 and 65. A 1978 amendment to the ADEA raised the upper limit for the protected group to age 70. Due to concerns about the effect of the abolition of mandatory retirement in institutions of higher education, this change did not take effect until July 1, 1982. Representatives of higher education institutions were primarily concerned about (a) a possible shortage of positions for junior faculty; (b) the fact that some faculty members would choose to teach longer than they would be competent to; (c) the effect of the legislation on affirmative action programs, since the reduction in junior positions would also reduce the number of openings for women and minorities; (d) a potential weakening of the protection of tenure, since uncapping could lead to more frequent performance evaluations and dismissals of tenured faculty, and (e) a negative effect on departmental budgets if most hiring is done at the junior level (since not as many highly paid senior

[52] Mandatory retirement would not be necessary for such a compensation scheme to emerge because

faculty can be replaced by less well-paid junior faculty). The 1986 amendment to the ADEA abolished mandatory retirement for most workers. Some states had already passed legislation prohibiting mandatory retirement before this date. A special provision in the 1986 amendment allowed institutions of higher education to continue to enforce mandatory retirement.[53] In 1991, a Congress-mandated study by the National Research Council (Hammond and Morgan) concluded that the fears of school administrators with regard to the abolition of mandatory retirement for tenured faculty were unfounded. As a result, the Congress allowed the exemption for university professors to expire by January 1, 1994.

For the most part, studies published prior to the elimination of mandatory retirement for faculty concluded that the change in legislation would have little impact on academic departments. Rees and Smith (1991) attempted to forecast the effect of mandatory retirement on faculty retirement rates by analyzing institutions where mandatory retirement had been abolished by state law. They surveyed 33 institutions (12 capped and 21 uncapped by state laws prohibiting mandatory retirement) over the 1988-1989 academic year. They found that the mean retirement age was higher in private versus public institutions, in research versus teaching institutions, and in institutions with better students. They did not, however, find systematic differences in the mean retirement age in capped versus uncapped institutions, suggesting that the abolition of mandatory retirement would have no effect on the mean age at retirement.[54] They also

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appropriately structured pensions (as, for example, pensions whose value varies with age of retirement) could serve the same purpose (Lazear, 1982).

[53] This exemption was also extended to fire fighters and police officers.

[54] This conclusion only extends to selective liberal arts colleges and leading public universities, because their sample does not include any uncapped private universities.

found no evidence in the literature suggesting that those who chose to stay beyond age 70 were incompetent or non-performing teachers and researchers.

Hammond and Morgan (1991) reached similar conclusions. Because they found that most faculty in their sample retired before the age of 70, and because only 1.6% of faculty in all uncapped institutions in their sample were over the age of 70, they concluded that the elimination of mandatory retirement would not have a large impact on the proportion of faculty who decided to work past the age of 70. In private research institutions, however, they believed that a larger proportion of faculty would choose to stay longer, since a large portion of the faculty in these institutions was observed to retire at the mandatory retirement age of 70. They also concluded that an increase in the number of faculty over the age of 70 was not likely to affect institutional quality. Finally, they asserted that the institution of tenure was unlikely to be undermined by the change in mandatory retirement laws, since dismissal of tenured faculty is a lengthy and costly process.

Recent studies, however, have suggested that the impact of the elimination of mandatory retirement in academic labor markets may not be negligible. A recent survey conducted by the American Association of University Professors' (AAUP) Committee on Retirement (*Survey of Changes in Faculty Retirement Policies*, 2000) observes that a greater proportion of faculty are staying beyond the age of 70 in doctoral versus other institutions, and in private versus public institutions. This survey collected data from 608 colleges and universities to obtain information on faculty retirement practices and the prevalence of early-retirement incentive programs.

A recent study by Ashenfelter and Card (2002) indicates that the abolition of mandatory retirement substantially reduced the retirement rates of 70- and 71-year-old professors. They employ data from 104 colleges and universities (*Faculty Retirement Survey*) and find that immediately after the prohibition of mandatory retirement, retirement rates for 70- and 71-year-olds fell to under 30% from the respective 75% and 60% rates prevalent during the mandatory retirement era.

The theoretical implications of ADEA in Lazear's model would be ambiguous. In the absence of mandatory retirement, wage-experience profiles are bound to become flatter, or mirror performance paths more closely. One reason for this is because retirement at a certain age can no longer be enforced through mandatory retirement. Another reason why the change in legislation may result in flatter wage-experience profiles is that, even if retirement incentives can be provided, the expected life-span of the Lazear contract becomes longer and rents for older workers are distributed over a longer period of time. The age discrimination legislation may even encourage Lazear contracts if workers fear the possibility of the firm reneging on the long-term contract and firing older workers who are owed payments in excess of their VMP (Neumark and Stock, 1999).

The institution of tenure complicates the analysis of the impact of age discrimination legislation on the academic labor market. Tenure paired with the absence of compulsory retirement, amounts to almost a guarantee of life-time employment. Note, however, that the existence of tenure does not rule out Lazear contracts. While providing employment insurance, tenure does not guarantee specific salary levels, and administrators can induce effort by altering salaries or their rates of change, rather than

firing.[55] Altering perquisites, departmental support, and work-loads can also serve this purpose. With respect to Lazear contracts, by reducing the possibilities for termination, tenure reduces the incentive-generating power of a back-loaded compensation structure. At the same time tenure precludes any concerns from faculty about the possibility of the university reneging on the long-term contract. These two effects combined, would suggest flatter wage-experience profiles in academia as compared to other professions, and at the same time a flattening of wage-experience paths after the abolition of mandatory retirement for tenured faculty.

Another potential effect of the abolition of mandatory retirement in academia could be a perceived weakening of the protective effect of tenure, since it increases incentives from administrators to engage in more frequent and rigorous post-tenure performance evaluations, and, in extreme cases, more dismissals. Rees and Smith (1991) call for a formal post-tenure review process similar to the one employed by the University of California system to remedy the life-time employment issue. Also, one of the recommendations of the National Research Council (NRC) report is to develop ways to provide tenured faculty with feedback on their performance, although the report expresses doubts about the cost-effectiveness of an elaborate review process. Both studies mention the possibility that the end of mandatory retirement may increase the number of dismissal cases, although the NRC report argues that this is unlikely considering the length and cost of the process. The reason why the abolition of mandatory retirement may increase the number of dismissal cases is because, while the administration may have been unwilling to engage is such a case when a non-performing

[55] While salaries rarely go down in nominal terms, in real terms they often do so.

faculty member was close to mandatory retirement age, in the absence of this previously costless separation, it will have incentives to face this problem earlier.

While dismissal of a tenured faculty member is an extreme and rare measure, anecdotal evidence suggests that departments often avoid it by engaging in actions leading to the voluntary resignation or retirement of incompetent faculty members. According to Hammond and Morgan, such actions often involve suggesting possible dismissal, completing the procedures leading to dismissal without carrying out the actual process, and negotiating arrangements for departure (such as buy-outs) without mention of dismissal. Resignations in lieu of dismissals benefit both the administration and the individual. The administration is relieved of the burden of assembling evidence and hurting employee morale, whereas the individual is saved the stigma of dismissal. This may be one reason why the number of observed formal dismissals is quite low.

Another potential effect of age-discrimination legislation concerns the choice of retirement plans offered. In the academic labor market, there are three prevalent pension plans: defined-contribution, defined-benefit, and a combination of the two. Defined-contribution plans specify a certain percentage that the individual contributes to his/her retirement fund, which is often matched by a specified percentage by the institution. The individual bears the risk of investing these funds. Most often the funds are managed by private insurers, the largest of them being the Teachers Insurance and Annuity Association – College Retirement Equities Fund (TIAA-CREF). Defined-contribution plans create incentives to delay retirement because of three reasons: (a) during the years that retirement is delayed more contributions are made, (b) prior accumulation earns

income during these years, and (c) life expectancy of the annuitant is shorter at retirement, so that the annuity from any given accumulation is larger.

Defined-benefit plans fix the amount of the pension benefit, rather than the amount of contribution made. Investment risk in this case is borne by the institution, which needs to guarantee a fixed payment upon retirement regardless of market performance. In general, defined-benefit plans do not encourage late retirement, since the annuity is determined in a way that is unrelated to current wages. In fact, appropriately structured defined-benefit plans (plans whose present value declines with retirement age), can be used to induce retirement at a certain age and facilitate Lazear contracts. In higher education, however, defined-benefit plans are different from those in industry. They also create incentives to delay retirement, since they often follow a formula of the form: x% * highest (or last) years' average salary * years of service (where x is a fixed factor determined by the administration, often about 2%).[56] This formula clearly shows that staying employed longer increases retirement benefits, because it (a) increases the number of years of service, and (b) assuming steady salary increases, the average salary portion of the formula is bound to increase. In recent years some institutions have been offering a hybrid of defined contribution and defined benefit plans. According to the *Survey of Changes in Faculty Retirement Policies* the breakdown of retirement plans offered by their sample of public doctoral institutions is as follows: 19

[56] There is sometimes an upper limit to what this formula generates. Most often this limit is set at 75-90% of final average salary. The highest limit is 100%, and would most likely not be binding until 37 years of service under the same pension plan. Note that under a defined-contribution plan there is no such limit, and some people wind up receiving more than 100% of final salary.

offered defined-contribution plans, 14 defined-benefit, and 13 a combination plan, with 51 institutions offering a choice between the systems.[57]

One expected effect of the prohibition of mandatory retirement would be the proliferation of early retirement incentive programs. According to the *Survey of Changes in Faculty Retirement Policies,* about 25 (60%) private doctoral and 97 (42%) public doctoral institutions have had one or more financial incentive programs since 1995 that encouraged tenured faculty members to retire prior to age 70. According to the same source, 25 (72%) private doctoral and 95 (38%) public doctoral institutions reported having negotiated buyouts (cash payments for early retirement). Such buy-outs often amount to a lump-sum payment close to a 9-18 month salary for early retirement. In recent years some institutions of higher education have been offering phase-outs, which allow a faculty member nearing retirement age to phase into retirement gradually by working part-time, but retaining full health and other benefits. The *Survey of Changes in Faculty Retirement Policies* reports that as of 1999, 26 (50%) of private doctoral and 97 (31%) of public doctoral institutions responding to the survey, currently use phased retirement programs.

Table 2.2 below summarizes retirement plans for Big Ten institutions and important changes in age-discrimination legislation for the respective states. It appears that most departments offer defined-contribution pension plans. It is also interesting to note that all universities make use of some form of early retirement program. Most interesting, it appears that some of these universities have stopped enforcing mandatory

[57] This compares with private doctoral institutions, among which 24 offered defined-contribution plans, and only one offered a combination plan.

retirement for faculty before 1994, due to special provisions in state legislation. This fact will be used to obtain identification in the empirical analysis.

Since plausible arguments can be made for either steeper or flatter wage-experience profiles after the abolition of mandatory retirement, the answer to this question remains to be answered empirically. Section IV.II will examine how changes in mandatory retirement laws have affected the steepness of wage profiles for economics faculty in Big Ten universities.

TABLE 2.2 – SUMMARY OF RETIREMENT PLANS AND MANDATORY RETIREMENT STATE LAWS FOR BIG TEN UNIVERSITIES

School	Pension plan	Incentives for early retirement	State law and special provisions for tenured faculty
University of Illinois	Defined benefit (until 1998; as of 1999 combination)	Early retirement option (for people at the age of 55; in 2002 early retirement rules were changed to include a penalty)	1967: enacted law prohibiting age discrimination; 1980 expanded coverage to cover people between the ages of 40-70. Mandatory retirement for tenured faculty enforced until the expiration of ADEA amendment, January 1, 1994.
University of Iowa	Defined Contribution	Early retirement (ages 57-64; effective during 1997-2002) Phase-out (effective in 1992)	1972: prohibited age discrimination for those over 18; 1983: banned mandatory retirement. Mandatory retirement for tenured faculty enforced until the expiration of ADEA amendment, January 1, 1994.
Indiana University	Combination	Supplemental Early Retirement Plan	1965: enacted laws prohibiting age discrimination for those 40-65; 1979: raised upper limit to 70. Mandatory retirement for tenured faculty enforced until the expiration of ADEA amendment, January 1, 1994.
Purdue University	Combination	Voluntary Early Partial Retirement Program	
Ohio State University	Combination	Early retirement plan (for faculty ages 64 to 70)	1961: enacted age-discrimination laws for people ages 40-65; 1990: extended coverage to those over 40. Tenured faculty exempted from above law. Mandatory retirement enforced until the expiration of ADEA amendment, January 1, 1994.

TABLE 2.2 (CONT'D)

University of Michigan	Defined Contribution	Phase-out	1965: passed age-discrimination laws protecting those between 35-60, and banned mandatory retirement for people under the age of 65; 1972: expanded coverage to those over 18. Effective May 1, 1991, enacted law prohibiting mandatory retirement for tenured faculty.
Michigan State University	Defined Contribution	Faculty employed prior to 1992 may serve their final year before retirement on a terminal consultantship	
University of Minnesota	Defined contribution	Early retirement incentives in effect since 1992	1977: prohibited age discrimination for those over 18; 1978: banned mandatory retirement for those under 70. Mandatory retirement for tenured faculty enforced until the expiration of ADEA amendment, January 1, 1994.
University of Wisconsin- Madison	Defined Benefit	Early retirement (after the age of 55)	1959: enacted age-discrimination laws for people ages 40-65. 1982: expanded coverage for those 40-70; banned mandatory retirement before the age of 70; 1984: removed age limit for age discrimination. Mandatory retirement abolished by state law for everyone as of 1989.

B. ADVERSE SELECTION

Guasch and Weiss (1980, 1982) claim that rising wage profiles may be due to information asymmetries. They assume that employees have better information about their expected productivity than do their employers. To separate low-productivity workers from high-productivity ones, firms institute testing or an apprenticeship program which sorts workers according to their abilities. During the apprenticeship or initial period when the firm is assessing the ability of the workers, it pays them wages below their marginal product. Once the abilities of the workers are revealed, high-ability types are paid a wage above their marginal product, and low-ability types are paid a wage just equal to their marginal product.[58] Therefore, for each individual worker, we could observe a wage profile that increases over his/her life cycle. When a sorting equilibrium exists, it is characterized by net payments below marginal product for workers who fail the test, and net payments above marginal product for those who pass it. While these studies do not aim to explain upward-sloping wage-experience profiles *per se*, they imply that such profiles may result from the firm's desire to sort workers according to their ability.

In the case of academia, it would be reasonable to argue that the institution of tenure resembles an assessment test much like the one described in Guasch and Weiss. If the sorting theory were adopted to explain rising wage profiles, then the implication for faculty salaries would involve wages of junior faculty being below their marginal product, and, after tenure, salaries would increase up to and above the marginal product. Because of the up-or-out feature of academic contracts, junior faculty who fail to pass the

test (obtain tenure) would have to leave the department. This implies that all faculty who remain with the department are paid a salary above marginal product beyond the junior stage. Section IV. III presents a test of these implications.

C. Worker Preferences for Rising Consumption Profiles

Loewenstein and Sicherman (1991) and Frank and Hutchens (1993) claim that people may prefer rising wage profiles because (a) they care not only about their consumption level, but also about its rate of change, and (b) they cannot defer present consumption to achieve upward-sloping consumption profiles.[59] The first study brings experimental evidence supporting such a claim. The second study brings examples from airline pilots and bus drivers to point out the wage-productivity divergence. They claim that, for these specific workers, productivity remains fairly constant over the life-cycle, and, therefore, their rising wage-experience profiles should be explained in terms other than productivity increases. The model that Frank and Hutchens present postulates that wage profiles should be steeper in occupations with the most extensive interaction among co-workers. This would follow from the assumption that people prefer increasing consumption profiles. However, according to this theory, people also tend to define their well-being around a standard based on the well-being of their social group. As a result, they find it harder to save in the company of people who spend more they do, and cannot, therefore, save to achieve increasing consumption profiles. Consequently, people form collective agreements with groups of like-minded people to resist the temptation to

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[58] Since in the model all workers pay a fee during the assessment period (either to take the test or in the form of lower wages during the apprenticeship), the low-ability types end up with wages net of assessment fees that fall below their marginal product.

consume too much in the present. Frank and Hutchens claim that a person's co-workers are more likely to make up the reference group because they engage in more interactions on a more continuous basis with the person. This is also the reason why it would be easier to enter collective consumption agreements with one's co-workers, since close proximity and frequent interaction make monitoring and enforcing of such agreements less costly. Co-workers can agree to restrict their ability to consume in the present by entering employment contracts that involve steep wage profiles. Such wage paths would only be appealing if the employer can credibly guarantee a long employment span. Therefore, the length of the contract emerges as the key feature of this theory. A steep wage profile would be desirable only if the life-span of the contract is considerably long. At the same time, a testable implication of this theory is that increased interaction with co-workers would be associated with stronger preferences for steep wage profiles. If the preferences for such profiles are widespread, competitive market forces will provide the necessary incentives to employers to offer back-loaded wage contracts.

In academia, cooperation among researchers is the norm nowadays, rather than the exception. Improvements in information technology have made geographical barriers less of an issue in recent years, so cooperation with colleagues across the world is quite feasible. The degree of academic cooperation (co-authorship of articles) would, therefore, depend in large part on the preferences of various researchers to interact closely with their colleagues. By analyzing cooperation patterns of faculty, it could be possible to test this hypothesis empirically. In fact, it could be argued that, if faculty care about their financial standing as compared to their peers, increased preferences for

[59] This assumption follows from the self-control literature. The idea is that people may find it hard to save when they are surrounded by people who enjoy higher spending levels than they do.

cooperating academically within the department would signal preferences for entering collective consumption agreements that restrict the ability to consume earlier in the work life-cycle. Since departments have a large degree of flexibility in setting wages, they would most likely recognize and incorporate such considerations in the employment contracts they offer. Consequently, it could be the case that increased interaction with co-workers, as measured by the degree of within-department co-authoring, would be associated with steeper wage-experience profiles. Given the nature of academia, where people constantly need to interact with their colleagues to discuss and improve their work, and frequently cooperate on common projects, it could also be argued that academics have a higher preference for steep wage profiles compared to people in other occupations. These hypotheses will be tested in Section IV.V.

IV. Empirical Analysis

This section employs the same dataset as the first chapter. It consists of observations from full-time economics faculty in nine large Midwestern universities[60] for the 1991-92, 1995-96, and 1998-99 academic years. Tables 1.3, 1.4, and 1.5 in Chapter 1, provide a description of the variables and summary statistics. Table 2.3 contains summary statistics of the key variables used and additional variables that appear here for the first time.

[60] Indiana University -Bloomington, University of Illinois at Urbana-Champaign, University of Iowa, Michigan State University, The Ohio State University, Purdue University, University of Michigan, University of Minnesota, and University of Wisconsin-Madison.

TABLE 2.3 – SUMMARY STATISTICS

Variable	Variable Description	Year	N	Mean	Std. Dev.
SALARY	Annual salary	1992	202	$85,350	$26,184
		1995	218	$87,412	$26,891
		1999	211	$93,395	$28,546
PUBLINDX[if] (based on impact factor)	Publication Index with W_j = impact factor	1992	243	126.72	124.08
		1995	243	151.67	141.49
		1999	243	184.64	160.27
EXPER	Experience measured as years since receiving the Ph.D. degree.	1992	226	16.36	9.81
		1995	237	18.51	10.33
		1999	240	22.27	10.50
OTHCITES	Number of citations from others	1992	243	216.21	318.57
		1995	243	248.67	345.08
		1999	243	269.05	362.42
HRDAFTER	=1 if person hired after the abolition of mandatory retirement	1992	49	0.21	0.41
		1995	49	0.21	0.41
		1999	49	0.21	0.41
COOPERATION	Average number of co-authors per period from the same department	1992	243	0.109	0.198
		1995	243	0.114	0.293
		1999	243	0.112	0.243
PCCOOP	Percentage of co-authors from same department (out of all co-authors)	1992	221	0.164	0.254
		1995	192	0.141	0.297
		1999	202	0.127	0.240

Note that for the purpose of this study, the stock of performance rather than the flow measure is the relevant variable for the empirical analysis. Academic departments do not only care about the recent publications of a faculty member. Rather, the reputation of the institution benefits from the accumulated record of lifetime publications of the researcher and the reputation that he/she has achieved in the academic community during the entire academic life. Therefore, all the productivity data used here (and discussed in Chapter 1, Section V) are cumulative in nature.

When discussing the different theories on wage-experience profiles, it should be noted that some of them refer to the cross-sectional properties of wage-experience paths, while others refer to their time-series properties. The human-capital theory describes the way in which wages and experience are related over an individual's life-cycle, and hence deals with the time-series behavior of the wage-experience path. The delayed-payment theory is also concerned with the behavior of wages over the life cycle. However, this theory implicitly generates implications on the cross-sectional properties of wage-experience profiles. Because the premise of this theory is to explain mandatory retirement policies, the implication is that a policy change (as for example, the abolition of mandatory retirement) may result in changes in wage-experience paths across all cohorts, regardless of where they are positioned in their life cycle. Theories arguing that workers prefer increasing wage profiles primarily characterize the life-cycle pattern of wages. Implicitly, however, these theories suggest that wage-experience paths may differ across various professions depending on the preferences that different groups of workers

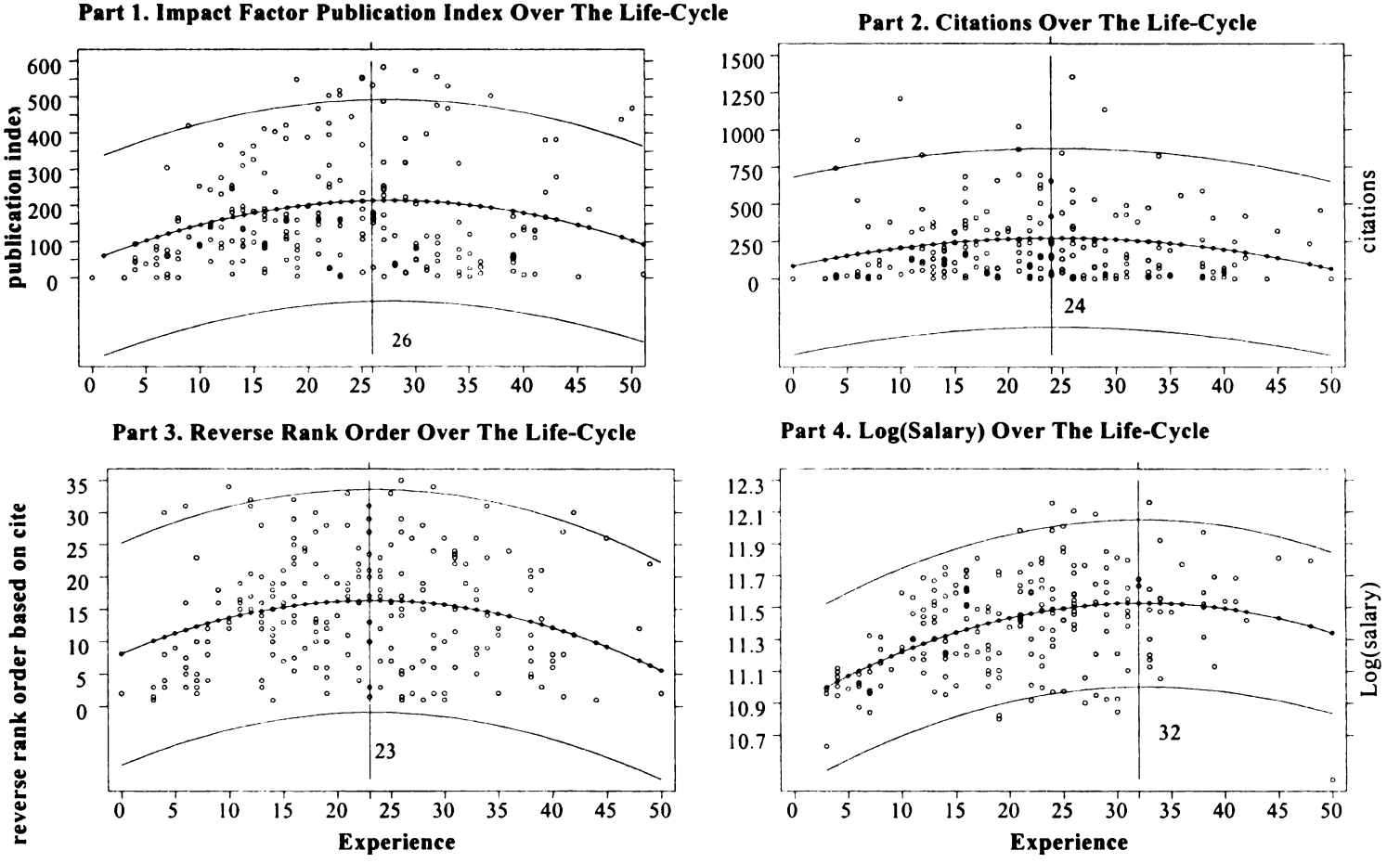
display.[61] These arguments will be taken into account when deriving the empirical tests later in this section.

In all the results from the empirical analysis in Chapter 1, the effect of experience was always present, regardless of the number or combination of performance measures included in the salary equation. Figure A summarizes the relationship of the performance indicators and salaries to experience. The graphs display the fitted regression lines of the measures of interest on a number of controls which include experience and its square. It appears that productivity turns around about 8 years earlier than salaries. The estimates suggest that between 23 and 31 years of experience, we would be observing increasing rates of compensation and declining rates of performance. Contrary to Medoff and Abraham (1980), who did not find evidence of performance increasing with experience, here performance and salaries increase over most of the range, but at different rates. Their slopes have the same sign for the most part, with the exception of the period between 23 and 31 years of experience. This does not change the fact that the effect of experience on salaries remains significant after controlling for performance, leaving unanswered the question: why do salaries increase with experience? It should be noted that the regression estimating the performance paths below do not describe the path of any particular researcher. In fact, with cumulative productivity measures, no individual researcher has a downward sloping portion in their performance profile. It is cohort effects that give rise

[61] This assumes that people in the same profession are similar to a large extent and their preferences with respect to features of employment contracts are alike. In fact the theory implies that cross-section wages should differ across *individuals*, given their *individual* preferences with respect to the slope of the wage path, and given that employers are willing and able to design individualized contracts.

to the shape of the performance curves below. Younger cohorts seem to publish more at all ages, and as a result the productivity curves appear to slope downwards.

FIGURE A – PERFORMANCE AND COMPENSATION OVER THE LIFE CYCLE



Note: The middle line represents the regression line of the variable on the vertical axis on experience, its square, and year dummies. In the case of log(salary), administrative duties, department dummies, and performance variables are also included as explanatory variables. The upper and lower bounds are calculated as two times the standard error of the residuals. The vertical line in each graph shows the turnaround point. Since productivity measures are cumulative in nature, for simplicity of representation only observations for the year 1999 are presented (the regression line, however, is obtained from a pooled regression including all years).

A. TESTING FOR THE EFFECT OF EXPERIENCE NET OF PRODUCTIVITY ON SALARIES

If experience represents human capital accumulation, one would expect that this fact would be revealed through the performance indicators. Thus, experience should carry no weight in a wage equation that includes performance data. For academic jobs, however, experience appears to be one of the most important variables in the salary determination, even after controlling for productivity. The most commonly used salary model in the case of academia involves an expression of wages as a linear function of productivity, experience, teaching performance, and administrative duties. Other variables included depend on the availability of data and scope of study. The quality of graduating institution is sometimes included to search for an effect of the quality of education received. Given the data set available for this study, the standard salary model takes the following form:

$$(1)\quad \text{Log}(SALARY_{it}) = \alpha_1 + \beta_{11}PUBLINDX_{it} + \beta_{12}VOLUME_{it} + \beta_{13}OTHCITES_{it} + \beta_{14}RANK_{it} + \beta_{15}DISTANCE_{it} + \beta_{16}EXPER_{it} + \beta_{17}EXPER_{it}^2 + \beta_{18}AWARD_{it} + \beta_{19}ADMIN + \beta_{1,10}CHAIR_{it} + \beta_{1,11}TOP10_i + \beta_{1,12}FEMALE_i + \delta_{1k}FIELD_i + \theta_l DEPARTMENT_i + \lambda_{11}Y95_t + \lambda_{12}Y99_t + u_{it}$$

where:
i = individual
t = 92, 95, 99
k = 1,..., 5 (FIELD = THEORY, ECONOMETRICS, MACRO/FINANCE, PUBLIC/URBAN, LABOR)
l = 1,..., 8 (corresponding to each department in alphabetical order)

This model includes all available explanatory variables. The purpose of this estimation is to test whether there is a return to experience after controlling for performance and other

important factors, and, if so, to assess the magnitude of this premium. Results are presented in Table 2.4, columns (1) – (6).

The first estimation omits all performance variables. This regression attempts to address the question: what would the experience premium be if productivity data were not available? The estimates suggest that the premium to the first year of experience in the absence of performance variables is 4.50%. This effect gradually declines due to the negative quadratic term, and is estimated to turn around at 30 years of experience. Perhaps a better figure characterizing the effect of experience on salaries is the experience premium at different experience levels, such as 5, 10, and 20. According to these estimates, a faculty member with 6 years of experience earns about 3.67% more than someone with 5 years of experience. This gap narrows at higher levels of experience, suggesting differences of 2.91% and 1.39% at 10 vs. 11 years of experience and 20 vs. 21 years, respectively. These comparisons, however, are not very effective because they employ estimates of the experience premium obtained in the absence of performance measures. Estimations (2) through (5) gradually introduce performance measures.

The second regression in Table 2.4 includes three performance measures, the impact factor publication index, volume of research and total number of citations from others. The partial effect of experience falls dramatically, suggesting that a major part of the experience premium estimated in (1) was due to human-capital accumulation. Adding these important research productivity variables, however, does not seem to eliminate the experience effect entirely. The estimates suggest an experience premium of about 2.97% now, versus 4.50% as estimated in (1). At 5, 10, and 20 years of experience,

one additional year is expected to be associated with a salary premium of 2.47%, 2.02% and 1.11% respectively. These remain significant differences in pay, especially after considering that the second regression controls for all research activity.

Next, a variable capturing teaching quality (AWARDS) is added, and the results are presented in column 3 of Table 2.4. The estimates suggest that a teaching award is associated with a salary premium of 7.79%. The experience effect goes down slightly after the inclusion of AWARDS, from 2.97% to 2.94%, and the fit also improves a little (R-squared increases from 0.58 to 0.59). The changes are more pronounced when controls for administrative duties are included. The fourth column presents estimates obtained after adding two variables, one controlling for the effect of being department chair and the other controlling for the effect of holding other administrative duties, such as director of graduate or undergraduate programs, dean, director of a research institute, etc. The experience premium goes down from 2.94% to 2.67% and the fit improves from 0.59 to 0.61. Holding an administrative position other than chair, is associated with a pay difference of about 14%, whereas being a department chair is estimated to induce a 19.6% difference in salary, holding research and teaching performance constant.

The analysis in the first chapter suggested that the ordinal rank within the department was a significant factor in explaining faculty salaries. Therefore, the fifth regression includes ordinal rank and distance between ranks as explanatory variables. This change further reduces the partial effect of experience from 2.67 to 2.47, but the effect remains strong and statistically significant. The fit improves substantially, as evidenced by R-squared which increases from 0.61 to 0.65. One last attempt at obtaining smaller returns to experience involves excluding Assistant Professors from the analysis.

Previous studies have commonly excluded assistants from the analysis under the assumption that the pay structure is quite different at this stage. The effect of experience is reduced from 2.47 to 2.35 in this case (Table 2.4, (6)), and remains statistically significant. The exclusion of Assistant Professors from the analysis, however, is not reasonable if one of the underlying assumptions is that faculty pay follows a work-life incentive scheme.

Given the estimates in Table 2.4, a comparison of the effect of one additional year of experience and publications is of interest. According to the estimates in column (5), an additional year of experience seems to be associated with a 2.47% increase in earnings, holding performance and other variables constant. A 10-page paper in the *Journal of Economic Literature*, (highest-ranked journal according to the impact factor ranking in Table 1.2 of Chapter 1, right column; impact factor = 6.41), is predicted to increase salary by approximately 2.24% (0.00035 x 10 pages x 6.41 x 100%) according to the estimates in Table 2.4 below (the coefficient of the publication index is 0.00035). In order to draw meaningful comparisons of the marginal effects of experience and the publication index, some additional calculations are required. In this sample, the average impact index per year of experience is 10 (standard deviation 13). Therefore, an additional average–performance year on the job is expected to result in an increase in salary of 3.5% (0.00035 x 10 x 100%) through the publication measure, and an additional 2.47% through the experience variable alone. It appears that the effect of experience net of performance is comparable to the marginal effect of productivity. This finding is quite striking, especially after considering that the productivity measures used here include lifetime publications in a 5000-journal list and are adjusted for quality. It should be

noted, though, that the effect of experience is declining over the life-cycle as evidenced by the negative coefficient of $EXPERIENCE^2$. The magnitude of this coefficient suggests that the experience premium at 5, 10, 20, years of experience is 2.1%, 1.76%, and 1.08% respectively. While declining, the effect of experience is only predicted to reduce to zero at 61 years of experience, which is well beyond the range of the sample used here.

By comparing the experience effect in model (5) (2.47%) to the one estimated in model (1) (4.5%), it can be concluded that part of experience does in fact represent human-capital accumulation which leads to higher productivity measures. That would be the reason why the estimated marginal effect of experience is reduced substantially when performance measures are included. The fact remains, though, that the drop in the experience premium when including all available performance variables is about 45% as compared to the case when no performance data are included, and remains strongly significant. The significant positive premium that experience carries net of performance indicators suggests the existence of a Lazear-type contract. The next subsection investigates this claim further.

TABLE 2.4– THE EFFECT OF EXPERIENCE ON SALARIES

	Dependent Variable: LOG(SALARY)					
Model	(1)	(2)	(3)	(4)	(5)	(6)
PUBLICATION INDEX (based on the impact factor)	–	0.00056 (0.00012)***	0.00054 (0.00013)***	0.00052 (0.00013)***	0.00035 (0.00011)***	0.00036 (0.00012)***
VOLUME OF RESEARCH	–	0.00004 (0.00003)	0.00004 (0.00003)	0.00003 (0.00002)	0.00001 (0.00002)	0.00000 (0.00002)
CITATIONS FROM OTHERS	–	0.00020 (0.00005)***	0.00019 (0.00005)***	0.00020 (0.00006)***	-0.00000 (0.00006)	-0.00001 (0.00007)
RANK WITHIN DEPARTMENT (based on citations)	–	–	–	–	-0.37510 (0.07438)***	-0.39198 (0.09100)***
DISTANCE BETWEEN RANKS (based on citations)	–	–	–	–	0.00003 (0.00004)	0.00003 (0.00004)
EXPERIENCE	0.04503 (0.00652)***	0.02967 (0.00560)***	0.02942 (0.00553)***	0.02669 (0.00560)***	0.02471 (0.00509)***	0.02346 (0.00733)***
$(\text{EXPERIENCE})^2$	-0.00076 (0.00018)***	-0.00045 (0.00015)***	-0.00045 (0.00015)***	-0.00040 (0.00015)***	-0.00034 (0.00013)**	-0.00033 (0.00018)*
AWARDS	–	–	0.07789 (0.04211)*	0.07564 (0.04106)*	0.08936 (0.03485)**	0.08637 (0.03520)**
ADMINISTRATIVE DUTIES	–	–	–	0.14138 (0.04815)***	0.13689 (0.04918)***	0.15177 (0.05054)***
CHAIR	–	–	–	0.19586 (0.03775)***	0.15293 (0.03537)***	0.16235 (0.03610)***
TOP10	0.01493 (0.03187)	0.00229 (0.02663)	0.00744 (0.02661)	0.01342 (0.02585)	0.00599 (0.02439)	0.01021 (0.02599)

TABLE 2.4 (CONTINUED)

MODEL	(1)	(2)	(3)	(4)	(5)	(6)
FEMALE	-0.08861	-0.04640	-0.04215	-0.04321	-0.02532	-0.02395
	(0.04974)*	(0.04144)	(0.04071)	(0.03924)	(0.03665)	(0.04082)
THEORY	0.00672	0.01135	0.01753	0.03808	0.04831	0.03015
	(0.04664)	(0.04146)	(0.04239)	(0.03923)	(0.03388)	(0.03784)
ECONOMETRICS	0.15122	0.07181	0.06413	0.08437	0.06604	0.05816
	(0.06653)**	(0.04964)	(0.04710)	(0.04698)*	(0.04056)	(0.03952)
MACRO/ FINANCE	0.00163	-0.00823	-0.00922	0.00640	0.00736	0.00269
	(0.04430)	(0.03209)	(0.03257)	(0.03083)	(0.02834)	(0.03096)
PUBLIC/ URBAN	-0.00558	0.00731	0.01136	0.01275	0.01768	0.00902
	(0.04728)	(0.04440)	(0.04445)	(0.04265)	(0.04013)	(0.04295)
LABOR	0.04150	0.01647	0.02107	0.00457	-0.01813	-0.04659
	(0.04050)	(0.03371)	(0.03319)	(0.03411)	(0.03671)	(0.04088)
UNIVERSITY OF ILLINOIS –	0.06837	0.10629	0.09703	0.06926	0.03627	0.03502
	(0.05457)	(0.04571)**	(0.04642)**	(0.04523)	(0.04028)	(0.04328)
INDIANA UNIVERSITY –	-0.01562	-0.03925	-0.04633	-0.06502	-0.04557	-0.04099
	(0.06237)	(0.04651)	(0.04514)	(0.05019)	(0.04102)	(0.04718)
UNIVERSITY OF IOWA	0.02954	0.09888	0.09588	0.07716	0.04584	0.05520
	(0.06750)	(0.05316)*	(0.05179)*	(0.04847)	(0.04102)	(0.04277)
THE OHIO STATE UNIVERSITY	0.10305	0.09423	0.09838	0.09189	0.09608	0.09480
	(0.06195)*	(0.05499)*	(0.05530)*	(0.05368)*	(0.05199)*	(0.05695)*
PURDUE UNIVERSITY	0.02772	0.10080	0.10386	0.09073	0.04552	0.04751
	(0.07298)	(0.06618)	(0.06541)	(0.05760)	(0.05575)	(0.06218)

TABLE 2.4 (CONTINUED)

Model	(1)	(2)	(3)	(4)	(5)	(6)
UNIVERSITY OF MICHIGAN	0.16821	0.08642	0.09050	0.05262	0.09799	0.08803
	(0.04751)***	(0.04106)**	(0.03981)**	(0.04231)	(0.04249)**	(0.04734)*
UNIVERSITY OF MINNESOTA	0.20208	0.15037	0.13879	0.13270	0.16144	0.20778
	(0.08165)**	(0.07079)**	(0.06827)**	(0.06784)*	(0.06097)***	(0.06791)***
UNIVERSITY OF WISCONSIN -	0.21750	0.10714	0.10764	0.09099	0.16273	0.17098
	(0.05630)***	(0.04234)**	(0.04308)**	(0.04519)**	(0.04468)***	(0.04986)***
YEAR 1995	0.10143	0.09098	0.09165	0.09237	0.10939	0.10673
	(0.01434)***	(0.01290)***	(0.01298)***	(0.01308)***	(0.01345)***	(0.01294)***
YEAR 1999	0.20497	0.17629	0.17738	0.17958	0.20849	0.20150
	(0.01584)***	(0.01465)***	(0.01473)***	(0.01482)***	(0.01603)***	(0.01686)***
CONSTANT	10.54552	10.57380	10.57143	10.59055	10.85832	10.89328
	(0.06794)***	(0.05937)***	(0.05778)***	(0.05800)***	(0.07057)***	(0.07840)***
Observations	615	615	615	615	615	546
R-squared	0.44	0.58	0.59	0.61	0.65	0.59

Note: Robust standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

B. TESTING LAZEAR'S THEORY ON DELAYED-PAYMENT SCHEMES

The change in legislation could be manifested in a number of different ways. One way would be through changes in the experience premium for existing contracts. The theory on back-loaded payment systems is based on the existence of implicit long-term contracts that the employer and employee agree upon at the beginning of the work life. If both parties entering such a contract did so explicitly and took legal steps to ensure its enforceability, a change in legislation would have no impact on existing contracts. The only difference would be observed in employment rates and wage paths between the people already in the work force before the change in legislation and the ones that join the workforce after the change. However, especially in the current setting, if such long-term contracts exist, they are entered implicitly, and the potential for reneging from either side is possible after the change in legislation. For the case of academia, the institution of tenure eliminates the chief possibility for the department to terminate existing contracts. However, because tenure only affects the duration of employment, the wage remains largely open to changes (although subject to substantial downward rigidity). As a result, in the current setting, for contracts established before the change in legislation, one potential impact would be observed in the wage paths. Of course, a large impact would be observed on the length of employment, as evidenced by Ashenfelter and Card (2000), who find that a substantial percentage of 70- and 71-year-old faculty stay employed. The data employed here, however, are not well-suited to an analysis of employment/retirement rates, mostly due to the small sample. Since academic salaries are determined periodically rather than once at the beginning of the contract, the administrators have the flexibility to institute wage changes. Therefore, it is possible that

for people hired before the end of mandatory retirement, the wage path has changed due to the new legislation. Because the change happens in the middle of the contract term, the department has fewer alternatives available to induce retirement. In the absence of retirement systems that provide early-retirement incentives, one of the possible choices for the department is to reduce the quasi-rents that older faculty are earning toward the end of the career, or rather spread the rents that they would be earning up to the mandatory retirement age over a longer time period (up to the expected retirement date). If this were the case, the data should show flatter wage profiles for faculty hired before the change in legislation. To test this hypothesis, a new variable is introduced (LAW) that takes a value of one if mandatory retirement was abolished in a particular department by state or federal law. Also, another variable (HRDAFTER) is generated that takes a value of one if the person joined a particular department after mandatory retirement was abolished in that institution. Model (2) below presents the specification used to test this proposition:

$$(2)\quad \text{Log}(SALARY_{it}) = \alpha_1 + \beta_{11}PUBLINDX_{it} + \beta_{12}VOLUME_{it} + \beta_{13}OTHCITES_{it} + \beta_{14}EXPER_{it} + \beta_{15}EXPER_{it}^2 + \beta_{16}LAW_t + \beta_{17}(EXPERit * LAW) + \beta_{18}AWARD_{it} + \beta_{19}ADMIN + \beta_{1,10}CHAIR_{it} + \beta_{1,11}TOP10_i + \beta_{1,12}FEMALE_i + \delta_{1k}FIELD_i + \theta_1 DEPARTMENT_i + \lambda_{11}Y95_t + \lambda_{12}Y99_t + u_{it}$$

Table 2.5 presents the results from estimating the model above with a few variations. The first regression only employs observations from people who were hired before the change in legislation. The coefficient of interest, β_{17}, is estimated to be negative and statistically significant at the 10% confidence level. It suggests that the experience premium has dropped by 0.02 percentage points after the abolition of mandatory

retirement for faculty employed before the change in legislation. The other variables have coefficients similar to previous estimations.

One concern with the above estimation is that the declining experience effect may be due to cohort effects. The group entering contracts before the abolition of mandatory retirement tends to be older than the group entering contracts after that. Over the 1992-1999 period the former group is aging and a large portion of the faculty may be on the downward sloping portion of the experience profile. The average experience for faculty hired before the end of mandatory retirement is 19, 21, and 25 for each year respectively. From estimations in the previous section (Table 2.4), the experience effect turns around at about 33 years. Therefore, the average faculty member hired before the change in legislation is younger than the average faculty member for whom the experience effect starts declining due to age effects alone. However, the portion of faculty in this group that has more than 33 years of experience increases over the observed period (15%, 17%, and 23% for each year respectively). Although increasing, these percentages could arguably still be too low to drive the main results.

To deal with the possibility of other time- and individual-specific factors influencing these results, the above model is estimated using a fixed effects method. The disadvantage of this method is that in general it does not allow estimating the effect of experience. The year dummies would be perfectly correlated with experience and increasing by Δt in every period. However, the change in the experience premium before and after the change in legislation can be estimated. The second regression in Table 2.5 presents the results of such estimation. The interaction term EXPERIENCE * LAW appears negative but statistically insignificant. Therefore, while there is some indication

that wage profiles may have flattened for faculty entering contracts before the ban on mandatory retirement, there is evidence that these results may be due to cohort effects.

When testing for the presence of Lazear-type contracts, job tenure would be the choice variable over overall labor-market experience. In the present context, however, there is a slight complication arising from the fact that, in academia, seniority[62] often has a negative sign in wage regressions that include total experience. The prevailing explanation for this phenomenon is that after controlling for total experience, longer seniority is associated with less mobility, and mobility in academia is associated with higher wages. However, this issue is overcome by decomposing total experience into prior market experience and seniority with current department. The third regression in Table 2.5 presents the estimation of such a model. Note that this model includes all observations (not just faculty hired before the elimination of mandatory retirement) and no interactions between the experience components and the dummy indicating a change in legislation. This regression is simply intended to provide preliminary findings on the effect of seniority on wages. From the estimates, it appears that one additional year of prior job experience is associated with a salary of 1.5 percentage points higher. Part of this premium could be due to the mobility factor. Another hypothesis relates to the possibility that part of this premium (and also part of the seniority premium) are due to preferences for increasing consumption profiles. This hypothesis will be tested later in the chapter. Regardless of the source, the premium of this portion of experience cannot be affected by the change in legislation. Legislation is most likely to impact the seniority premium. The estimates on the third column of Table 2.5 suggest that one additional

[62] The rest of the discussion will refer to job tenure as "seniority" so as to avoid confusion between the concepts of job tenure and academic tenure.

year with the same department is associated with a 0.93% increase in salary. Model (3) below tests whether this premium has decreased as a result of the change in legislation for the portion of faculty entering contracts before this change:

$$(3)\quad \text{Log}(SALARY_{it}) = \alpha_1 + \beta_{11}PUBLINDX_{it} + \beta_{12}VOLUME_{it} + \beta_{13}OTHCITES_{it} + \beta_{14}PJMEXPER_{it} + \beta_{15}PJMEXPER_{it}^2 + \beta_{16}LAW_t + \beta_{17}SENIORITY_{it} + \beta_{18}SENIORITY_{it}^2 \; \beta_{19}(SENIORITY_{it} * LAW) + \beta_{1,10}AWARD_{it} + \beta_{1,11}ADMIN + \beta_{1,12}CHAIR_{it} + \beta_{1,13}TOP10_i + \beta_{1,14}FEMALE_i + \delta_{1k}FIELD_i + \theta_1 DEPARTMENT_i + \lambda_{11}Y95_t + \lambda_{12}Y99_t + u_{it}$$

The estimates appear on the last column of Table 2.5. The interaction SENIORITY * LAW appears negative, but statistically insignificant, suggesting no change in the seniority premium due to legislation for faculty entering contracts before this event.

TABLE 2.5– TESTING FOR A CHANGE IN WAGE PROFILES FOR FACULTY HIRED BEFORE THE CHANGE IN LEGISLATION

Dependent Variable: LOG(SALARY)	OLS	FE	OLS	OLS
Model	(1)	(2)	(3)	(4)
PUBLICATION INDEX (based on the impact factor)	0.00053 (0.00014)***	0.00056 (0.00017)***	0.00058 (0.00012)***	0.00055 (0.00013)***
VOLUME OF RESEARCH	0.00001 (0.00002)	0.00002 (0.00004)	0.00001 (0.00002)	0.00001 (0.00002)
CITATIONS FROM OTHERS	0.00021 (0.00006)***	0.00011 (0.00012)	0.00021 (0.00006)***	0.00022 (0.00006)***
EXPERIENCE	0.02441 (0.00638)***	–	–	–
$(\text{EXPERIENCE})^2$	-0.00023 (0.00012)*	-0.00055 (0.00011)***	–	–
LAW	0.08163 (0.05666)	0.00150 (0.03512)	–	0.04045 (0.04280)
EXPERIENCE * LAW	-0.00466 (0.00281)*	-0.00090 (0.00171)	–	–
PRIOR JOB MARKET EXPERIENCE	–	–	0.01494 (0.00467)***	0.01406 (0.00503)***
$(\text{PRIOR JOB MARKET EXPERIENCE})^2$	–	–	-0.00002 (0.00020)	-0.00003 (0.00022)
SENIORITY	–	–	0.00925 (0.00415)**	0.00858 (0.00555)
$(\text{SENIORITY})^2$	–	–	-0.00001 (0.00012)	0.00009 (0.00013)
SENIORITY * LAW	–	–	–	-0.00383 (0.00297)

TABLE 2.5 (CONT'D)

MODEL	(1)	(2)	(3)	(4)
ASSISTANT PROFESSOR	–	–	–	–
ASSOCIATE PROFESSOR	–	–	–	–
AWARDS	0.07559 (0.04424)*	-0.00855 (0.08820)	0.07722 (0.03729)**	0.08644 (0.04173)**
ADMINISTRATIVE DUTIES	0.14969 (0.04793)***	0.06651 (0.03456)*	0.15238 (0.04817)***	0.15533 (0.04942)***
CHAIR	0.22437 (0.03724)***	0.10685 (0.03609)***	0.23843 (0.04188)***	0.24899 (0.04027)***
TOP10	0.04252 (0.02606)	–	0.03113 (0.02427)	0.04964 (0.02645)*
FEMALE	-0.05748 (0.04346)	–	-0.03494 (0.03729)	-0.05681 (0.04204)
THEORY	0.04624 (0.04832)	–	0.04073 (0.03969)	0.03671 (0.04746)
ECONOMETRICS	0.09104 (0.05406)*	–	0.09702 (0.04663)**	0.08938 (0.05269)*
MACRO/ FINANCE	0.01278 (0.03264)	–	0.03230 (0.03027)	0.02097 (0.03237)
PUBLIC/ URBAN	-0.00308 (0.04316)	–	0.02191 (0.04301)	0.00874 (0.04518)
LABOR	-0.01404 (0.04249)	–	0.00579 (0.03660)	-0.00854 (0.04380)

TABLE 2.5 (CONT'D)

Model	(1)	(2)	(3)	(4)
UNIVERSITY OF ILLINOIS – URBANA CHAMPAIGN	0.07540 (0.04866)	–	0.06637 (0.04187)	0.07768 (0.04677)*
INDIANA UNIVERSITY – BLOOMINGTON	-0.05994 (0.05490)	–	-0.05853 (0.04920)	-0.05658 (0.05369)
UNIVERSITY OF IOWA	0.07902 (0.05306)	–	0.07989 (0.04606)*	0.08805 (0.05040)*
THE OHIO STATE UNIVERSITY	0.08873 (0.05987)	–	0.09340 (0.05395)*	0.09648 (0.05955)
PURDUE UNIVERSITY	0.12676 (0.06805)*	–	0.12292 (0.05680)**	0.14056 (0.06590)**
UNIVERSITY OF MICHIGAN	0.03203 (0.04859)	–	0.04205 (0.04128)	0.03444 (0.04954)
UNIVERSITY OF MINNESOTA	0.20413 (0.06311)***	–	0.12168 (0.06640)*	0.17334 (0.06399)***
UNIVERSITY OF WISCONSIN - MADISON	0.08162 (0.05480)	–	0.07478 (0.04290)*	0.06542 (0.05375)
YEAR 1995	0.09921 (0.01369)***	0.21221 (0.02035)***	0.09594 (0.01348)***	0.11014 (0.01525)***
YEAR 1999	0.17418 (0.02074)***	0.43021 (0.03486)***	0.19337 (0.01566)***	0.19289 (0.02286)***
CONSTANT	10.54811 (0.09416)***	11.22912 (0.05861)***	10.68156 (0.04502)***	10.67541 (0.07121)***
Observations	521	528	601	521
R-squared	0.63	0.74	0.63	0.62

Note: Robust standard errors in parentheses; * significant at 10%; ** significant at 5%; *** significant at 1%

Now the analysis turns to the comparison of wage paths between faculty entering contracts before and after the elimination of mandatory retirement. At first, the following specification is offered:

$$(4)\quad \text{Log}(SALARY_{it}) = \alpha_1 + \beta_{11}PUBLINDX_{it} + \beta_{12}VOLUME_{it} + \beta_{13}OTHCITES_{it} + \beta_{14}EXPER_{it} + \beta_{15}EXPER_{it}^2 + \beta_{16}HRDAFTER_{it} + \beta_{17}(EXPER_{it} * HRDAFTER_{it}) + \beta_{1,8}AWARD_{it} + \beta_{1,9}ADMIN + \beta_{1,10}CHAIR_{it} + \beta_{1,11}TOP10_i + \beta_{1,12}FEMALE_i + \delta_{1k}FIELD_i + \theta_1 DEPARTMENT_i + \lambda_{11}Y95_t + \lambda_{12}Y99_t + u_{it}$$

The coefficient of interest is β_{17} on the interaction term between experience and the dummy indicating that the faculty member was hired after the change in legislation. Table 2.6 presents the regression estimates. The first regression includes all observations. The coefficient on the interaction term EXPER * HRDAFTER appears positive and significant, suggesting an increase in the premium of experience by one percentage point. Therefore, faculty entering contracts after the elimination of mandatory retirement appear to earn 1% more for each additional year compared to the rest of faculty.

The same conclusion is reached after decomposing experience into prior job market experience and seniority. There is a concern about the fact that the findings from the previous estimation may be picking up the effect of mobility on wages. Faculty with more total experience are also more likely to have changed departments more frequently. Prior research indicates that mobility is positively related to wages. Therefore, the next specification decomposes experience into two separate components, and estimates the

change in the effect of remaining with the same department one additional year that is due to the elimination of the mandatory retirement clause in faculty contracts:

$$(5)\quad \text{Log}(SALARY_{it}) = \alpha_1 + \beta_{11}PUBLINDX_{it} + \beta_{12}VOLUME_{it} + \beta_{13}OTHCITES_{it} + \beta_{14}PJMEXPER_{it} + \beta_{15}PJMEXPER_{it}^2 + \beta_{16}HRDAFTER_t + \beta_{17}SENIORITY_{it} + \beta_{18}SENIORITY_{it}^2 \ \beta_{19}(SENIORITY_{it} * HRDAFTER_{it}) + \beta_{1,10}AWARD_{it} + \beta_{1,11}ADMIN + \beta_{1,12}CHAIR_{it} + \beta_{1,13}TOP10_i + \beta_{1,14}FEMALE_i + \delta_{1k}FIELD_i + \theta_l DEPARTMENT_i + \lambda_{11}Y95_t + \lambda_{12}Y99_t + u_{it}$$

The second regression in Table 2.6 presents the estimates. Here the measure of interest is β_{19} which is estimated to be positive and significant. The results appear stronger than before. The estimates suggest that another year with the same department is associated with a 0.67% increase in salary. However, faculty entering contracts after the abolition of mandatory retirement seem to be earning a salary increase of about 2.6 percentage points higher for the same year. The effect is significant at the 2% confidence level.

Again, the concern with the above estimations is that they may be confounded by cohort effects. The cohort entering contracts later is likely to be younger (average experience is 16 years, versus 22 years for the group hired before the change in legislation). Therefore, the next two regressions estimate models (4) and (5) above excluding first time hires (Assistant Professors). The results presented in columns (3) and (4) of Table 2.6 appear quite similar to the ones presented earlier. Faculty joining after the change in legislation are estimated to earn 1.2% more for each year of experience than the rest, according to the estimate on the interaction term EXPER*HRDAFTER on the third column. The same story holds for the seniority premium. According to estimates, faculty hired after the change in law earn about 2.5

percentage points more for each year of stay with the current department. Seniority otherwise appears insignificant, whereas prior job-market experience has a positive and significant coefficient, suggesting earnings of 1-1.5% more for each year of prior experience. Introducing dummies for Assistant and Associate instead of dropping observations from Assistant Professors yields similar results (not shown here).

Removing observations from Assistant Professors may mitigate the cohort effects embedded in the data to a certain extent, but it does not resolve the problem entirely. Changes in the professional environment over the period and the composition of the newer cohorts of faculty may be driving the observed effect of experience. These factors may be correlated with the variables of interest. It could be the case that younger faculty are more familiar with recent research which would contribute to their research performance. They may also be more narrowly specialized, and this would affect the types of journals in which they publish, and ultimately the publication index used here. Also, due to the proliferation in professional journals and the speed of information dissemination in recent years, it could be the case that older faculty have had fewer available venues in which to publish and promote their earlier research, and fewer opportunities to be cited at the same age as the younger cohorts. At the same time, older cohorts may have had a longer time span over which to publish and accumulate citations to published research. All these unobservable factors would be correlated with the performance variables as well as the variable of interest, EXPERIENCE. Therefore, models (4) and (5) are estimated using a fixed effects method. The regression results are tabulated in columns (5) and (6) of Table 2.6. A number of dummy variables, as well as experience and prior job-market experience, drop out of the estimation because they do

not vary over time. Seniority is weakly identified due to differences induced by mobility. The coefficient on the interaction term EXPER*HRDAFTER is amazingly similar to the one estimated using OLS. Its magnitude suggests earnings of 1.1 percentage points higher for faculty hired after the elimination of mandatory retirement (Table 2.6, column (5)). The seniority premium for this group also appears positive and significant, suggesting earnings of 1.5 percentage points higher. This estimate is substantially smaller than the ones obtained using OLS (Tables 2.5-2.6) suggesting that cohort effects may have been partially responsible for the large effect estimated previously. The main conclusion, however, remains that the Lazear contracts appear stronger after the elimination of mandatory retirement.

TABLE 2.6– TESTING FOR DIFFERENCES IN WAGE PROFILES BEFORE AND AFTER THE CHANGE IN LEGISLATION

Dependent Variable: LOG(SALARY)	ALL (OLS)		EXCLUDES ASSISTANTS (OLS)		ALL (FIXED EFFECTS)	
Model	(1)	(2)	(3)	(4)	(5)	(6)
PUBLICATION INDEX (based on the impact factor)	0.00048 (0.00012)***	0.00056 (0.00012)***	0.00047 (0.00012)***	0.00050 (0.00012)***	0.00049 (0.00016)***	0.00059 (0.00016)***
VOLUME OF RESEARCH	0.00002 (0.00002)	0.00001 (0.00002)	0.00002 (0.00002)	0.00002 (0.00002)	0.00003 (0.00004)	0.00002 (0.00004)
CITATIONS FROM OTHERS	0.00022 (0.00006)***	0.00022 (0.00006)***	0.00021 (0.00005)***	0.00021 (0.00005)***	0.00007 (0.00012)	0.00006 (0.00013)
EXPERIENCE	0.02112 (0.00556)***	–	0.01237 (0.00567)**	–	–	–
(EXPERIENCE)^2	-0.00024 (0.00012)*	–	-0.00008 (0.00012)	–	-0.00062 (0.00010)***	–
HRDAFTER	-0.04732 (0.06992)	-0.08595 (0.06140)	-0.09020 (0.09110)	-0.09026 (0.08361)	–	–
EXPERIENCE * HRDAFTER	0.01018 (0.00526)*	–	0.01174 (0.00618)*	–	0.01055 (0.00600)*	–
PRIOR JOB MARKET EXPERIENCE	–	0.01485 (0.00466)***	–	0.00919 (0.00507)*	–	–
(PRIOR JOB MARKET EXPERIENCE)^2	–	-0.00001 (0.00020)	–	0.00012 (0.00022)	–	-0.03273 (0.04963)
SENIORITY	–	0.00668 (0.00513)	–	-0.00087 (0.00500)	–	-0.24512 (0.28018)
(SENIORITY)^2	–	0.00006 (0.00013)	–	0.00021 (0.00013)	–	-0.00036 (0.00012)***
SENIORITY * HRDAFTER	–	0.02605 (0.01068)**	–	0.02507 (0.01443)*	–	0.01520 (0.00639)**

TABLE 2.6 (CONT'D)

	ALL (OLS)		EXCLUDES ASSISTANTS (OLS)		ALL (FIXED EFFECTS)	
MODEL	(1)	(2)	(3)	(4)	(5)	(6)
AWARDS	0.05332	0.07353	0.04781	0.06632	0.02788	0.02775
	(0.04077)	(0.03757)*	(0.04006)	(0.03682)*	(0.07623)	(0.07952)
ADMINISTRATIVE DUTIES	0.14172	0.15507	0.15746	0.16824	0.06585	0.06893
	(0.04710)***	(0.04828)***	(0.04794)***	(0.04884)***	(0.03735)*	(0.03903)*
CHAIR	0.21217	0.24120	0.22299	0.23736	0.10714	0.13035
	(0.03691)***	(0.04176)***	(0.03763)***	(0.04109)***	(0.03887)***	(0.04032)***
TOP10	0.02663	0.03233	0.03052	0.03378	–	–
	(0.02397)	(0.02401)	(0.02508)	(0.02458)		
FEMALE	-0.04013	-0.03977	-0.03654	-0.03700	–	–
	(0.03808)	(0.03615)	(0.04164)	(0.03874)		
THEORY	0.03918	0.03877	0.01880	0.01206	–	–
	(0.04104)	(0.04015)	(0.04622)	(0.04359)		
ECONOMETRICS	0.09944	0.09715	0.09003	0.07793	–	–
	(0.04696)**	(0.04614)**	(0.04697)*	(0.04494)*		
MACRO/ FINANCE	0.02249	0.03232	0.00563	0.00775	–	–
	(0.03031)	(0.03009)	(0.03176)	(0.03151)		
PUBLIC/ URBAN	0.01049	0.02373	-0.00395	0.00683	–	–
	(0.04074)	(0.04297)	(0.04340)	(0.04436)		
LABOR	0.00223	0.00460	-0.01447	-0.00972	–	–
	(0.03564)	(0.03742)	(0.03871)	(0.03969)		
UNIVERSITY OF ILLINOIS – URBANA CHAMPAIGN	0.07275	0.06893	0.07359	0.07208	–	–
	(0.04392)*	(0.04138)*	(0.04773)	(0.04552)		
INDIANA UNIVERSITY – BLOOMINGTON	-0.05363	-0.05643	-0.04154	-0.03831	–	–
	(0.05005)	(0.04877)	(0.05736)	(0.05337)		

TABLE 2.6 (CONT'D)

	ALL (OLS)		EXCLUDES ASSISTANTS (OLS)		ALL (FIXED EFFECTS)	
Model	(1)	(2)	(3)	(4)	(5)	(6)
UNIVERSITY OF IOWA	0.07193	0.08345	0.08863	0.10006	–	–
	(0.04810)	(0.04585)*	(0.05034)*	(0.04840)**		
THE OHIO STATE UNIVERSITY	0.09033	0.09676	0.09225	0.09857	–	–
	(0.05386)*	(0.05414)*	(0.05701)	(0.05744)*		
PURDUE UNIVERSITY	0.11469	0.12699	0.12194	0.13166	–	–
	(0.05811)**	(0.05676)**	(0.06056)**	(0.05901)**		
UNIVERSITY OF MICHIGAN	0.05300	0.04286	0.03846	0.03682	–	–
	(0.04122)	(0.04140)	(0.04362)	(0.04488)		
UNIVERSITY OF MINNESOTA	0.15978	0.12221	0.22212	0.19809	–	–
	(0.06641)**	(0.06620)*	(0.06778)***	(0.06620)***		
UNIVERSITY OF WISCONSIN -	0.07087	0.06469	0.07300	0.06842	–	–
	(0.04277)*	(0.04300)	(0.04534)	(0.04599)		
YEAR 1995	0.08776	0.09892	0.08989	0.10369	0.20577	0.89597
	(0.01175)***	(0.01199)***	(0.01198)***	(0.01196)***	(0.01635)***	(0.84109)
YEAR 1999	0.17091	0.18445	0.17394	0.19317	0.43642	2.04345
	(0.01528)***	(0.01785)***	(0.01625)***	(0.01798)***	(0.03221)***	(1.96125)
CONSTANT	10.61883	10.69925	10.73241	10.81140	11.19398	15.98856
	(0.06547)***	(0.05145)***	(0.06595)***	(0.04885)***	(0.04738)***	(6.54109)**
Observations	601	601	537	537	608	608
R-squared	0.64	0.64	0.59	0.59	0.73	0.71

Note: Robust standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

The abolition of mandatory retirement may have had an impact on wage changes rather than entire wage paths. The question now is whether there are any differences in wage growth between people hired before and after the elimination of mandatory retirement. For the 1992-1995 period, this comparison will involve faculty hired between 1991-1992 by the departments of Michigan State University and the University of Michigan, and faculty hired during 1989-1992 by the economics department of the University of Wisconsin, to faculty hired before 1992 in all other departments. As of 1992, there are 10 faculty members (4 Professors, 3 Associates and 3 Assistants, or 6% of the sample) who joined departments that eliminated mandatory retirement by state law after this change took effect. The reason why faculty hired during 1992-1995 are excluded is because the wage growth cannot be calculated for them due to the missing wage observations for year 1992. For the 1995-1999 period faculty joining the other departments during 1994-1995 (after mandatory retirement was abolished by federal law), as well as faculty joining the departments without mandatory retirement during 1992-1995 (Michigan and Michigan State) and during 1992-1995 (Wisconsin) are added to the analysis. These faculty members also enter contracts without a mandatory retirement clause, and have salary observations available for both 1995 and 1999. There are 20 additional faculty members that fall in this category as of 1999 (for a total of 30, or 16% of the sample).

The specification for this analysis is presented below:

$$(6)\quad [\text{Log}(\text{SALARY}_{i,\,t+\Delta t}) - \text{Log}(\text{SALARY}_{it})] = \alpha_1 + \beta_{11}\text{PUBLINDX}_{it} + \beta_{12}\text{VOLUME}_{it} + \beta_{13}\text{OTHCITES}_{it} + \beta_{14}\text{EXPER}_{it} + \beta_{15}\text{EXPER}_{it}^2 + \beta_{16}\text{HRDAFTER}_{it} + \beta_{17}(\text{EXPER}_{it} * \text{HRDAFTER}_{it}) + \beta_{18}\text{AWARD}_{it} +$$

$$\beta_{19}ADMIN_{it} + \beta_{1,10}CHAIR_{it} + \beta_{1,11}TOP10_i + \beta_{1,12}FEMALE_i + \delta_{1k}FIELD_i + \theta_lDEPARTMENT_i + u_{it}$$

Given this specification, the question posed becomes: given the productivity and other employee characteristics as of time *t*, what wage changes were instituted for the period preceding *t* + *t*, and were they different for faculty entering contracts without a mandatory retirement clause? Table 2.7 presents the regression results.

The first regression suggests that the wage increases over 1992-1995 for faculty hired after the change in law, are about 1.9 percentage points higher than the rest. The negative experience effect should be interpreted in the context of performance being held constant. Given that almost all observable performance components are included, more experience suggests a longer time required to achieve the same performance targets. The effect disappears for 1995-1999. From these results it can be concluded that the effect of the change in law on salary increases is weak at best.

TABLE 2.7– TESTING FOR DIFFERENCES IN WAGE GROWTH RATES

Dependent Variable: Δ LOG(SALARY)		
Model	(1) (1992-1995)	(2) (1995-1999)
PUBLICATION INDEX (based on the impact factor)	0.00007 (0.00008)	-0.00001 (0.00009)
VOLUME OF RESEARCH	0.00002 (0.00002)	-0.00001 (0.00003)
CITATIONS FROM OTHERS	0.00002 (0.00003)	0.00000 (0.00003)
EXPERIENCE	-0.01987 (0.00977)**	-0.00288 (0.00550)
$(\text{EXPERIENCE})^2$	0.00037 (0.00024)	-0.00011 (0.00013)
HRDAFTER	-0.15685 (0.07732)**	0.09034 (0.08196)
EXPERIENCE * HRDAFTER	0.01881 (0.00908)**	-0.00214 (0.00806)
AWARDS	0.00700 (0.03954)	-0.01298 (0.02656)
ADMINISTRATIVE DUTIES	-0.02462 (0.02304)	0.04367 (0.02373)*
CHAIR	0.09979 (0.06125)	0.15217 (0.09419)
TOP10	0.02314 (0.02675)	0.02971 (0.01919)
FEMALE	-0.00761 (0.03507)	-0.06166 (0.02686)**
THEORY	-0.07910 (0.03513)**	-0.01467 (0.03253)
ECONOMETRICS	0.00157 (0.03298)	-0.05005 (0.02816)*
MACRO/ FINANCE	-0.02976 (0.02997)	0.01603 (0.02060)
PUBLIC/ URBAN	-0.03867 (0.02624)	0.00200 (0.03342)

TABLE 2.7 (CONT'D)

MODEL	(1)	(2)
LABOR	-0.00338	-0.04182
	(0.02771)	(0.03684)
UNIVERSITY OF ILLINOIS – URBANA	0.02897	-0.00194
CHAMPAIGN	(0.03933)	(0.02798)
INDIANA UNIVERSITY – BLOOMINGTON	-0.07350	-0.04702
	(0.02735)***	(0.02855)
UNIVERSITY OF IOWA	-0.00512	0.05639
	(0.03116)	(0.03392)*
THE OHIO STATE UNIVERSITY	0.00127	-0.00937
	(0.03798)	(0.02943)
PURDUE UNIVERSITY	0.00375	0.04729
	(0.03367)	(0.03062)
UNIVERSITY OF MICHIGAN	-0.00463	0.05150
	(0.02472)	(0.03295)
UNIVERSITY OF MINNESOTA	0.04427	0.21201
	(0.08854)	(0.09155)**
UNIVERSITY OF WISCONSIN - MADISON	-0.06308	0.03901
	(0.02870)**	(0.04146)
CONSTANT	0.36013	0.22187
	(0.09567)***	(0.05893)***
Observations	180	188
R-squared	0.26	0.42

Note: Robust standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

By consolidating all the evidence gathered in the above analysis, the emerging impact of state and federal legislation on Lazear contracts for economics faculty in the Big Ten seems to be a positive one, in the sense that Lazear contracts appear stronger after the elimination of mandatory retirement. These findings are similar in nature to the ones obtained by Neumark and Stock (1999) using a broader group of workers over a substantially longer period of time. The possibility that the positive effect in Neumark and Stock is due to reduced chances for reneging from the side of the firm, does not apply in this setting. Once faculty are granted tenure, the department has very few options for terminating a contract. In the previous section, it was mentioned that, after the abolition of mandatory retirement, post-tenure reviews, whether formal or informal, may have intensified. But there is no evidence in the literature of increased dismissal of tenured faculty.

One explanation for the observed steeper wage-experience profiles, may hinge on the extended length of the contract life-cycle. Because the day of separation has been postponed, this may have resulted in a more permanent attachment to the job. As a result the Lazear contracts appear stronger. The hypothesis of flatter wage profiles after the elimination of mandatory retirement rests on the implicit assumption that Lazear contracts are already in place and negotiated on a widespread basis. However, it could also be the case that Lazear contracts were not that strong before and not everyone was implicitly offered one. The department most likely possesses inside (unobserved) information on the probability that a newly hired faculty stays with the department (depending on the quality of the match) and may incorporate these considerations into the contract offered. The elimination of mandatory retirement may increase the chances that

someone stays with the department longer, especially lateral movers in later stages of their work lifecycle. As a result, more Lazear contracts that would not be offered before are now offered. The evidence supports this hypothesis by showing that lateral movers are on steeper wage profiles, and more so than first-time entrants. However, it remains unclear why faculty would want to separate at any point in time given that the structure of faculty retirement programs offers few incentives to do so.

The first chapter discussed a tournament incentive scheme. If both incentive schemes were thought to be weaved into the same payment structure to achieve the same motivation goals, and be thought of as substitutes, one hypothesis would be that a strengthening of Lazear contracts would result in a decreased emphasis on the tournament incentive scheme. It is interesting to observe that the data support the possibility of such a shift of emphasis from one incentive system to the other. Table 2.8 below presents results showing that the wages of faculty entering contracts without a mandatory retirement clause depend much less on the ordinal rank within the department than the wages of the rest of the faculty.

The standard regression model estimated earlier is enhanced to include an interaction term between rank order and the dummy indicating that the faculty member was hired after the elimination of mandatory retirement (RANK * HRDAFTER). Interestingly, the estimated coefficient on this interaction term is positive and significant, suggesting a reduction in the weight of rank order in the salaries of faculty hired after the elimination of mandatory retirement. According to the first regression in Table 2.8, for faculty hired after the abolition of mandatory retirement, one step up in rank increases salary by only 0.6% ($|-0.39 + 0.22| \times 0.036 \times 100\%$). For all other faculty, one step up in

rank order is associated with a 1.4% increase in salary (|-0.39| x 0.036 x 100%). A fixed effects estimation confirms these findings. In fact, the second and third regressions suggest a reversal in the effect of rank order for faculty hired after the legislation change (the positive effect of the interaction term more than offsets the negative effect of the rank-order variable). A heavier reliance on Lazear contracts and decreased emphasis on tournament incentives would be a plausible explanation for this observation.

TABLE 2.8– THE POTENTIAL IMPACT OF THE CHANGE IN LEGISLATION ON TOURNAMENT INCENTIVES

Dependent Variable: LOG(SALARY)	OLS	FIXED EFFECTS	
Model	(1)	(2)	(3)
PUBLICATION INDEX (based on the impact factor)	0.00036 (0.00011)***	0.00053 (0.00016)***	0.00053 (0.00015)***
VOLUME OF RESEARCH	-0.00001 (0.00002)	0.00002 (0.00004)	0.00002 (0.00004)
CITATIONS FROM OTHERS	0.00002 (0.00006)	0.00000 (0.00016)	–
EXPERIENCE	0.02110 (0.00491)***	–	–
$(EXPERIENCE)^2$	-0.00021 (0.00011)**	-0.00059 (0.00010)***	-0.00059 (0.00010)***
HRDAFTER	-0.16877 (0.08492)**	–	–
EXPERIENCE * HRDAFTER	0.01098 (0.00536)**	0.01423 (0.00617)**	0.01423 (0.00616)**
RANK ORDER	-0.39411 (0.08074)***	-0.21213 (0.14004)	-0.21223 (0.11725)*
DISTANCE BETWEEN RANKS	0.00001 (0.00003)	-0.00004 (0.00007)	-0.00004 (0.00007)
RANK * HRDAFTER	0.22488 (0.09241)**	1.20921 (0.50457)**	1.20922 (0.50376)**
AWARDS	0.06658 (0.03430)*	0.02397 (0.07598)	0.02398 (0.07562)
ADMINISTRATIVE DUTIES	0.13905 (0.04870)***	0.06231 (0.03719)*	0.06231 (0.03713)*
CHAIR	0.16780 (0.03534)***	0.10426 (0.03867)***	0.10426 (0.03854)***
TOP10	0.01001 (0.02218)	–	–
FEMALE	-0.01869 (0.03568)	–	–
THEORY	0.05251 (0.03455)	–	–
ECONOMETRICS	0.08625 (0.03986)**	–	–
MACRO/ FINANCE	0.02891 (0.02815)	–	–

TABLE 2.8 (CONTINUED)

	OLS	FIXED EFFECTS	
MODEL	(1)	(2)	(3)
PUBLIC/ URBAN	0.01854	–	–
	(0.03856)		
LABOR	-0.02752	–	–
	(0.03833)		
UNIVERSITY OF ILLINOIS – URBANA	0.02996	–	–
CHAMPAIGN	(0.03873)		
INDIANA UNIVERSITY – BLOOMINGTON	-0.04401	–	–
	(0.04082)		
UNIVERSITY OF IOWA	0.03070	–	–
	(0.03991)		
THE OHIO STATE UNIVERSITY	0.07794	–	–
	(0.05085)		
PURDUE UNIVERSITY	0.04570	–	–
	(0.06147)		
UNIVERSITY OF MICHIGAN	0.08775	–	–
	(0.04146)**		
UNIVERSITY OF MINNESOTA	0.17894	–	–
	(0.06022)***		
UNIVERSITY OF WISCONSIN - MADISON	0.13027	–	–
	(0.04248)***		
YEAR 1995	0.10190	0.20559	0.20559
	(0.01242)***	(0.01628)***	(0.01612)***
YEAR 1999	0.19237	0.43440	0.43440
	(0.01670)***	(0.03219)***	(0.03209)***
CONSTANT	10.87982	11.20869	11.20876
	(0.07553)***	(0.09486)***	(0.07458)***
Observations	601	608	608
R-squared	0.68	0.74	0.74

Note: Robust standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

C. Testing Lazear's Delayed-Payments Theory Using Data from the Current Population Survey

The Current Population Survey (CPS) offers data that would allow testing for the impact of the elimination of mandatory retirement on faculty salary profiles. The Outgoing Rotations of 1979-2002 are used to extract earnings and demographic data on people who report to be college or university teachers. Since no more information is available on the occupation (other than subject taught), the assumption is made that most of the observations include tenured or tenure-track faculty. The analysis of this section follows closely Neumark and Stock (1999). The identifying information comes from states that abolished mandatory retirement by state law before January 1, 1994, when the federal law came into effect. Table 2.9 below summarizes important changes in state laws that affected mandatory retirement for faculty. It appears that only eight states eliminated mandatory retirement by state law before 1994: Connecticut, Florida, Hawaii, Maine, Michigan, Texas, Utah, and Wisconsin. These states will be part of Group 1, or the group eliminating mandatory retirement for both private and public institutions. Group 2 will include states that abolished mandatory retirement for public (most often State) employees. The analysis will be carried out first using only Group 1 states, and then adding information from Group 2 states. One reason for this breakdown is that Group 2 states, when prohibiting mandatory retirement for State employees, do not make specific reference to tenured faculty. From reviewing various cases and judges' opinions, it appears that, for the most part, the law applied to faculty in state institutions of higher education.

TABLE 2.9 IMPORTANT CHANGES IN STATE LAWS REGARDING MANDATORY RETIREMENT OF TENURED FACULTY

Group	State	Year	Legislation	Coding	
				MR1	MR2
2	Arizona	1988	Removes requirement that members of the State retirement system retire at the age of 70.	0	1*
		1978	Raises mandatory retirement age from 65 to 70 for State employees with provision for continued employment after age 70 upon application and approval.	0	0
1	Arkansas	1989	Extends protection under age discrimination laws to State and local employees over 40 (instead of those between 40 and 70). Permits mandatory retirement for executive and high-policymaking employees and until January 1, 1994 for tenured employees at institutions of higher education.	0	0
1		1979	Prohibits age discrimination in State and local public employment against persons age 40-70, with certain exceptions. Employees over age 70 may continue in employment with annual written authorization from their employers.	0	0
1	California	1983	Abolishes mandatory retirement for State and local public employees except those in public safety. Able employees may work beyond mandatory retirement date upon written request as previously applicable to private sector employees. Also mandatory retirement permitted for tenured higher- education faculty, and certain executives and physicians.	0	0

TABLE 2.9 (CONT'D)

1		1979	Permits mandatory retirement of tenured faculty after age 65 prior to July 1, 1982, and age 70 thereafter, and mandatory retirement for persons in executive and high policy-making positions at age 65.	0	0
1		1978	Allows private and public sector employees to work beyond the normal retirement date if the employee so requests in writing and demonstrates continued adequate job performance.	0	0
1	Colorado	1979	Raises mandatory retirement age to 70 from 65 for State employees but allows employment continuation on a year-to-year basis beyond retirement age.	0	0
1	Connecticut	1989	Prohibits mandatory retirement of tenured employees at independent institutions of higher education, effective July 1, 1993.	1	1
2	Delaware	1986	Allows state employees to work after mandatory retirement age of 70 on a year-to-year basis upon written approval.	0	0
1	District of Columbia	1988	Removes mandatory retirement requirements (at the age of 70) for public school teachers.	0	0
2	Georgia	1984	Abolishes mandatory retirement under the State employee's retirement system except for specified public safety personnel.	0	1*
1	Florida	1976	Enacts age discrimination law abolishing mandatory retirement for State and local government employees at age 65.	1	1
1	Hawaii	1984	Bans mandatory retirement in both the public and private sectors.	1	1

TABLE 2.9 (CONT'D)

1	Illinois	1987	Removes the upper age limit for coverage of the prohibition against age discrimination in employment. Allows mandatory retirement for the employment groups specified in the Federal ADEA.	0	0
1	Indiana	1979	Raises upper limit for State protection from 65 to 70. Mandatory retirement age for teachers is raised from 66 to 71 with provision for work beyond that age with a doctor's certificate.	0	0
2	Iowa	1979	Bans mandatory retirement for State employees. Other public sector employees may work beyond age 70 with employer approval, except for police and firefighters, who must retire at age 65.	0	1*
1	Kansas	1988	Amends ADEA to extend coverage to all persons over 18 (rather than those between 40 and 70). Allows mandatory retirement for and high-policymaking employees and until January 1, 1994 for tenured employees at institutions of higher education.	0	0
1		1977	Abolishes mandatory retirement of State employees through an Executive Order, effective November 23, 1977.	0	0
2	Louisiana	1987	Repeals mandatory retirement provisions at age 70 for public employees. Mandatory retirement of law enforcement personnel and firefighters continues to be required at age 65.	0	1*
1	Maine	1979	Prohibits mandatory retirement at any age in private employment (formerly applying only to public employment) beginning January 1, 1980.	1	1
2	Maryland	1988	Removes mandatory retirement at the age of 70 for those under the State employees and teachers retirement systems.	0	1*

TABLE 2.9 (CONT'D)

1	Michigan	1991	Amends the Civil Rights Act to prohibit mandatory retirement for employees of institutions of higher education who are serving under unlimited tenure.	1	1
1	Minnesota	1987	Eliminates mandatory retirement at age 70 for State employees in the executive branch under the State retirement system or the teachers' retirement system except for tenured faculty at institutions of higher education. Correctional and law enforcement personnel remain subject to mandatory retirement requirements.	0	0
1		1979	Changes the date for prohibition of mandatory retirement from June 1, 1980 to April 24, 1979, for *most* private and public employees.	0	0
1	Mississippi	1980	Raises mandatory retirement age for public employees from 65 to 70 applicable to all, except elected officials and those appointed by the Governor.	0	0
1	Nebraska	1987	Removes mandatory retirement requirements at the age of 70 for employees of public institutions of higher education, with the exception of tenured faculty members or law enforcement personnel.	0	0
1		1982	Allows county, state, and school system employees to work beyond mandatory retirement age with the approval of their employers.	0	0
1		1979	Raises upper limit for age discrimination prohibition from 65 to 70 years (lower limit is 40). Mandatory retirement age for state employees is raised to 70.	0	0
2	New Hampshire	1979	Abolishes mandatory retirement for all public sector employees and those in the private sectors whose employers employ 6 workers or more.	0	1*†

TABLE 2.9 (CONT'D)

1	New Jersey	1985	Prohibits mandatory retirement in the private and public sectors except for public safety employees, judges, tenured higher-education faculty, and certain executive, but employers may refuse to hire or promote any person over 70.	0	0
2	North Carolina	1984	Bans mandatory retirement under the State and local government employees' retirement systems. Local boards of education and the Board of Governors of the University of North Carolina may provide for the retirement of certain personnel at the age of 70 provided they allow for continued service on a year-to-year basis beyond mandatory retirement age.	0	1*†
2		1979	Raises mandatory retirement age for public employees from 65 to 70 and allows year-to-year employment after that with employer's approval.	0	0
1	Ohio	1990	Changes provisions of mandatory retirement for executives and high policymaking personnel and tenured employees at institutions of higher education to coincide with the Federal ADEA.	0	0
1	South Carolina	1988	Allows mandatory retirement provisions for tenured faculty at institutions of higher education as well as executive and high policymaking employees until January 1, 1994. Until this time allows age-related hiring and discharge decisions for firefighters and law enforcement officers.	0	0
1		1979	Extends the human affairs law previously applicable to public employees only, to the private sector employers of 15 or more workers. Mandatory retirement age for teachers was increased from 65 to 70, with an extension to 72 possible upon approval of annual requests.	0	0
1	South Dakota	1987	Removes mandatory retirement clause (at age 70) for State employees and teachers, except for tenured faculty at State colleges.	0	0

TABLE 2.9 (CONT'D)

1		1979	Raises mandatory retirement age from 65 to 70 for public employees except for law enforcement officers and firemen for whom the mandatory retirement age remains 55.	0	0
1	Tennessee	1988	Uncaps the age requirement for antidiscrimination laws applicable to both private and public employees. Allows mandatory retirement provisions for tenured faculty at institutions of higher education as well as executive and high policymaking employees until January 1, 1994.	0	0
1		1979	Eliminates mandatory retirement age of 70 from the retirement system for State employees, except for firefighters, police officers, wildlife officers and State university employees.	0	0
1	Texas	1989	Uncaps the mandatory retirement age. Mandatory retirement is also prohibited for tenured faculty at institutions of higher education.	1	1
1	Utah	1987	Extends coverage of the age discrimination prohibition to those 70 and over (instead of only those between 40 and 70). Mandatory retirement is no longer permitted, except for executives and high policymaking employees who are required to retire at 65.	1	1
2	Vermont	1987	Removes mandatory retirement requirement at the age of 70 for municipal employees.	0	1†
2		1981	Amends age discrimination law to prohibit age discrimination to persons 18 or older. Bans mandatory retirement for everybody except police and firefighters and tenured faculty at higher education institutions who are required to retire at 65 (after July 1982, at 70).	0	0

TABLE 2.9 (CONT'D)

2	Virginia	1987	Removes mandatory retirement requirement at the age of 70 for state employees and professional or clerical employees of county, city, or other local public school boards.	0	1*†
2		1980	Sets mandatory retirement age at 70 for State employees and teachers under the State Supplemental Retirement Act. Formerly the employer could require mandatory retirement at any age between 65-70.	0	0
1	Washington	1979	Raises mandatory retirement age from 65 to 70 for all public employees except police and firefighters. Mandatory retirement at 70 can be waived by an individual's employer.	0	0
1	Wisconsin	1989	Bans mandatory retirement.	1	1
1	United States	1994	Allows the exemption from mandatory retirement protection for tenured faculty to expire.	1	1

Source: Monthly Labor Review (all years), Neumark and Stock (1999), Northrup (1980), Rees and Smith (1991).

Notes:
* State government employees
† Local government employees
For states not in this list it is assumed that federal law applies.

The variables MR1 and MR2 appearing in the last columns of Table 2.9 are dummy variables indicating if a law banning mandatory retirement was in place in a particular state and year. The specification for this analysis will be similar to model (4) in the previous subsection with the following modifications:

$$(7) \quad \text{Log}(SALARY_{it}) = \alpha + \beta_{11}AGE_{it} + \beta_{12}AGE^2_{it} + \beta_{13}(HRDAFTER^j_{it} * AGE_{it}) + \beta_{14}HRDAFTER^j_{it} + \beta_{15}STATEMP_{it} + \beta_{16}LOCEMP_{it} + \beta_{17}UNION_{it} + \beta_{18}MARRIED_{it} + \beta_{19}FEMALE_i + \beta_{1,10}BLACK_i + \beta_{1,11}OTHER_i + \delta YEAR_t + \theta_l STATE_i + u_{it}$$

where:

i = person
t = year
j = G1, G2 depending on whether Group 1 or Group 2 states are used
l = state

Data from 1983-2002 are extracted from the Outgoing Rotations of the CPS, and only persons whose occupation is teacher in college or university are kept. Persons with less than a Bachelor's degree and persons younger than 24 are dropped from the analysis. One reason for this is that people younger than 24 either will not have had the time to obtain at least a Bachelor's degree, or they are unlikely to be tenured or tenure-track postsecondary teachers. Because the education variable before 1991 does not distinguish among people with a Bachelor's degree or more, the analysis cannot be restricted to Ph.D. recipients alone if the years before 1991 are to be included. The analysis was replicated for 1992-2002, using only persons who have a doctorate degree. This group would be more likely to include tenured or tenure-track faculty. The problem with this approach is that most states passed legislation banning mandatory retirement for tenured faculty before 1992, and most useful variation is lost when years before 1992 are

excluded. In addition, only 10 years of observations are now available instead of 19. The sample size is reduced by about eight times, and this may be partly the reason why no meaningful results were obtained when using this second approach. This section only provides results based on the 1983-2002 period.

The CPS, unfortunately, does not provide information on job tenure, so the variable HRDAFTER is inferred in a similar fashion as in Neumark and Stock. The NSF *Survey of Doctoral Recipients for the United States* for several years reports the median age at the time of doctorate to be slightly less than 34. As a result the variable HRDAFTER is constructed by assigning a value of one to people age 34 or less in the year that mandatory retirement legislation is passed. In the next year, the variable HRDAFTER receives a value of one if the person is 35 years old or more, and so on. Because not all faculty teaching in postsecondary institutions are required to have a Ph.D. degree, the age at the time of first hire is likely less than 34. The analysis was replicated using age 31 as the cutoff point, but the results were not very sensitive to this change. People older than 34 in the year when mandatory retirement legislation was passed are excluded from part of the analysis. The treatment and control groups will be described in more detail later in the chapter.

The dependent variable SALARY refers to weekly earnings. After restricting the sample to teachers in postsecondary institutions, there were no observations involving unemployed, part-time workers, or workers with a job but not currently working. Other variables used include sector, which distinguishes between private, state government (STATEMP), and local government (LOCEMP). Federal employees are dropped from the analysis (there are fewer than 100 such observations for the 1983-2002 period). The

job sector cannot be identified in detail before 1983. In particular, before this year it is impossible to distinguish whether the person is working for the federal, state, or local government. The laws, however, distinguished among these different employers and the information would be quite necessary for the analysis. The cleanest approach would be to drop observations for the years before 1983. The analysis was replicated using all years and assuming that all government employees during the 1979-1982 period were employed by their respective state government (in later years, this is the prevalent group), but the results did not differ substantially.

It should be noted that the variable HRDAFTER for this portion of the analysis only refers to (potentially) people being hired for the first time. Because there is no information on lateral entry, it is assumed that all older faculty have entered contracts when they were about 34 years old, and have not switched institutions or states since. This assumption is quite restrictive, especially after considering that people with higher accumulated human capital are more mobile, and the current group is likely to include a large number of migrants. However, it is impossible to separate migrants from this group without drastically reducing the sample size.[63] Another important shortcoming of this analysis when compared to the previous one is that productivity data are not available. An advantage, however, is that cohort effects are less likely to influence the findings when using CPS data, since, for every year, a mostly different faculty cohort is surveyed, rather than the same group followed over time (and aging during the process). There is

[63] Potentially, the March supplements could be used instead to find out if the person has migrated from a different state in the previous year. This would resolve most of the problem, although the issue of people switching institutions within the same area would remain unaddressed. However, the March supplements yield very small sample sizes for the current purposes and would, as a result, provide little information on the subject.

no apparent reason why different cohorts from the CPS should have a different average age, unless by chance.

The discussion now turns to the empirical results. First, model (7) is estimated using the entire sample. In particular, the treatment group will be composed of people 34 years or younger at the time that mandatory retirement was banned by state or federal law, as of the year when the legislation change took effect, or 35 years or younger the year after the change, and so on. The control group will include all faculty before the change in legislation (state or federal), as well as all faculty over 34 the year of the change, over 35 the year after the change, and so on. This will be referred to as the unrestricted control group. First, only Group 1 states will be considered and the federal law will be assumed to apply to the rest of the states including Group 2 states. The regression results appear in the first four columns of Table 2.10 below. Age appears to have a large and significant impact on earnings. The estimates suggest about 16% higher wages for the first additional year of experience. The effect gradually declines in the hypothesized quadratic fashion, and is expected to turn around at about 52 years of age. The interaction of interest becomes stronger as controls for occupation (here, type of subject taught), and state are added. It remains statistically insignificant, although the p-values are in the lower teens for the most part. The same observations hold for the case when Group 2 states are added to the analysis. The estimated coefficients on other variables confirm previous findings in the literature. It appears that faculty employed by state and local institutions are paid less, and significantly so in the case of local institutions (about 15-17% less). Faculty whose wages are negotiated by a union appear to earn about 8% more. The demographic variables indicate that females earn about 30-

32% less, blacks earn about the same as whites, whereas faculty other than black or white earn about 10% less.

TABLE 2.10 – TESTING FOR THE EFFECT OF MANDATORY RETIREMENT LEGISLATION ON WAGE PROFILES (UNRESTRICTED CONTROLS)

	Dependent Variable: Log (Salary)							
MODEL	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
AGE	0.16303 (0.00430)***	0.16166 (0.00427)***	0.16062 (0.00428)***	0.14633 (0.00923)***	0.16279 (0.00439)***	0.16163 (0.00436)***	0.16176 (0.00437)***	0.14725 (0.00922)***
$(AGE)^2$	-0.00154 (0.00004)***	-0.00153 (0.00004)***	-0.00152 (0.00004)***	-0.00152 (0.00005)***	-0.00154 (0.00004)***	-0.00153 (0.00004)***	-0.00153 (0.00004)***	-0.00153 (0.00005)***
$HRDAFTER^{G1}$ * AGE	0.00141 (0.00210)	-0.00008 (0.00208)	0.00361 (0.00220)	0.00327 (0.00245)	–	–	–	–
$HRDAFTER^{G1}$	-0.02505 (0.07968)	0.03562 (0.07902)	-0.06280 (0.08124)	-0.04696 (0.09630)	–	–	–	–
$HRDAFTER^{G2}$ * AGE	–	–	–	–	0.00105 (0.00200)	-0.00038 (0.00198)	0.00240 (0.00205)	0.00200 (0.00226)
$HRDAFTER^{G2}$	–	–	–	–	-0.01822 (0.07752)	0.04276 (0.07688)	-0.01668 (0.07826)	0.00318 (0.09043)
STATEMP	-0.01735 (0.01163)	-0.01691 (0.01159)	0.01987 (0.01253)	0.01983 (0.01253)	-0.01836 (0.01167)	-0.01818 (0.01163)	0.01624 (0.01255)	0.01601 (0.01256)
LOCEMP	-0.16786 (0.02090)***	-0.15314 (0.02084)***	-0.14776 (0.02143)***	-0.14595 (0.02145)***	-0.16795 (0.02090)***	-0.15330 (0.02085)***	-0.14925 (0.02143)***	-0.14744 (0.02145)***
UNION	0.08078 (0.02412)***	0.07621 (0.02388)***	0.08006 (0.02389)***	0.07923 (0.02390)***	0.08122 (0.02412)***	0.07675 (0.02388)***	0.08059 (0.02389)***	0.07981 (0.02390)***

TABLE 2.10 (CONT'D)

MARRIED	0.03892 (0.01208)***	0.02687 (0.01200)**	0.03014 (0.01201)**	0.02943 (0.01202)**	0.03890 (0.01208)***	0.02670 (0.01200)**	0.02997 (0.01201)**	0.02919 (0.01202)**
FEMALE	-0.32922 (0.01128)***	-0.30967 (0.01161)***	-0.30967 (0.01156)***	-0.30852 (0.01157)***	-0.32949 (0.01127)***	-0.30987 (0.01161)***	-0.30995 (0.01156)***	-0.30878 (0.01156)***
BLACK	0.01989 (0.02659)	0.01523 (0.02639)	0.00734 (0.02673)	0.00726 (0.02674)	0.01903 (0.02660)	0.01427 (0.02640)	0.00631 (0.02673)	0.00622 (0.02674)
OTHER	-0.09285 (0.01971)***	-0.09346 (0.01964)***	-0.10140 (0.01975)***	-0.10119 (0.01978)***	-0.09285 (0.01972)***	-0.09367 (0.01964)***	-0.10132 (0.01975)***	-0.10114 (0.01978)***
CONSTANT	1.55055 (0.13832)***	1.64998 (0.15017)***	1.50343 (0.15984)***	2.05178 (0.35073)***	1.55752 (0.13975)***	1.65273 (0.15140)***	1.48185 (0.16115)***	2.04111 (0.35039)***
OTHER CONTROLS								
OCCUPATION DUMMIES	no	yes	yes	yes	no	yes	yes	yes
STATE DUMMIES	no	no	yes	yes	no	no	yes	yes
YEAR DUMMIES	yes	yes	yes	yes	yes	yes	yes	yes
YEAR-AGE INTERACTIONS	no	no	no	yes	no	no	no	yes
OBSERVATIONS	16,973	16,973	16,973	16,973	16,973	16,973	16,973	16,973
R-SQUARED	0.30	0.32	0.33	0.33	0.30	0.32	0.33	0.33

Note: Standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%
The above estimations include the unrestricted control group.

The change in legislation may have had a different impact on faculty employed by private institutions, compared to faculty employed by state or local colleges and universities. For the case when mandatory retirement was abolished for both the private and public sector, private institutions may have had a larger flexibility in the retirement systems they provide, and may have adjusted faster to the change. According to the *Survey of Changes in Faculty Retirement Policies,* a higher percentage of private institutions offer early and phased retirement programs, and negotiate buyouts when compared to public institutions. Because the earlier analysis employing Big Ten faculty data only included public institutions, such differences could not be observed. To capture any potential differences in the impact of the elimination of mandatory retirement for public and private institutions, the model below is introduced:

$$(8)\quad \text{Log}(SALARY_{it}) = \alpha + \beta_{11}AGE_{it} + \beta_{12}AGE^2_{it} + \beta_{13}(HRDAFTER^j_{it} * AGE_{it}) + \beta_{14}HRDAFTER^j_{it} + \beta_{15}(HRDAFTER^j_{it} * AGE_{it} * STATEMP_{it}) + \beta_{16}(HRDAFTER^j_{it} * AGE_{it} * LOCEMP_{it}) + \beta_{17}STATEMP_{it} + \beta_{18}LOCEMP_{it} + \beta_{19}UNION_{it} + \beta_{1,10}MARRIED_{it} + \beta_{1,11}FEMALE_i + \beta_{1,12}BLACK_i + \beta_{1,13}OTHER_i + \delta_t YEAR_t + \theta_i STATE_i + u_{it}$$

Table 2.11 below presents the estimates. The treatment and control groups remain the same as in the previous estimation. The first four columns present estimations using only Group 1 states. The interaction $HRDAFTER^{G1}_{it} * AGE_{it}$ appears significant, suggesting an increase in the slope of the age-earnings profile after the change in legislation. The effect is small, about 0.39 percentage points (compared to the 1 percentage point estimated with the Big Ten data). The triple interactions $HRDAFTER^{G1}_{it} * AGE_{it} * STATEMP_{it}$ and $HRDAFTER^{G1}_{it} * AGE_{it} *$

$LOCEMP_{it}$ have negative estimated signs, suggesting a smaller increase in the earnings slope for faculty employed by state and local institutions. The net effect on the slope remains positive, since the estimated coefficients on the triple interactions do not exceed in magnitude the coefficient of $HRDAFTER^{G1}_{it} * AGE_{it}$. This interaction appears much stronger, and about twice as large in regressions (3) and (4), when state and year-age interactions are added. Its magnitude suggests steeper age profiles of about 0.5-0.6 percentage points for faculty entering contracts after the change in legislation. This effect does not change substantially when states in Group 2 are added to the analysis. The results are presented in the last four columns of Table 2.11. The coefficients and significance on the terms $HRDAFTER^{G2}_{it} * AGE_{it} * STATEMP_{it}$ and $HRDAFTER^{G2}_{it} * AGE_{it} * LOCEMP_{it}$ do not change substantially, as evidenced by results in columns (5) and (6).

Since the states in Group 2 passed legislation affecting the public sector, it is interesting to see whether the wage slopes changed differently for the public sector for these states compared to the others. Columns (7) and (8) present estimates from regressions that include both triple interactions for Group 1 and Group 2 states. The question they attempt to answer is: given the differences in the public and private sectors with respect to changes in wage profiles, did these profiles change any differently for states that passed legislation affecting only the public sector, compared to states that passed legislation concerning both sectors? The fact that $HRDAFTER^{G1}_{it} * AGE_{it} * STATEMP_{it}$ and $HRDAFTER^{G1}_{it} * AGE_{it} * LOCEMP_{it}$ appear insignificant in regressions (7) and (8) suggests that the effect of mandatory retirement laws on wage

profiles was similar in Group 1 states as compared to Group 2 states, and for both these groups wage profiles have steepened overall, although less so for faculty employed by the public sector.

TABLE 2.11 – TESTING FOR THE EFFECT OF MANDATORY RETIREMENT LEGISLATION ON WAGE PROFILES (UNESTRICTED CONTROLS)

	Dependent Variable: Log (Salary)							
MODEL	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
AGE	0.16265 (0.00430)***	0.16127 (0.00427)***	0.16031 (0.00428)***	0.14615 (0.00923)***	0.16120 (0.00437)***	0.14703 (0.00922)***	0.16119 (0.00437)***	0.00000 (0.00000)
$(AGE)^2$	-0.00154 (0.00004)***	-0.00152 (0.00004)***	-0.00151 (0.00004)***	-0.00152 (0.00005)***	-0.00152 (0.00004)***	-0.00153 (0.00005)***	-0.00152 (0.00004)***	-0.00153 (0.00005)***
$HRDAFTER^{G1}$ * AGE	0.00392 (0.00218)*	0.00244 (0.00217)	0.00579 (0.00227)**	0.00535 (0.00251)**	–	–	–	–
$HRDAFTER^{G1}$	-0.03718 (0.07990)	0.02340 (0.07923)	-0.06928 (0.08140)	-0.04812 (0.09638)	–	–	–	–
$HRDAFTER^{G1}$ * AGE * STATEMP	-0.00333 (0.00079)***	-0.00334 (0.00078)***	-0.00319 (0.00078)***	-0.00318 (0.00078)***	–	–	-0.00033 (0.00109)	-0.00029 (0.00111)
$HRDAFTER^{G1}$ * AGE * LOCEMP	-0.00340 (0.00151)**	-0.00341 (0.00150)**	-0.00282 (0.00150)*	-0.00289 (0.00150)*	–	–	0.00451 (0.00458)	0.00449 (0.00458)
$HRDAFTER^{G2}$ * AGE	–	–	–	–	0.00511 (0.00216)**	0.00450 (0.00235)*	0.00503 (0.00222)**	0.00446 (0.00237)*
$HRDAFTER^{G2}$	–	–	–	–	-0.04139 (0.07851)	-0.01151 (0.09050)	-0.03902 (0.08004)	-0.01111 (0.09082)
$HRDAFTER^{G2}$ * AGE * STATEMP	–	–	–	–	-0.00303 (0.00077)***	-0.00302 (0.00077)***	-0.00276 (0.00121)**	-0.00278 (0.00123)**
$HRDAFTER^{G2}$ * AGE * LOCEMP	–	–	–	–	-0.00319 (0.00146)**	-0.00327 (0.00146)**	-0.00734 (0.00448)	-0.00741 (0.00448)*

TABLE 2.11 (CONT'D)

STATEMP	0.01162	0.01216	0.04803	0.04790	0.04475	0.04432	0.04471	0.04429
	(0.01349)	(0.01342)	(0.01429)***	(0.01429)***	(0.01450)***	(0.01451)***	(0.01451)***	(0.01451)***
LOCEMP	-0.14247	-0.12749	-0.12568	-0.12341	-0.12443	-0.12208	-0.12460	-0.12225
	(0.02296)***	(0.02288)***	(0.02343)***	(0.02346)***	(0.02356)***	(0.02359)***	(0.02356)***	(0.02359)***
UNION	0.08078	0.07621	0.08009	0.07928	0.08024	0.07954	0.08018	0.07946
	(0.02411)***	(0.02387)***	(0.02388)***	(0.02389)***	(0.02388)***	(0.02389)***	(0.02388)***	(0.02389)***
MARRIED	0.03816	0.02611	0.02941	0.02869	0.02939	0.02855	0.02938	0.02855
	(0.01208)***	(0.01199)**	(0.01200)**	(0.01201)**	(0.01200)**	(0.01201)**	(0.01200)**	(0.01201)**
FEMALE	-0.32894	-0.30933	-0.30932	-0.30815	-0.30941	-0.30822	-0.30923	-0.30804
	(0.01127)***	(0.01161)***	(0.01156)***	(0.01157)***	(0.01155)***	(0.01156)***	(0.01156)***	(0.01157)***
BLACK	0.01947	0.01496	0.00717	0.00713	0.00676	0.00671	0.00618	0.00615
	(0.02659)	(0.02639)	(0.02673)	(0.02673)	(0.02672)	(0.02673)	(0.02673)	(0.02674)
OTHER	-0.09259	-0.09324	-0.10139	-0.10122	-0.10144	-0.10139	-0.10132	-0.10124
	(0.01970)***	(0.01963)***	(0.01974)***	(0.01977)***	(0.01974)***	(0.01977)***	(0.01974)***	(0.01977)***
CONSTANT	1.54052	1.64476	1.49335	2.03935	1.47213	2.02375	1.47335	2.02551
	(0.13827)***	(0.15010)***	(0.15979)***	(0.35059)***	(0.16110)***	(0.35028)***	(0.16122)***	(0.35051)***
OTHER CONTROLS								
OCCUPATION DUMMIES	no	yes	yes	yes	yes	yes	yes	yes
STATE DUMMIES	no	no	yes	yes	yes	yes	yes	yes
YEAR DUMMIES	yes	yes	yes	yes	yes	yes	yes	yes
YEAR-AGE INTERACTIONS	no	no	no	yes	no	yes	no	yes
OBSERVATIONS	16,973	16,973	16,973	16,973	16,973	16,973	16,973	16,973
R-SQUARED	0.30	0.32	0.33	0.33	0.33	0.33	0.33	0.33

Note: Standard errors in parentheses; * significant at 10%; ** significant at 5%; *** significant at 1%.

Now the analysis restricts the control group by dropping all observations of faculty 35 years or older as of the year when the change in legislation occurred, 36 years or more a year after, and so on (for Group 1 states). For Group 2 states, the control group is restricted slightly differently. Since legislation for this group only affected the public sector, it seemed sensible to exclude not only public employees older than 35, but also private employees in this age group as of the year of the change. This way, the control group consists of faculty age 34 or younger in the private sector, as well as all faculty before the change in legislation. Table 2.12 below summarizes the treatment and control groups for both cases:

TABLE 2.12 – TREATMENT AND CONTROL GROUPS

GROUP 1 STATES	GROUP 2 STATES
Treatment Group: All faculty age 34 or younger as of time T, age 35 or younger as of time T+1, etc.	*Treatment Group:* All faculty age 34 or younger as of time T, age 35 or younger as of time T+1, etc., and working in the public sector (state or local or both).
Control Group: All faculty before time T, plus all faculty age 35 or more at time T, age 36 or more at time T+1, etc.	*Control Group:* All faculty before time T, plus all faculty after time T and working in the private sector, plus all faculty age 35 or more at time T, age 36 or more at time T+1, etc., and working for the public sector.
Restricted Control Group: All faculty before time T.	*Restricted Control Group:* All faculty before time T, plus all faculty age 34 or less as of time T and working for the private sector, age 35 or less at time T and working for the private sector, etc.

Note: T = time mandatory retirement was banned by state law.

Table 2.13 presents the new estimates using the restricted control group and only Group 1 states (treating Group 2 states the same as the rest of the states). The interaction of interest, HRDAFTER*AGE, appears significant only in regressions including all three sets of dummies (occupational, year, and state). The previous findings with respect to the differential effect of legislation on the wage paths for different sectors are confirmed. It appears that state employees are on wage paths with slopes 0.2 percentage points flatter than private employees, whereas local employees are on wage paths with even flatter slopes (about 0.3-0.4 percentage points smaller). Other variables have very similar signs and effects as before.

TABLE 2.13 – TESTING FOR THE EFFECT OF MANDATORY RETIREMENT LEGISLATION ON WAGE PROFILES (RESTRICTED CONTROLS)

	Dependent Variable: Log (Salary)							
MODEL	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
AGE	0.16361 (0.00524)***	0.16327 (0.00522)***	0.15928 (0.00522)***	0.00000 (0.00000)	0.16310 (0.00524)***	0.16271 (0.00522)***	0.15890 (0.00523)***	0.14460 (0.00940)***
$(AGE)^2$	-0.00155 (0.00006)***	-0.00155 (0.00006)***	-0.00151 (0.00006)***	-0.00151 (0.00006)***	-0.00155 (0.00006)***	-0.00154 (0.00006)***	-0.00151 (0.00006)***	-0.00151 (0.00006)***
$HRDAFTER^{G1}$ * AGE	0.00063 (0.00215)	-0.00104 (0.00214)	0.00495 (0.00235)**	0.00362 (0.00380)	0.00248 (0.00225)	0.00085 (0.00223)	0.00648 (0.00242)***	0.00494 (0.00383)
$HRDAFTER^{G1}$	-0.02592 (0.08492)	0.03645 (0.08439)	-0.11490 (0.08757)	-0.07054 (0.14370)	-0.03768 (0.08514)	0.02510 (0.08461)	-0.12120 (0.08775)	-0.06794 (0.14389)
$HRDAFTER^{G1}$ * AGE * STATEMP	–	–	–	–	-0.00207 (0.00088)**	-0.00216 (0.00087)**	-0.00205 (0.00087)**	-0.00202 (0.00087)**
$HRDAFTER^{G1}$ * AGE * LOCEMP	–	–	–	–	-0.00370 (0.00161)**	-0.00363 (0.00160)**	-0.00303 (0.00160)*	-0.00335 (0.00160)**
STATEMP	-0.05660 (0.01471)***	-0.05559 (0.01472)***	-0.00921 (0.01598)	-0.00876 (0.01598)	-0.02623 (0.01958)	-0.02375 (0.01954)	0.02153 (0.02068)	0.02153 (0.02068)
LOCEMP	-0.17625 (0.02712)***	-0.16734 (0.02705)***	-0.15627 (0.02770)***	-0.15395 (0.02775)***	-0.13189 (0.03258)***	-0.12337 (0.03246)***	-0.11855 (0.03312)***	-0.11268 (0.03320)***
UNION	0.11233 (0.02982)***	0.11112 (0.02957)***	0.11717 (0.02950)***	0.11508 (0.02951)***	0.11207 (0.02981)***	0.11081 (0.02957)***	0.11697 (0.02949)***	0.11480 (0.02950)***
MARRIED	0.06645 (0.01500)***	0.05120 (0.01492)***	0.05795 (0.01494)***	0.05576 (0.01495)***	0.06596 (0.01499)***	0.05071 (0.01492)***	0.05757 (0.01494)***	0.05537 (0.01495)***

TABLE 2.13 (CONT'D)

FEMALE	-0.29489	-0.27589	-0.27818	-0.27689	-0.29488	-0.27580	-0.27808	-0.27678
	(0.01416)***	(0.01447)***	(0.01437)***	(0.01439)***	(0.01415)***	(0.01447)***	(0.01437)***	(0.01439)***
BLACK	0.06280	0.05409	0.04645	0.04620	0.06310	0.05442	0.04683	0.04669
	(0.03490)*	(0.03472)	(0.03493)	(0.03495)	(0.03490)*	(0.03472)	(0.03493)	(0.03495)
OTHER	-0.12104	-0.12445	-0.12822	-0.12845	-0.12166	-0.12493	-0.12877	-0.12907
	(0.02240)***	(0.02240)***	(0.02253)***	(0.02257)***	(0.02240)***	(0.02239)***	(0.02253)***	(0.02256)***
CONSTANT	1.52516	1.56387	1.45238	2.00154	1.51202	1.55488	1.43808	1.99468
	(0.14832)***	(0.16459)***	(0.17557)***	(0.34876)***	(0.14840)***	(0.16464)***	(0.17563)***	(0.34878)***
OTHER CONTROLS								
OCCUPATION DUMMIES	no	yes	yes	yes	no	yes	yes	yes
STATE DUMMIES	no	no	yes	yes	no	no	yes	yes
YEAR DUMMIES	yes	yes	yes	yes	yes	yes	yes	yes
YEAR-AGE INTERACTIONS	no	no	no	yes	no	no	no	yes
OBSERVATIONS	10,038	10,038	10,038	10,038	10,038	10,038	10,038	10,038
R-SQUARED	0.34	0.36	0.37	0.38	0.34	0.36	0.37	0.38

Note: Standard errors in parentheses.
* significant at 10%; ** significant at 5%; *** significant at 1%.

The same analysis is repeated after adding Group 2 states to the list of states that passed legislation banning mandatory retirement. Table 2.14 presents the estimates. The interaction of interest, HRDAFTER*AGE, appears significant and positive in regressions including all three sets of dummies, occupation, state and year. The magnitude of the coefficient has also increased compared to previous estimations, and suggests that slopes of wage profiles have increased by 0.4-0.6 percentage points for faculty entering the job market after the ban on mandatory retirement. For faculty working in public institutions, the slope increase is smaller, ranging between 0.35 to 0.4 for state employees, and 0.18 – 0.22 for local government employees, according to regressions (5) and (6).

In sum, these results provide evidence similar to what was obtained using Big Ten faculty data, which points toward steeper wage profiles after the change in legislation. It should be noted that overall, the impact of the mandatory retirement ban on wage paths is quite weak in this analysis. Results are statistically significant only in particular specifications. However, the fact remains that all regression results point toward a strengthening of Lazear contracts after the elimination of mandatory retirement.

TABLE 2.14 – TESTING FOR THE EFFECT OF MANDATORY RETIREMENT LEGISLATION ON WAGE PROFILES (RESTRICTED CONTROLS)

	Dependent Variable: Log (Salary)					
MODEL	(1)	(2)	(3)	(4)	(5)	(6)
AGE	0.15953 (0.00551)***	0.15927 (0.00548)***	0.15625 (0.00548)***	0.14050 (0.00953)***	0.15571 (0.00548)***	0.13967 (0.00954)***
$(AGE)^2$	-0.00150 (0.00006)***	-0.00149 (0.00006)***	-0.00147 (0.00006)***	-0.00146 (0.00006)***	-0.00146 (0.00006)***	-0.00146 (0.00006)***
$HRDAFTER^{G2}$ * AGE	0.00062 (0.00207)	-0.00120 (0.00206)	0.00407 (0.00220)*	0.00425 (0.00313)	0.00584 (0.00231)**	0.00567 (0.00318)*
$HRDAFTER^{G2}$	-0.00602 (0.08163)	0.06775 (0.08110)	-0.06670 (0.08350)	-0.07817 (0.12206)	-0.08456 (0.08374)	-0.08366 (0.12208)
$HRDAFTER^{G2}$ * AGE * STATEMP	–	–	–	–	-0.00189 (0.00089)**	-0.00179 (0.00089)**
$HRDAFTER^{G2}$ * AGE * LOCEMP	–	–	–	–	-0.00358 (0.00158)**	-0.00391 (0.00158)**
STATEMP	-0.05982 (0.01501)***	-0.05898 (0.01500)***	-0.02006 (0.01619)	-0.02008 (0.01620)	0.01032 (0.02189)	0.00851 (0.02192)
LOCEMP	-0.17350 (0.02770)***	-0.16368 (0.02759)***	-0.15893 (0.02817)***	-0.15551 (0.02823)***	-0.11228 (0.03434)***	-0.10495 (0.03445)***
UNION	0.12135 (0.03024)***	0.11822 (0.02996)***	0.12812 (0.02987)***	0.12635 (0.02989)***	0.12699 (0.02987)***	0.12521 (0.02989)***

TABLE 2.14 (CONT'D)

MARRIED	0.06141	0.04515	0.05004	0.04782	0.04975	0.04751
	(0.01511)***	(0.01502)***	(0.01502)***	(0.01504)***	(0.01502)***	(0.01504)***
FEMALE	-0.29008	-0.27148	-0.27283	-0.27217	-0.27241	-0.27178
	(0.01424)***	(0.01454)***	(0.01444)***	(0.01446)***	(0.01444)***	(0.01446)***
BLACK	0.06311	0.05708	0.04673	0.04473	0.04671	0.04476
	(0.03525)*	(0.03502)	(0.03521)	(0.03524)	(0.03521)	(0.03523)
OTHER	-0.11707	-0.11900	-0.12268	-0.12209	-0.12325	-0.12272
	(0.02226)***	(0.02224)***	(0.02236)***	(0.02240)***	(0.02236)***	(0.02239)***
CONSTANT	1.59153	1.64913	1.52413	2.12585	1.51025	2.12232
	(0.15277)***	(0.16888)***	(0.17941)***	(0.34992)***	(0.17946)***	(0.34996)***
OTHER CONTROLS						
OCCUPATION DUMMIES	no	yes	yes	yes	yes	yes
STATE DUMMIES	no	no	yes	yes	yes	yes
YEAR DUMMIES	yes	yes	yes	yes	yes	yes
YEAR-AGE INTERACTIONS	no	no	no	yes	no	yes
OBSERVATIONS	9,866	9,866	9,866	9,866	9,866	9,866
R-SQUARED	0.34	0.36	0.37	0.37	0.37	0.37

Note: Standard errors in parentheses.
* significant at 10%; ** significant at 5%; *** significant at 1%.

D. ADVERSE SELECTION

According to the model developed by Guasch and Weiss (1980, 1982), adverse selection may be the reason why rising wage profiles are observed. They claim that firms sort workers, either through an apprenticeship period, or by subjecting them to testing. During the apprenticeship, workers are paid wages below their marginal product, and the ones who are revealed to be better performers are paid wages above marginal productivity. In an academic department, the process of tenure is very similar in nature to the apprenticeship described in Guasch and Weiss. Therefore, one would expect that wages of junior faculty be below the value of marginal product, whereas senior faculty should be earning a positive return to experience. To test this, model (1) reproduced below is estimated separately for junior and for tenured faculty, to see how the return to experience compares between the two groups. Results are presented in Table 2.15.

$$(1)\quad \text{Log}(SALARY_{it}) = \alpha_1 + \beta_{11}PUBLINDX_{it} + \beta_{12}VOLUME_{it} + \beta_{13}OTHCITES_{it} + \beta_{14}EXPER + \beta_{15}EXPER^2 + \beta_{16}AWARD_{it} + \beta_{17}ADMIN_{it} + \beta_{18}CHAIR_{it} + \beta_{19}TOP10_i + \beta_{1,10}FEMALE_i + \delta_{1k}FIELD_i + \theta_l DEPARTMENT_i + \lambda_{11}Y95_t + \lambda_{12}Y99_t + u_{it}$$

The findings here run opposite to what was expected. The return to experience for junior faculty is about 5.3% per additional year, whereas for tenured faculty this return is about 2%. While it is interesting to observe that experience does not turn around for junior faculty (the coefficient of squared experience is insignificant), the fact that junior faculty appear to be paid well above their productivity and more so compared to tenured faculty, is quite puzzling. From the perspective of the adverse selection model, this finding refutes its main implication. It could, however, be the case that departments have better information about the potential of junior faculty than what shows through the

performance variables employed here. During the first years, junior faculty invest heavily in human capital, and departments may be able to observe this activity before it materializes in the form of published research, and reward it accordingly. The data employed here, however, should be able to capture this effect to a certain degree, since they were collected a few years after the observation years chosen for this analysis. If, for example, in 1992 the department had a vague idea of the unpublished research or citations received by a junior faculty member, as of 2000 when the data were observed, this information had become public. Therefore, if departments really compensate junior faculty based on information that *as of the year that compensation is received* is unavailable to the market, collecting the data a few years after the fact may mitigate this problem to a certain degree.

TABLE 2.15 – TESTING THE ADVERSE SELECTION THEORY

Dependent Variable: LOG(SALARY)		
Model	(1) Assistants	(2) Tenured Faculty
PUBLICATION INDEX (BASED ON THE IMPACT FACTOR)	0.00050 (0.00086)	0.00051 (0.00013)***
VOLUME OF RESEARCH	0.00031 (0.00023)	0.00003 (0.00002)
CITATIONS FROM OTHERS	0.00017 (0.00021)	0.00019 (0.00005)***
EXPERIENCE	0.05323 (0.02895)*	0.02025 (0.00811)**
$(\text{EXPERIENCE})^2$	-0.00243 (0.00164)	-0.00029 (0.00020)
TEACHING AWARDS	-0.13136 (0.12517)	0.07012 (0.04185)*
ADMINISTRATIVE DUTIES	-0.11730 (0.09034)	0.15776 (0.04889)***
CHAIR	–	0.20720 (0.03888)***
TOP10	-0.04927 (0.06432)	0.02028 (0.02703)
FEMALE	-0.03004 (0.09843)	-0.03958 (0.04282)
THEORY	0.00726 (0.08278)	0.02391 (0.04569)
ECONOMETRICS	0.16502 (0.12241)	0.08212 (0.04501)*
MACRO/FINANCE	0.08732 (0.08463)	-0.00430 (0.03294)
PUBLIC/URBAN	0.07641 (0.09066)	-0.00053 (0.04482)
LABOR	0.03470 (0.09195)	-0.00876 (0.03783)
INDIANA UNIVERSITY - BLOOMINGTON	0.09177 (0.11662)	0.07064 (0.04918)
THE OHIO STATE UNIVERSITY	0.01971 (0.08647)	-0.05909 (0.05866)
PURDUE UNIVERSITY	0.14938 (0.10011)	0.09171 (0.05104)*
UNIVERSITY OF ILLINOIS AT URBANA	0.15089 (0.08000)*	0.09332 (0.05741)
UNIVERSITY OF IOWA	0.09520 (0.11070)	0.10800 (0.06139)*

TABLE 2.15 (CONT'D)

UNIVERSITY OF MICHIGAN	0.18329	0.03764
	(0.09727)*	(0.04517)
UNIVERSITY OF MINNESOTA	0.00556	0.18082
	(0.10189)	(0.07540)**
UNIVERSITY OF WISCONSIN – MADISON	0.11167	0.09935
	(0.09089)	(0.04644)**
YEAR 1995	0.11168	0.09332
	(0.08508)	(0.01261)***
YEAR 1999	0.14957	0.18035
	(0.06413)**	(0.01602)***
CONSTANT	10.46593	10.67171
	(0.18503)***	(0.08101)***
Observations	69	546
R-squared	0.53	0.55

Note: Robust standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%.

E. WORKER PREFERENCES FOR RISING CONSUMPTION PROFILES

While section IV.II discussed the possibility that the experience premium in this setting is due to Lazear-type implicit contracts, it also revealed that the end of mandatory retirement may have resulted in steeper wage profiles. In the current job market, characterized by the tenure institution that removes almost all potential threat from the firm reneging on the contract, as well as retirement systems that do not incorporate incentives to retire, Lazear's theory on back-loaded payment systems does not provide a compelling justification for the observed steeper wage profiles. So the analysis turns to theories suggesting that the experience premium may be due to a forced savings mechanism.

The theory claiming that rising wage profiles are due to employee preferences for increased consumption has found support empirically. Neumark (1995) finds that people who receive tax refunds (*i.e.*, have a preference for forced saving) face steeper earning profiles. Frank and Hutchens also find support for this hypothesis. They claim that professions characterized by higher interaction among workers will tend to offer contracts involving steep wage profiles. In order to test this hypothesis, they employ a number of measures of coworker interaction.[64] One of them is related to the degree of worker interaction off the job, another measures the presence of certain contractual features[65], and the third index involves a measure of the likelihood of future separation from the job. They find evidence that jobs with a higher interaction index have sharper

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[64] The data used come from the Department of Labor's 1977 cross-section of the *Quality of Employment Survey*.

[65] Frank and Hutchens argue that demonstration effects cause people to save too little, or similarly, under-consume goods that cannot affect one's status (non-positional goods). They claim that the presence of certain features in the labor contracts (such as firm-provided health and life insurance or paid vacation)

wage-experience profiles, but caution against generalizing the results, due to the imprecision of the worker-interaction proxies used in their analysis.

The Big Ten faculty data are not very suitable to testing whether rising wage profiles are preferred by faculty. The problem here is that there may be very little variation in preferences among people in the same profession, specializing in the same general field, and working in very similar departments and institutions. However, the data allow for a test similar to the one conducted by Frank and Hutchens. Following the same logic, one could ask whether departments in which people cooperate more academically offer steeper wage-experience profiles. The average number of co-authors from the same department for each faculty member can be used as a proxy for faculty cooperation. If faculty in a certain department tend to cooperate more with each other, then the claim could be made that they spend more time with colleagues and have a higher probability of engaging in interpersonal comparisons. The theory suggests that close interaction with people who may consume more makes it more difficult to restrict current consumption. This, in turn, may give rise to collective consumption agreements that involve steep wage profiles as a forced saving mechanism. In this way, close interaction may give rise to preferences for rising wage profiles. Steep wage profiles would resolve the self-control problem by restricting consumption at present, while at the same time satisfying these preferences for steep profiles. Assuming that the department takes these preferences into account when setting wages, steeper wage profiles should be observed in departments where co-authoring within the department is more intensive. The following specifications are offered to test this hypothesis:

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indicate an effort from the firm to mitigate the problem arising by the tendency of workers to under-consume such goods.

$$(8)\quad \text{Log}\,(\overline{\text{SALARY}_{jt}}) = \alpha_1 + \beta_{11}\overline{\text{VOLUME}_{jt}} + \beta_{12}\overline{\text{OTHCITES}_{jt}} + \beta_{13}\overline{\text{EXPER}_{jt}} + \beta_{14}(\overline{\text{EXPER}_{jt}})^2 + \beta_{15}\overline{\text{TOP10}_{jt}} + \beta_{16}\overline{\text{COOP}_{jt}} + \delta\,(\overline{\text{COOP}_{jt}} * \overline{\text{EXPER}_{jt}}) + \lambda_{11}\text{Y95}_t + \lambda_{12}\text{Y99}_t + v_{jt}$$

$$(9)\quad \text{Log}\,(\overline{\text{SALARY}_{jt}}) = \alpha_1 + \beta_{11}\overline{\text{VOLUME}_{jt}} + \beta_{12}\overline{\text{OTHCITES}_{jt}} + \beta_{13}\overline{\text{EXPER}_{jt}} + \beta_{14}(\overline{\text{EXPER}_{jt}})^2 + \beta_{15}\overline{\text{TOP10}_{jt}} + \beta_{16}\overline{\text{PCSAME}_{jt}} + \delta\,(\overline{\text{PCSAME}_{jt}} * \overline{\text{EXPER}_{jt}}) + \lambda_{11}\text{Y95}_t + \lambda_{12}\text{Y99}_t + v_{jt}$$

where:

j = department
t = 92, 95, 99

All the variables in this model represent averages at the department level for each time period. It should be noted that this analysis is more meaningful at the department level, since, according to theory, all workers have to come to an implicit agreement to collectively restrict current consumption. The variable COOP in model (8) is the average number of co-authors from the same department for each faculty member in each period. This measure would serve as a proxy for interaction with co-workers, similar to the indexes used for the same purpose by Frank and Hutchens. However, this measure may be affected by individual preferences for co-authoring in general. Faculty who prefer to co-author more with researchers both within and outside the department, are more likely to have a larger COOP measure. Therefore, a new variable is introduced, PCSAME, which represents the percentage of co-authors from the same department out of all

researchers who have co-authored with a particular faculty member. This variable is derived according to the following formula:

$$PCSAME_{it} = \frac{1}{\eta} \sum_{\tau \leq t} \left(\frac{\sum_{0}^{D} d}{\sum_{1}^{N} n} \right)$$

where:

i = person
t = 92, 95, 99
n = total number of co-authors in an article
d = total number of co-authors in an article from the same department
η = number of articles published during the period $\tau \leq$ t
τ = year when article was published.

So PCSAME is an average of the ratio of within-department to total number of co-authors, over all published articles during the period under consideration. Table 2.16 presents descriptive statistics for the variables used in this model. The estimates of models (8) are tabulated in Table 2.17. The first four columns present OLS estimates, whereas the last three display results from fixed effects estimations. The publication index appears significant and positive in most regressions, as expected. From the negative coefficient of COOP it can be inferred that departments where people cooperate more pay slightly less. The large coefficient should be interpreted carefully because COOP takes very small values. A change of one standard deviation from the mean COOP value (mean = 0.11 and standard deviation = 0.05) according to the estimates is associated with a difference in average salaries of 8.5 percentage points. This could partially be due to the fact that, when holding constant total department research activity, a higher rate of co-authoring indicates less individual research, which is associated with higher rewards than co-authored research. Because COOP only includes co-authorship

within the department, its correlation with publication measures, which are corrected for overall co-authorship, is of little concern.

The variable of central interest in this analysis is the interaction COOP*EXPERIENCE. It appears from the estimates that this variable is significant in most regressions, and the sign suggests a higher return to experience in departments with higher average cooperation among faculty. The estimated magnitude suggests that co-authoring of about one standard deviation above the mean is associated with a return to one additional year of experience of 0.5 percentage points higher (0.05 * 0.1). EXPERIENCE itself appears insignificant, but the effect is most likely due to the very small variation of this variable across departments. It is interesting, however, to notice that departments are able to preserve a certain average academic age of their faculty over time.[66] The second regression includes a categorical variable to control for department for the purpose of conserving degrees of freedom. The third regression adds department dummies, but the results remain similar. In order to control for the possibility of part of the results being driven by differences in the composition of departments with respect to grade level, the fourth regression adds dummies for assistant and associate.

To eliminate the possibility that the results arise partly due to department- and year-specific factors, the last three regressions employ a fixed effects method to estimate model (8). The experience variable does not drop out in these regressions, because it is weakly identified through lateral or new entries and exits of faculty. The average department experience varies very slightly due to these moves, but enough to identify this

[66] In fact, the loss of control over the average age of faculty was one of the arguments for keeping mandatory retirement in academic contracts. Interestingly, this does not appear to be an issue for the nine departments considered here.

variable. The interaction of interest remains significant, and its coefficient does not vary much across specifications.

TABLE 2.16 – SUMMARY OF VARIABLES BY DEPARTMENT

Variable	VOLUME		CITATIONS		EXPERIENCE		TOP 10		COOP		PCSAME	
	Mean (st. dev)	Min. max.	Mean (st. dev)	Min. max.	Mean (st. dev)	Min. max..	Mean (st. dev)	Min. max..	Mean (st. dev)	Min. max..	Mean (st. dev)	Min. max.
University of Illinois	398 (102)	297 502	134 (12)	124 147	20 (3)	17 23	0.58 (0.04)	0.54 0.61	0.10 (0.03)	0.13 0.07	0.14 (0.04)	0.18 0.09
Indiana University	474 (113)	351 574	342 (30)	323 377	20 (1)	19 22	0.36 (0.05)	0.33 0.42	0.05 (0.03)	0.09 0.02	0.08 (0.05)	0.14 0.04
University of Iowa	227 (45)	199 279	172 (11)	160 180	22 (1)	21 23	0.46 (0.08)	0.39 0.55	0.13 (0.03)	0.17 0.09	0.17 (0.04)	0.21 0.11
Michigan State University	546 (76)	465 616	254 (8)	249 263	18 (2)	17 20	0.43 (0.03)	0.40 0.47	0.15 (0.04)	0.20 0.12	0.20 (0.06)	0.25 0.12
The Ohio State University	352 (59)	303 417	206 (14)	197 222	19 (3)	17 22	0.78 (0.03)	0.76 0.81	0.12 (0.08)	0.19 0.04	0.15 (0.08)	0.23 0.03
Purdue University	221 (66)	165 293	156 (2)	155 158	22 (2)	19 24	0.16 (0.03)	0.13 0.19	0.12 (0.03)	0.16 0.09	0.13 (0.03)	0.15 0.10
University of Michigan	575 (89)	474 642	343 (57)	280 391	20 (3)	18 23	0.85 (0.01)	0.84 0.87	0.16 (0.03)	0.20 0.14	0.18 (0.05)	0.23 0.13
University of Minnesota	478 (64)	408 535	385 (97)	281 474	27 (6)	20 32	0.59 (0.05)	0.55 0.64	0.03 (0.02)	0.05 0.02	0.06 (0.03)	0.08 0.02
University of Wisconsin	562 (76)	495 645	184 (54)	125 228	20 (2)	18 22	0.62 (0.01)	0.60 0.63	0.10 (0.04)	0.15 0.07	0.11 (0.04)	0.17 0.07
Overall	426 (146)	165 645	242 (97)	124 474	21 (3)	17 32	0.54 (0.21)	0.13 0.87	0.11 (0.05)	0.20 0.02	0.14 (0.06)	0.25 0.02

Notes:

* The mean values here represent averages over time of means of the variables by department. The minimum and maximum values, therefore, represent the change in the magnitude of the departmental average of the variable over the 1992-1999 period.

TABLE 2.17 – TESTING THE THEORY ON PREFERENCES FOR RISING WAGE PROFILES USING DATA FROM ACADEMIC ECONOMISTS

Dependent Variable: LOG(AVERAGE SALARY BY DEPARTMENT)							
	OLS ESTIMATION				FIXED EFFECTS ESTIMATION		
Model	(1)	(2)	(3)	(4)	(5)	(6)	(7)
PUBLICATION INDEX (IMPACT FACTOR)	0.00092 (0.00031)**	0.00077 (0.00031)**	0.00075 (0.00019)***	0.00008 (0.00067)	–	0.00075 (0.00075)	0.00008 (0.00069)
VOLUME OF RESEARCH	-0.00030 (0.00014)*	-0.00019 (0.00015)	-0.00009 (0.00016)	-0.00021 (0.00016)	–	-0.00009 (0.00017)	-0.00021 (0.00015)
CITATIONS	0.00027 (0.00027)	0.00011 (0.00027)	0.00043 (0.00026)	0.00034 (0.00042)	0.00096 (0.00042)**	0.00043 (0.00047)	0.00034 (0.00041)
EXPERIENCE	-0.02430 (0.03626)	-0.00269 (0.02819)	-0.00063 (0.00796)	-0.00423 (0.01213)	-0.00044 (0.02172)	-0.00063 (0.01653)	-0.00423 (0.01388)
$(\text{EXPERIENCE})^2$	0.00044 (0.00055)	0.00007 (0.00041)	-0.00064 (0.00054)	-0.00074 (0.00103)	0.00010 (0.00081)	-0.00064 (0.00065)	-0.00074 (0.00063)
COOP * EXPERIENCE	0.10119 (0.04165)**	0.09741 (0.04512)*	0.09574 (0.05014)*	0.10206 (0.03670)**	0.06747 (0.05017)	0.09574 (0.04097)**	0.10206 (0.03461)**
COOP	-1.70718 (0.68934)**	-1.79453 (0.77149)**	-1.84342 (0.89913)*	-1.87069 (0.76860)**	-1.40408 (0.96092)	-1.84342 (0.76014)**	-1.87069 (0.63465)**
TOP10	–	–	-4.03156 (0.94195)***	-4.41706 (0.85499)***	–	-4.03156 (1.25353)**	-4.41706 (1.17422)***
ASSISTANT	–	–	–	-0.25437 (0.26335)	–	–	-0.25437 (0.15313)
ASSOCIATE	–	–	–	-0.16513 (0.05820)**	–	–	-0.16513 (0.07660)*

TABLE 2.17 (CONT'D)

YEAR 1995	0.08222 (0.04971)	0.06971 (0.04072)	0.12115 (0.03030)***	0.16197 (0.07455)*	0.04796 (0.05670)	0.12115 (0.04896)**	0.16197 (0.05243)**
YEAR 1999	0.22197 (0.08918)**	0.18211 (0.06603)**	0.31440 (0.09473)**	0.40248 (0.21839)	0.14190 (0.15663)	0.31440 (0.13273)**	0.40248 (0.13924)**
DEPARTMENT	no	yes	no	no	no	no	no
DEPARTMENT DUMMIES	no	no	yes	yes	no	no	no
CONSTANT	11.30830 (0.41225)***	11.06352 (0.34786)***	13.13427 (0.42223)***	13.55418 (0.62592)***	10.93113 (0.27892)***	13.02054 (0.67571)***	13.48111 (0.70764)***
Observations	27	27	27	27	27	27	27
R-squared	0.89	0.91	0.99	0.99	0.97	0.99	0.99

Note: Robust standard errors in parentheses (corrected for heteroskedasticity and autocorrelation among observations from the same department)
* significant at 10%; ** significant at 5%; *** significant at 1%.

Now the analysis focuses on the estimation of model (9) which includes the variable PCSAME measuring co-authorship within the department as a percentage of total co-authorship. This variable intends to correct for personal preferences for co-authoring among researchers. The same types of regressions are run, and the results appear in Table 2.19. The interaction of interest, PCSAME * EXPER, appears even stronger than in the previous estimations. An increase of one standard deviation in the percentage of cooperation within the department is associated with about 0.5 - 0.9 percentage points higher average department salary for each additional year of experience, depending on the specification used. This effect remains statistically significant and varies little in coefficient after department and grade level dummies are introduced. The fixed effects estimations yield slightly coefficients that are slightly smaller, but overall surprisingly similar. Therefore, the data suggest that departments with a higher percentage of within-department co-authorship tend to offer steeper profiles. This finding would support the forced-saving theory of Frank and Hutchens.

TABLE 2.18 – TESTING THE THEORY ON PREFERENCES FOR RISING WAGE PROFILES USING DATA FROM ACADEMIC ECONOMISTS

Dependent Variable: LOG(AVERAGE SALARY BY DEPARTMENT)							
	OLS ESTIMATION				FIXED EFFECTS ESTIMATION		
Model	(1)	(2)	(3)	(4)	(5)	(6)	(7)
PUBLICATION INDEX (IMPACT FACTOR)	0.00104 (0.00025)***	0.00088 (0.00027)**	0.00038 (0.00026)	-0.00005 (0.00049)	–	0.00038 (0.00060)	-0.00005 (0.00060)
VOLUME OF RESEARCH	-0.00033 (0.00015)*	-0.00022 (0.00017)	-0.00028 (0.00018)	-0.00031 (0.00015)*	–	-0.00028 (0.00016)	-0.00031 (0.00015)*
CITATIONS	0.00022 (0.00026)	0.00009 (0.00027)	0.00087 (0.00019)***	0.00081 (0.00023)***	0.00106 (0.00035)**	0.00087 (0.00037)**	0.00081 (0.00035)*
EXPERIENCE	-0.04051 (0.03314)	-0.01533 (0.02857)	-0.00995 (0.00646)	-0.01044 (0.00966)	-0.01170 (0.02017)	-0.00995 (0.01365)	-0.01044 (0.01255)
$(\text{EXPERIENCE})^2$	0.00055 (0.00049)	0.00017 (0.00041)	-0.00031 (0.00038)	-0.00041 (0.00072)	0.00050 (0.00072)	-0.00031 (0.00054)	-0.00041 (0.00057)
COOP * EXPERIENCE	0.14896 (0.03577)***	0.12246 (0.04305)**	0.13234 (0.03808)***	0.12138 (0.03062)***	0.08029 (0.04049)*	0.13234 (0.03847)***	0.12138 (0.03618)**
COOP	-2.62898 (0.54534)***	-2.23281 (0.69929)**	-2.44228 (0.63145)***	-2.23450 (0.54716)***	-1.66084 (0.74495)**	-2.44228 (0.67299)***	-2.23450 (0.63147)**
TOP10	–	–	-4.15727 (0.95370)***	-4.16396 (0.71673)***	–	-4.15727 (1.04873)***	-4.16396 (1.03553)***
ASSISTANT	–	–	–	-0.15666 (0.19704)	–	–	-0.15666 (0.11511)
ASSOCIATE	–	–	–	-0.11579 (0.05886)*	–	–	-0.11579 (0.06771)

TABLE 2.18 (CONT'D)

YEAR 1995	0.08124 (0.05186)	0.06756 (0.04162)	0.09663 (0.03498)**	0.12468 (0.05175)**	0.01750 (0.05083)	0.09663 (0.04087)**	0.12468 (0.04647)**
YEAR 1999	0.23794 (0.09326)**	0.19439 (0.07256)**	0.27612 (0.07904)***	0.33571 (0.14847)*	0.07571 (0.13719)	0.27612 (0.10886)**	0.33571 (0.12226)**
DEPARTMENT	no	yes	no	no	no	no	no
DEPARTMENT DUMMIES	no	no	yes	yes	no	no	no
CONSTANT	11.56689 (0.36988)***	11.25710 (0.36138)***	13.27389 (0.48113)***	13.40944 (0.40575)***	10.96860 (0.24470)***	13.12715 (0.57085)***	13.29202 (0.60860)***
Observations	27	27	27	27	27	27	27
R-squared	0.90	0.92	0.99	0.99	0.98	0.99	0.99

Note: Robust standard errors in parentheses (corrected for heteroskedasticity and autocorrelation among observations from the same department)
* significant at 10%; ** significant at 5%; *** significant at 1%.

Another testable implication involves comparisons across occupations. In Section III.III, it was argued that people working in academia may have a higher preference for steep wage profiles compared to people in other occupations. The reason for this would be because the nature of academic work involves a higher degree of cooperation and interaction with colleagues than most other jobs. The model below is used to obtain some indication of the relative steepness of wages between academia and other occupations requiring a similar degree of human capital:

$$(10)\quad \text{Log (WEEKLY EARNINGS)}_{it} = \alpha_1 + \beta_{11}\text{AGE} + \beta_{12}\text{AGE}^2 + \beta_{13}\text{PROFESSOR}_{it} + \beta_{14}(\text{PROFESSOR}_{it} * \text{AGE}_{it}) + \beta_{15}\text{UNION}_{it} + \beta_{16}\text{MARRIED}_{it} + \beta_{17}\text{FEMALE}_i + \beta_{18}\text{BLACK}_i + \beta_{19}\text{OTHER}_i + \beta_{1,10}\text{STATEMP}_{it} + \beta_{1,11}\text{LOCEMP}_{it} + \beta_{1,12}\text{FEDEMP}_{ii} + \lambda_t\text{YEAR}_t + \delta_i\,\text{STATE} + \theta_i\text{OCCUPATION} + u_{it}$$

Data from the Outgoing Rotations of the Current Population Survey during 1992-2002 are obtained, and only people who have a Ph.D. degree are kept in the sample.[67] Those who reported teaching in a postsecondary institution as their occupation are assumed to be professors (PROFESSOR). Out of 17,747 people in the sample, 4,761 fall in this category. If the assumption is made that people in academia interact more with their colleagues than people in other professions, and if increased interaction with co-workers is associated with increased preferences for rising wage profiles (as claimed by Frank and Hutchens), one would expect a positive coefficient on the interaction term PROFESSOR

[67] The estimation is carried out using only Ph.D.-recipients to simplify the analysis. In particular, it could be argued that people who pursue doctoral studies may be different in their preferences for rising wage profiles than others. Also, by analyzing people of similar ability (by holding constant the degree of education) the bias arising from not including performance data in the regressions is mitigated to a large extent. The choice of 1992 as the cutoff year is due to the unavailability of detailed information on graduate education before this point.

* EXPERIENCE in the above specification. Another reason why this could be the case, in addition to the argument about the higher degree of cooperation in academia, would be that contracts in academia are more likely to be long term. The length of the contract is a key feature of the model developed by Frank and Hutchens, because that makes collective agreements on restricting current consumption possible and worthwhile. Finally, it could be argued that academic departments have more flexibility in setting salaries in response to employee preferences rather than, for example, government organizations, research institutes, or other business organizations that tend to be larger, more hierarchical, and employ a wide range of people from different backgrounds in addition to Ph.D. recipients. While it is impossible to control for performance in this analysis, some indication of the direction of the effect can still be obtained. Table 2.19 presents the estimates. Also, since experience on the job cannot be measured, age is used instead. The correlation between age and experience is bound to be quite large, especially considering that the amount of schooling for people in this sample is about the same. The partial effect of one additional year on weekly earnings is estimated to be about three times the magnitude of the one estimated for Big Ten faculty (about 9.6% vs. 2.3 – 4.5% as presented in Table 2.4). The main reason for this could be that the previous estimations employed a number of detailed performance variables. According to the first two regressions, it appears that Ph.D. recipients employed in academia make on average 42% less than other people. This difference is reduced by half when occupation dummies are added to the model. According to the third regression estimates, academic Ph.D.'s appear to earn on average 20% less than people with a Ph.D. in other occupations. However, the most interesting finding emerging from these regressions is the positive and

significant coefficient on the interaction term PROFESSOR * AGE, suggesting that Ph.D.-recipients teaching in postsecondary institutions face steeper wage profiles. In particular, the estimated effect suggests a return of about 0.5-0.8 percentage points higher for this group, depending on the specification.

Other variables of interest suggest that federal employment is associated with a premium ranging from 4-8%, whereas working for a local government institution is associated with lower earnings of about 4-6%. Married people earn on average between 6-10% more than others. It appears that female Ph.D. recipients earn about 17-20% less than males. Blacks seem to earn about 10-14% less, and people of other races about 4-8% less than whites. While these results cannot amount to conclusive evidence refuting or supporting the theory of preferences for rising wage profiles, they provide some indication that the argument has relevance.

TABLE 2.19 – TESTING THE THEORY ON PREFERENCES FOR RISING WAGE PROFILES USING CURRENT POPULATION SURVEY DATA

Dependent Variable: LOG(WEEKLY EARNINGS)			
Model	(1)	(2)	(3)
AGE	0.09457	0.09507	0.09390
	(0.00428)***	(0.00428)***	(0.00402)***
$(AGE)^2$	-0.00099	-0.00100	-0.00095
	(0.00005)***	(0.00005)***	(0.00004)***
PROFESSOR	-0.41767	-0.39071	-0.20177
	(0.05340)***	(0.05344)***	(0.11753)*
PROFESSOR * AGE	0.00799	0.00751	0.00528
	(0.00115)***	(0.00115)***	(0.00114)***
UNION	0.00284	0.00766	-0.00487
	(0.02500)	(0.02465)	(0.02413)
MARRIED	0.09614	0.10106	0.06400
	(0.01185)***	(0.01184)***	(0.01069)***
FEMALE	-0.19601	-0.20028	-0.16576
	(0.01119)***	(0.01113)***	(0.01063)***
BLACK	-0.10633	-0.13571	-0.09126
	(0.02531)***	(0.02554)***	(0.02319)***
OTHER (RACE)	-0.03859	-0.06264	-0.07863
	(0.01498)**	(0.01514)***	(0.01461)***
STATE GOVERNMENT EMPLOYEE	-0.01142	0.02485	-0.00483
	(0.01166)	(0.01207)**	(0.01178)
LOCAL GOVERNMENT EMPLOYEE	-0.05843	-0.04876	-0.04408
	(0.01786)***	(0.01775)***	(0.01903)**
FEDERAL EMPLOYEE	0.07636	0.05283	0.03868
	(0.02007)***	(0.02181)**	(0.02116)*
YEAR DUMMIES	yes	yes	yes
OCCUPATION DUMMIES	no	no	yes
STATE DUMMIES	no	yes	yes
CONSTANT	4.64460	4.43031	4.63104
	(0.09452)***	(0.11682)***	(0.20600)***
Observations	16,909	16,909	16,909
R-squared	0.10	0.12	0.26

Note: Robust standard errors in parentheses;
* significant at 10%; ** significant at 5%; *** significant at 1%.

The experience premium for professors could not be reproduced for most other occupations in this dataset, such as physicians, lawyers, social scientists, and engineers. However, steep wage profiles emerged just as strong in the case of college and university administrators[68]. The choice of these occupation groups was conditioned by the data. Among the Ph.D.-recipients in the Outgoing Rotation files for 1992-2002, these occupations had better representation than others.

Table 2.20 presents these estimates. All regressions include state, year, and occupation dummies. Interestingly, the premium associated with AGE is about the same for all occupations in Table 2.20, and it ranges between 9.4-9.5% per additional year. OCCUPATION in this table refers to one of the particular occupations in each column. This dummy is insignificant for the case of physicians, lawyers, social scientists, and engineers. However, for the case of college and university administrators and natural scientists, this dummy suggests earnings of 32% and 38% lower, respectively. The interaction OCCUPATION * AGE, however, is positive for both of these occupation groups. Its magnitude suggests that administrators in postsecondary institutions are likely to face wage profiles with slopes of about 0.7 percentage points higher then the rest of the group. For natural scientists this estimate is slightly less, suggesting slopes of about 0.5 percentage points higher. The statistically smaller intercepts paired with the larger slopes suggest much steeper wage profiles for college and university administrators and for natural scientists, compared to other Ph.D. holders in the sample.

The case of education administrators is similar to that of post-secondary faculty. In fact, most academic deans and chairpersons begin their careers as faculty in their

[68] Elementary and secondary school administrators were also considered, but their age profiles were statistically no different from others.

respective institutions and often continue to hold a teaching position. Therefore, the same claim could be made about their wage profiles as the one that was made in the case of postsecondary faculty earlier on. The case of natural scientists is somewhat similar to the case of faculty, in the sense that a good portion of this group devotes time to academic research, which involves the same type of scholarly cooperation discussed earlier. In addition, large numbers of natural scientists are also involved in field research, and work under conditions similar to those of airline pilots described in Frank and Hutchens. Physicists and astronomers, whose research involves unique and expensive laboratories and equipment, often work in large teams. Geoscientists, such as geologists, geophysicists, and oceanographers, spend a lot of time performing field work, which involves long hours in remote locations and extensive association with co-workers. Atmospheric scientists, likewise, often work in weather stations that operate around the clock 7 days a week. Therefore, similar claims could be made about the degree of co-worker interaction for this occupation group as what was made in the study by Frank and Hutchens.

TABLE 2.20 – TESTING THE THEORY ON PREFERENCES FOR RISING WAGE PROFILES USING CURRENT POPULATION SURVEY DATA

Dependent Variable: LOG(WEEKLY EARNINGS)						
	PHYSICIANS (N=1,961)	LAWYERS (N=1,154)	SOCIAL SCIENTISTS (N=593)	ENGINEERS (N=727)	COLLEGE/ UNIVERSITY ADMINSTRATORS (N=612)	NATURAL SCIENTISTS (N=1,649)
Model	(1)	(2)	(3)	(4)	(5)	(6)
AGE	0.09522	0.09504	0.09502	0.09531	0.09547	0.09372
	(0.00404)***	(0.00403)***	(0.00399)***	(0.00399)***	(0.00401)***	(0.00406)***
$(AGE)^2$	-0.00095	-0.00095	-0.00095	-0.00095	-0.00095	-0.00094
	(0.00004)***	(0.00004)***	(0.00004)***	(0.00004)***	(0.00004)***	(0.00004)***
OCCUPATION	0.15098	-0.03272	-0.43201	0.08526	-0.31980	-0.38044
	(0.26606)	(0.18540)	(0.41291)	(0.20080)	(0.15780)**	(0.20388)*
OCCUPATION * AGE	-0.00053	0.00018	-0.00455	-0.00166	0.00691	0.00478
	(0.00178)	(0.00187)	(0.00363)	(0.00165)	(0.00308)**	(0.00176)***
UNION	-0.00774	-0.00785	-0.00801	-0.00792	-0.00855	-0.00806
	(0.02408)	(0.02410)	(0.02409)	(0.02410)	(0.02409)	(0.02409)
MARRIED	0.06254	0.06243	0.06228	0.06258	0.06225	0.06209
	(0.01052)***	(0.01052)***	(0.01051)***	(0.01052)***	(0.01051)***	(0.01051)***
FEMALE	-0.16764	-0.16758	-0.16737	-0.16743	-0.16737	-0.16804
	(0.01049)***	(0.01049)***	(0.01051)***	(0.01049)***	(0.01049)***	(0.01049)***
BLACK	-0.09419	-0.09422	-0.09420	-0.09427	-0.09448	-0.09422
	(0.02304)***	(0.02304)***	(0.02304)***	(0.02303)***	(0.02304)***	(0.02303)***
OTHER (RACE)	-0.08192	-0.08205	-0.08179	-0.08204	-0.08195	-0.08106
	(0.01459)***	(0.01461)***	(0.01459)***	(0.01460)***	(0.01460)***	(0.01457)***

TABLE 2.20 (CONT'D)

STATE GOVERNMENT EMPLOYEE	-0.00283	-0.00277	-0.00267	-0.00280	-0.00340	-0.00186
	(0.01167)	(0.01167)	(0.01166)	(0.01166)	(0.01168)	(0.01168)
LOCAL GOVERNMENT EMPLOYEE	-0.04511	-0.04500	-0.04473	-0.04516	-0.04009	-0.04444
	(0.01893)**	(0.01892)**	(0.01892)**	(0.01892)**	(0.01998)**	(0.01892)**
FEDERAL EMPLOYEE	0.03672	0.03668	0.03628	0.03673	0.03727	0.03572
	(0.02110)*	(0.02110)*	(0.02110)*	(0.02109)*	(0.02111)*	(0.02106)*
YEAR DUMMIES	yes	yes	yes	yes	yes	
OCCUPATION DUMMIES	yes	yes	yes	yes	yes	
STATE DUMMIES	yes	yes	yes	yes	yes	
CONSTANT	4.56151	4.56756	4.56472	4.55867	4.56094	4.61109
	(0.20296)***	(0.20327)***	(0.20221)***	(0.20258)***	(0.20256)***	(0.20387)***
Observations	16,905	16,905	16,905	16,905	16,905	16905
R-squared	0.27	0.27	0.27	0.27	0.27	0.27

Note: Robust standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

While these results do not provide indisputable proof of the forced saving theory on steep wage profiles, they do provide indications that its main implications may be true. In the context of this study, this theory better explains the observed steeper slopes of wage paths after the abolition of mandatory retirement. The length of employment is of central importance for this theory. At the same time, the literature shows that this particular aspect of academic employment has undergone significant change after the elimination of mandatory retirement (Ashenfelter and Card, 2002). Longer employment, paired with already extensive co-worker cooperation, may be responsible for the steeper wage profiles observed in the profession. Implicitly entering collective agreements to restrict current spending becomes even more attractive and provides bigger payoffs in the framework of the theory on forced saving mechanisms when employment length increases. For the case of academia, unlike most other professions, the institution of tenure serves the purpose of extending the employment period indefinitely. This may be one reason why the findings are more pronounced for this profession. At the same time, the increased length of employment with the same firm implies that interpersonal comparisons of relative consumption are carried out for even longer periods of time, and this may further increase the potential for workers to exhibit preferences for steep wage profiles.

V. CONCLUSION

This chapter tests the implications of competing theories explaining upward-sloping wage-experience profiles. The observed robust effect of experience in the academic labor market, the length of academic contracts, and the availability of information on productivity make the academic market a prime setting for investigating and comparing implicit contract theories.

Evidence does provide some support for Lazear's theory of delayed payment schemes. Faculty that entered academic contracts in institutions after they discontinued the practice of mandatory retirement by either state or federal law, appear to be on steeper wage paths, contrary to what this theory would suggest. The wage profiles of faculty hired before the change in legislation do appear somewhat flatter, but this finding is not robust to the particular specifications used.

The adverse-selection theory is not supported by the data. This theory implies that junior faculty, because their ability is still being assessed, are paid a wage below the value of marginal product. The data show that the return to experience for junior faculty largely exceeds the return to experience for tenured faculty, all else equal.

Finally, the empirical analysis grants credibility to the theory on employee preferences for rising wage profiles. In particular, it appears that departments where faculty cooperates more (with each other) are characterized by steeper wage profiles. Using data from the Outgoing Rotations of the Current Population Survey, the study brings evidence that Ph.D. recipients employed in academia face steeper wage profiles than people of similar education in different occupations. This finding may explain why wage paths have become steeper after the abolition of mandatory retirement for tenured

faculty. The change in legislation has resulted in a significant increase in employment length and, under these circumstances, it is more likely to observe preferences for rising wage profiles. Increased length of employment increases the time during which people engage in comparisons of relative consumption, and at the same time makes implicit collective agreements to restrict present consumption more attractive.

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CHAPTER THREE

DO FISCAL POLICIES AFFECT THE MIGRATION OF HUMAN CAPITAL?

I. INTRODUCTION

Early studies on human capital have determined that investment decisions depend on the expected rate of return, much like investment in physical capital (Becker, 1962 and Heckman, 1976). In a world with a public sector, income taxes, by reducing the expected rate of return on human capital, would deter investment in human capital. A number of studies investigated this proposition (Heckman, 1976; Lucas, 1990; Davies and Whalley, 1993, etc.) and arrived at conflicting conclusions. While the central discussion in this strand of literature concerns human-capital investment over time in an isolated environment (state or country), the presence of many similar jurisdictions, which vary widely in their choice of taxes, makes the discussion more interesting and at the same time increases its importance and relevance.

The migration literature has brought extensive evidence on the effects that the public sector has on individual migration decisions. The literature, however, has focused mostly on the impact of welfare spending on the migration of the poor (see, *e.g.,* Brehm and Saving, 1964; Gallaway, Gilbert, and Smith, 1967; DeJong and Donnelly, 1973; Pack, 1973; Sommers and Suits, 1973; Cebula, 1974). In light of the extensive discussion on the impact of taxes on human capital, the other end of the spectrum appears just as important. The impact of fiscal policies on the decision to migrate for the better educated will be the center of the present discussion. By overlooking human-capital

migration and focusing on welfare migration, one runs the risk of ignoring the interdependence of spending and taxation at the local government level. Areas engaging in high welfare spending have been shown to attract in-migration from less-skilled labor, whose increased share in the population of the area is likely to exert pressure on the local governments to increase the share of future welfare spending. When increased spending on welfare is financed through local taxes, this creates incentives for the higher-skilled (and higher-income) individuals to out-migrate. The reason for this is because high-skilled labor earns higher wages and possesses more human capital, both of which are negatively affected by higher income taxes. Such out-migration is bound to reduce the local tax base, and the combined effect of in-migration from less skilled labor, and out-migration of high-skilled labor over time will result in areas that vary widely in their economic growth rates.

Using information from the decennial *Census of Population* for 1990, this study finds that lower state income taxes are associated with higher migration rates into these states from high-skilled labor. The empirical findings confirm the results of previous studies on the positive effect of higher per-capita spending on education on in-migration. However, such spending appears to be less attractive to the higher-educated individuals in the sample, since higher education spending shares are found to be associated with lower migration rates from this group.

Section 2 offers a broad review of the literature on human capital taxation and migration; Section 3 will present the model specification and discuss the choice of the econometric method used; Section 4 will present and discuss the findings; and Section 5 will offer a few concluding remarks.

II. Review of Literature

Despite a number of conflicting views on the direction of the effect of income taxes on human capital, the literature, for the most part, agrees that such effects are negative. Heckman (1976) argues for a positive influence of income taxation on human capital, through the reduced rate of return on physical capital, which makes human capital a better investment. Lucas (1990) believes that the effect of taxes on human capital has the reverse sign, because of the reduction in working hours (from the reduction in net wages) and hence a decline in the utilization of human capital, which brings returns in human-capital investment down. Ballard (2000) shows that Lucas may have overestimated the effect of taxation on labor supply by misspecifying the labor-supply function. Davies and Whalley (1993) point out that most results of the effect of taxation on human capital come from partial equilibrium analyses. Their general-equilibrium framework allows them to take into account the (potential) substitutability of human with physical capital and conclude that the effect of income taxes on human-capital investment is very small. Dupor, Lochner, Taber, and Wittekind (1996) point out that there are a number of factors involved in the decision to invest in human capital other than income taxes, and they do not all work in the same direction. In particular, they investigate the effects of two features of the current tax code, namely, its progressivity and the nondeductibility of inputs into human-capital production. Using 1970 Census data, they find, for the most part, a negative impact of taxes on human-capital accumulation.

Recently, human-capital investment is being studied against a tax-competition background. Poutvaara (2000) and Justman and Thisse (1997, 2000) have laid the

foundation for a discussion of human-capital investment, local public funding of education, and population mobility, in a federation where jurisdictions engage in tax competition. These studies represent a natural extension to the literature on capital tax competition, and provide implications that are the same for human capital as for physical capital. Zodrow and Mieszkowski (1984) showed that capital taxation in a federation with tax competition would violate the Samuelson Rule for public good provision in the direction of underprovision. The driving forces behind their results are the regional fiscal externalities that independent jurisdictions fail to take into account when determining their taxes and spending levels. An increase in the tax rate of any jurisdiction causes a capital outflow, and hence an inflow for the other jurisdictions. This positive externality is ignored by the local governments, so the tax rate they choose is "too low", and as a result, public goods are underprovided[69].

Parallel to these studies runs the literature on the fiscal migration effects of multi-government systems. Flatters, Henderson, and Mieszkowski (1974), Buchanan and Goetz (1972), and Stiglitz (1977, 1983) all argue that, in general, the migration of population among a limited number of identical jurisdictions results in an inefficient equilibrium, as migrants do not take into account the fiscal externalities (higher tax burden for people at the home jurisdiction to finance a fixed amount of public expenditures, and lower tax burden at the host jurisdiction for the same reasons).

Poutvaara, and Justman and Thisse seem to have combined these strands of literature into their analysis of human-capital investment, mobility, and local public goods provision. Just like Zodrow and Mieszkowski, Justman and Thisse conclude that

[69] This result does not hold, however, in the presence of a labor tax. It also does not hold if the assumption of identical jurisdictions is relaxed.

high tax rates on labor in a state will induce a migration of skilled labor among identical jurisdictions, and ultimately will result in underprovision of public education. Poutvaara models investment in human capital as a risky investment, and acknowledges the possible erosion in regional tax bases from migration. The option of migrating, however, also serves as insurance against region-specific shocks in the model, thus encouraging investment in human capital.

The results of these studies depend in large part on the question they have tried to answer, as well as the assumptions made. All of them explain some aspect of the human-capital investment decision in a world with taxes, but because of the nature of human capital, results can be anything but definite. Ultimately this remains an empirical question, and only applied work can provide an answer with respect to the overall impact of fiscal policies on human-capital migration.

Empirical studies have evidenced a strong impact of the public sector on migration. A number of studies have found that, while high levels of welfare spending attract in-migration from non-whites and the poor, this same type of spending inhibits in-movement from whites and/or high-income individuals, whereas education spending has the opposite effect on the two groups (see, *e.g,*. Pack, 1973). Pack also finds that high property taxes are associated with lower migration rates of whites. A number of empirical studies on the impact of fiscal policies on migration make use of Canadian data. Day (1992) concludes that provincial government spending in Canada has a strong impact on migration, and the sign of the effect depends on the type of spending. In particular, she finds that higher per-capita spending on education increases the propensity of in-migration, as does spending on health, whereas spending on social programs has the

opposite effect. Shaw (1986) finds that federal equalization grants in Canada reduce the propensity of migration for the purpose of taking advantage of disparities in economic opportunities across regions. Liu (1977) includes an average state tax rate variable in his estimations of migration probability, but finds no effect of this variable on migration.[70] Part of the reason why some studies have found inconclusive evidence with respect to the effect of taxes on migration, is because the measurements of the tax burden have been very crude and involved large measurement errors. Much of the effort in this study, therefore, has concentrated on obtaining reliable estimates of average tax rates that are education-specific. This ensures that both the effect of tax rates on migration, and the effect of tax rates on the migration of high-skilled labor are assessed during the course of the same analysis. The next section presents the model specification and provides details on the variables used in the empirical analysis.

III. METHODOLOGY AND DATA

A. METHODOLOGY

This study takes the disequilibrium approach in the analysis of migration. In particular, migration here is regarded as a way of taking advantage of gains from differences in economic opportunities that exist among different states.[71] The assumption will be that the migrant selects the destination state based on the economic opportunities offered by all the possible choices. In particular, the fact that a mover in the U.S. has the

[70] He defines the average tax rate as the ratio of total tax revenues to total personal income.

[71] Another approach is the equilibrium hypothesis that regards differences in economic opportunities in different areas as representing compensating differentials. Given this hypothesis, migration cannot result in utility gains for any individual agent.

choice of relocating to fifty other states will explicitly enter the model specification. The dependent variable will be specified as the ratio of people moving from state *k* to state *l* to the total number of people remaining in state *k*. This ratio will be education- and marital-status-specific, and will be calculated for each origin-destination state pair.

Because income-tax liability depends on the income of the individuals, effort is spent to obtain closer measures of this variable than what can be achieved by simply using statutory tax rates.[72] Ideally, one would need to obtain the state tax returns of individuals to determine their tax burden. Because such effort is outside of the scope and the possibilities of this study, the next-best alternative is to use secondary data that draw their information from filed tax returns. To obtain estimates of the sales-tax burden that are comparable in nature to the estimated income taxes used here (*i.e.*, vary with income), data on expenditures of individuals from various education groups were obtained and a rough measure of their average sales-tax rate was estimated. The estimated sales-tax figures account for the fact that some states have exemptions of food items. Section III.II will describe the process of obtaining the average tax variables in more detail.

The empirical analysis will employ education-specific average income tax rates (and, for the income tax, these measures will also be marital-status-specific). Because of the construction of the tax variables, the impact of the tax-rate differential between origin and destination state will be a monotonic function of education level. If taxes increase with education (as in the case of income tax), it will be assumed that the impact of the tax rate on migration will be increasing on the amount of education. Conversely, if the tax burden goes down with education (as in the case of sales tax), a higher impact of the tax

[72] Even in that case, an educated guess about the income bracket of the person needs to be made, since some states have progressive income taxes, not just flat rates.

rate on migration will be interpreted to be decreasing in the amount of education. The downside of this method is that the two effects, namely the effect of the state tax differential and the effect of the state tax differential on the better educated, cannot be separated. As a result, it will not be possible to obtain a magnitude for the difference of the impact of taxes on the various education groups. Given the design of existing tax codes, it is doubtful whether a method can be designed to separate these effects. The main problem stems from the fact that tax codes are designed around income, and income is determined by human capital.

The literature suggests that local labor markets have an important effect on migration (see, *e.g.*, Topel, 1986). As a result, the specification will include labor-market variables, such as employment rates, in origin and destination states. The employment rate in the destination state will serve as a proxy for the probability of getting a job in the new area.[73] Wage differentials between the origin and destination states will also be part of the specification. There are two main reasons for including wages in the specification. The first is related to the argument that local wage differences drive migration from lower- to higher-earning areas. The second reason concerns the argument that local labor-market shocks may not simply be reflected in unemployment rates, but also in the wage levels. To construct the wage difference variable, the logarithm of average earnings of comparable individuals (same education and marital status) who remain in the origin state will be subtracted from the logarithm of earnings of similar individuals in the destination state. The underlying assumption will be that the average wage rate of similar individuals in the destination state proxies for the wage rate that the particular individual

[73] The underlying assumption here is that a portion of the migrants may not move because of job transfers, but instead they move to find a job.

will receive upon relocation. Another assumption behind such comparisons of labor-market variables will be that people who do not migrate serve as an appropriate reference group.[74]

The migration literature has shown that family considerations are important in the decision to migrate. Mincer's work (1978) suggests that husband-wife families are less likely to migrate, and that women are usually "tied-movers", *i.e.*, persons whose migration outcomes are dominated by those of the spouse. To abstract from these considerations, females are dropped from the sample, and a marital-status variable is included in the specification. To remove all identifiable tied-movers, only heads of household are kept in the sample. Also, because marital status has a substantial effect on income-tax liability, all average tax rates calculated are marital-status-specific.

Population size is an important element of migration models. The central claim of "gravity" migration models is that migration is directly related to the respective population sizes of the origin and destination states (see Greenwood, 1997 for a discussion of these models, pp. 663-666). Larger areas will offer more reasons to move there, whereas smaller areas will offer fewer reasons to stay. There is another purpose for considering population size in a migration regression. Schultz (1982) argues that larger areas will be characterized by a larger proportion of within-area moves, rather than moves outside the boundaries of the area (because of their size). Large regions, therefore, will appear to be characterized by more non-migration, thus causing population and/or land in the origin state to be spuriously correlated with non-migration. In order to correct for this spurious relationship, the frequency of non-migration will be explicitly

[74] Morrison (1977) argues that this may not be the case, since migrants may have a broader set of opportunities and information than non-migrants.

incorporated into the dependent variable characterizing migration rates. The analysis will also include population sizes of the destination and origin states, as well as their difference (all in logarithmic form) as a control variable.

One shortcoming of the analysis is that it does not control for the reasons why people in the sample decide to migrate. Results could be very different, and potentially stronger, if, for example, people who move for family-related reasons were excluded. A survey by Lansing and Mueller (1967) shows that about 24% of the moves in their sample involved family reasons (marriage, divorce, separation, being closer to family members, health considerations, etc.). This problem may be mitigated by the fact that the empirical analysis here aggregates the number of moves to specific locations from any given state. It is unlikely that a large proportion of people move from state A to state B for family reasons.

The analysis also does not distinguish between repeat movers (people who have migrated multiple times), return movers (people who return to the state from which they originally migrated), and first-time movers (people who move from the birth state for the first time). Kau and Sirmans (1976) argue that such classification of migrants reduces estimation bias by canceling out individual preferences to migrate.[75] According to Kennan and Walker (2003), data from the National Longitudinal Survey of Youth (NLSY) for the period of 1979-1992 show that between 35 - 50% of all moves are repeat moves and between 13-24% of moves are return moves, (with the percentages going down with education in both cases). The decennial Census data used for this analysis, however, does not provide such details on the type of move. The above estimates suggest

[75] Their results with respect to the impact of local welfare spending on each of the three groups are qualitatively the same as the results of other studies that do not classify migrants this way.

that this problem may be substantial, especially after noticing that the breakdown of movers into each category depends on the education level.

B. THE DATA

This study employs data from the decennial *Census of Population* for 1990 provided by the Integrated Public Use Microdata Series (IPUMS). The 1% unweighted public-use sample is used. The sample was restricted to males of ages 18-60 who were heads of household. The reason for restricting the sample to males was to exclude issues arising from the fact that a large portion of females are tied-migrants, or considerations of welfare-induced migration from female household heads. Migrants were identified as people who reported to have lived in a different state in 1985. The Census data do not give any further details on the year when migration took place. This is clearly a shortcoming of the analysis, since people may have migrated multiple times within the period in question (in which case the state of residence in 1985 may not be the state from which they migrated last). However, the assumption is made that the proportion of multiple movers during such a short period is negligible. The sample was divided into eight groups by marital status and level of education (two marital status categories: married or not, and four education categories: less than high school education, high school or some college, college education, and graduate education). For each group, the proportion of people migrating from the origin state to each of the fifty remaining states was calculated to obtain the variable of interest, MPRAT. This variable is obtained as the ratio of migrants (M) to a particular state to the total population (P) remaining in the

state. This rate is calculated separately for each group and origin-destination state pair.[76] Of course, this variable takes a large number of zero values for origin-destination state cells where there was no migration from the particular marital status – education group during the period of time in question.

Data on state income-tax liabilities were estimated using *TAXSIM*. This term refers to a FORTRAN program from the National Bureau of Economic Research (NBER) that provides estimates of individual tax liabilities under U.S. Federal and State income tax laws. TAXSIM draws upon a database of actual tax returns (around 80,000 returns prepared for public use by the Statistics of Income Division of the Internal Revenue Service since 1973 and for each year afterwards). To calculate state tax liabilities, the program uses tax forms issued by the individual states each year. The state income tax liability is calculated based on sixteen characteristics of the taxpayer: marital status, number of children, age exemptions (number of taxpayers over 65 years of age), wage and salary income of taxpayer and spouse, dividend income, property income including interest, alimony, income from non-farm self-employment, fellowships, etc., pensions, gross Social Security income, non-taxable transfer income, such as welfare, municipal bond interest, child support, rent paid (used for state property-tax rebates), property taxes paid, itemized deductions, child care expenses, and unemployment compensation. Since the decennial Census data do not provide such detailed information on income, the March supplement of the Current Population Survey (CPS) was used instead. Income information on groups of individuals by marital status and education was extracted from

[76] The institutional population (most importantly the military) is excluded from all the population and migration variables.

the March CPS and was later fed into the TAXSIM program.[77] The choice of years to be used from the March CPS was limited by the availability of income data. A large number of income variables in this survey became available only after 1988. Data from the 1988 and 1989 March supplements on male household heads between the ages of 18-60 were extracted and merged (income data from 1989 were deflated to represent 1988 dollars). The sample was then divided into eight groups based on marital status and education the same way as the Census data. Income and other information on each individual were then fed into the TAXSIM program to obtain average state income tax rates as of 1988[78] for each individual, as well as group averages. Note that this procedure incorporates measures of education and income (which proxy for human capital) into the estimated state tax liabilities.

In the absence of a TAXSIM- type program for sales taxes, a similar but less accurate method was used. Statutory sales tax rates for 1988 were obtained from the staff reports of the Advisory Commission on Intergovernmental Relations (ACIR). Using a combination of this source and an article by Feenberg and Rosen (1986), it was determined whether particular states had sales-tax exemptions for food items. Data from the *Consumer Expenditure Survey* were used to calculate an average state sales-tax rate that varies by education group following the formula:

$$\frac{\$FOOD \times \%FOOD\ \ TAX + \$EXPENDITURE \times \%SALES\ \ TAX}{TOTAL\ \ INCOME}$$

[77] Not all information required by TAXSIM was found in the March CPS. In particular, information on itemized deductions, rent and property taxes paid, and child care expenses were not available.

[78] The average tax rate is calculated as the total tax divided by the total income of the individual (multiplied by 100).

where the food tax rate is either zero or the sales-tax rate. EXPENDITURE refers to total consumer expenditures excluding food.[79] This calculation was performed for each education category separately, thus resulting in an average sales-tax rate that is similar in nature to the income-tax rate calculated by TAXSIM (*i.e.,* that varies with income and education.)

Table 3.1 contains summary statistics of the key variables used in the empirical analysis. One striking feature emerging from the table is the fact that M_{kl}, representing the number of male household heads that migrate from state *k* to *l*, is zero for a large number of origin-destination state pairs. The mean of this variable is provided both with the zero values, and without. It appears that the largest average number of moves occurs in the married-high school category. It is also interesting to note that this variable obtains a maximum for the NY-FL, NY-NJ, or NY-CA combinations. The TAXSIM-estimated average income-tax rate ranges from 0 to 8.45%, whereas the average sales-tax rate ranges from 0 – 6.67%. Table 3.2 presents the estimated sales tax and income tax by education group. Note that the estimated income tax is progressive, whereas the estimated sales tax appears regressive. Note that the number of observations for each education-marital status category should be $51^2 = 2{,}601$, and 2550 when same-state pairs are excluded (the regression analysis does so). The total number of observations in the sample is 20,808 (8 categories * 51^2 state-pair combinations).

[79] Estimations were tried with total expenditure excluding housing expenses, but the results did not change substantially.

TABLE 3.1 – SUMMARY OF VARIABLES

(i, j)	Variable Name	Variable Description	Mean	St. Dev.	Min	Max	N
Married; no high school (i=1, j=1)	M_{kl}	number of people moving from state k to state l (for k l)	1.31	3.83	0	90	2,550
		$M_{kl} > 0$	3.76	5.73	(1,661)	(NY to FL)	889
	P_k	number of people in state k (for i=1, j=1)	1,172	1,362	78	7,210	2,601
	$MPRAT_{kl}$	$=M_{kl}/P_k$ (for k l)	0.0015	0.0044	0	0.1011	2,550
	ATR_k	average state income tax rate in origin (k) state	2.66	1.71	0	7.07	2,601
	$DIFTAX_{kl}$	$=ATR_l - ATR_k$	0	2.42	-7.07	7.07	2,601
	W_k	average income from wages in origin (j) state (for i=1, j=1)	17,421	3,252	11,442	24,664	2,601
	DIFLWAGE	$= \ln(W_l) - \ln(W_k)$	0	0.2640	-0.7680	-0.7680	2,601
	AGE	average age in origin state (for i=1, j=1)	44	1.63	40	47	2,601
	WHITE	proportion of whites in origin state (for i=1, j=1)	0.8560	0.1650	0.1233	0.9940	2,601
	BLACK	proportion of blacks in origin state (for i=1, j=1)	0.0918	0.1362	0	0.8493	2,601
Married; high school (i=2, j=1)	M_{kl}	number of people moving from state k to state l (for k l)	7.35	15.52	0	291	2,550
		$M_{kl} > 0$	9.66	17.16	(610)	(NY to FL)	1940
	P_k	number of people in state k (for i=1, j=1)	4,000	3,945	197	20,162	2,601
	$MPRAT_{kl}$	$=M_{kl}/P_k$ (for k l)	0.0024	0.0066	0	0.2335	2,550
	ATR_k	average state income tax rate in origin (k) state	3.094	1.8577	0	7.39	2,601
	$DIFTAX_{kl}$	$=ATR_l - ATR_k$	0	2.6533	-7.39	7.39	2,601
	W_k	average income from wages in origin (j) state (for i=1, j=1)	25,515	3,851	17,747	35,275	2,601
	DIFLWAGE	$= \ln(W_l) - \ln(W_k)$	0	0.2163	-0.6869	0.6869	2,601
	AGE	average age in origin state (for i=1, j=1)	40	0.7842	38	43	2,601
	WHITE	proportion of whites in origin state (for i=1, j=1)	0.8953	0.1516	0.1272	0.9950	2,601
	BLACK	proportion of blacks in origin state (for i=1, j=1)	0.0705	0.1234	0	0.8545	2,601

TABLE 3.1 (CONT'D)

(i, j)	Variable Name	Variable Description	Mean	St. Dev.	Min	Max	N
Married; college degree (i=3, j=1)	M_{kl}	number of people moving from state k to state l (for k l)	3.42	7.22	0	136	2,550
		$M_{kl} > 0$	5.73	8.60	(1,027)	(NY to NJ)	1,523
	P_k	number of people in state k (for i=1, j=1)	1,121	1,216	90	6,580	2,601
	$MPRAT_{kl}$	$=M_{kl}/P_k$ (for k l)	0.0038	0.0085	0	0.2111	2,550
	ATR_k	average state income tax rate in origin (k) state	3.5073	2.0940	0	8	2,601
	$DIFTAX_{kl}$	$=ATR_l - ATR_k$	0	2.99	-8	8	2,601
	W_k	average income from wages in origin (j) state (for i=1, j=1)	38,270	6,394	24,027	52,361	2,601
	DIFLWAGE	$= \ln(W_l) - \ln(W_k)$	0	0.2488	-0.7790	0.7790	2,601
	AGE	average age in origin state (for i=1, j=1)	41	1.0757	38.935	45	2,601
	WHITE	proportion of whites in origin state (for i=1, j=1)	0.9246	0.1094	0.2746	1	2,601
	BLACK	proportion of blacks in origin state (for i=1, j=1)	0.0388	0.0554	0	0.3673	2,601
Married; graduate degree (i=4, j=1)	M_{kl}	number of people moving from state k to state l (for k l)	2.29	4.76	0	94	2,550
		$M_{kl} > 0$	4.18	5.77	(1,151)	(NY to NJ)	1,399
	P_k	number of people in state k (for i=1, j=1)	719	832	64	4474	2,601
	$MPRAT_{kl}$	$=M_{kl}/P_k$ (for k l)	0.0040	0.0080	0	0.1594	2,550
	ATR_k	average state income tax rate in origin (k) state	3.7835	2.1735	0	8.45	2,601
	$DIFTAX_{kl}$	$=ATR_l - ATR_k$	0	3.10	-8.45	8.45	2,601
	W_k	average income from wages in origin (j) state (for i=1, j=1)	47,967	7,820	29,476	68,366	2,601
	DIFLWAGE	$= \ln(W_l) - \ln(W_k)$	0	0.2429	-0.8413	0.8413	2,601
	AGE	average age in origin state (for i=1, j=1)	44	0.8160	42	47	2,601
	WHITE	proportion of whites in origin state (for i=1, j=1)	0.9258	0.0700	0.5556	1	2,601
	BLACK	proportion of blacks in origin state (for i=1, j=1)	0.0324	0.0394	0	0.2375	2,601

TABLE 3.1 (CONT'D)

(i, j)	Variable Name	Variable Description	Mean	St. Dev.	Min	Max	N
Not married; no high school (i=1, j=0)	M_{kl}	number of people moving from state k to state l (for k l)	0.56	1.6167	0	29	2,550
		$M_{kl} > 0$	2.3181	2.5981	(1,934)	(NY to FL)	616
	P_k	number of people in state k (for i=1, j=1)	330	376	28	2,017	2,601
	$MPRAT_{kl}$	$=M_{kl}/P_k$ (for k l)	0.0022	0.0069	0	0.1084	2,550
	ATR_k	average state income tax rate in origin (k) state	2.7137	1.6937	0	5.95	2,601
	$DIFTAX_{kl}$	$=ATR_l - ATR_k$	0	2.41921	-5.95	5.95	2,601
	W_k	average income from wages in origin (j) state (for i=1, j=1)	12,554	2,819	6,655	19,278	2,601
	DIFLWAGE	$= \ln(W_l) - \ln(W_k)$	0	0.3380	-1.0635	1.0635	2,601
	AGE	average age in origin state (for i=1, j=1)	40	2.13	34	46	2,601
	WHITE	proportion of whites in origin state (for i=1, j=1)	0.7886	0.1712	0.1111	1	2,601
	BLACK	proportion of blacks in origin state (for i=1, j=1)	0.1649	0.1777	0	0.8889	2,601
Not married; high school (i=2, j=0)	M_{kl}	number of people moving from state k to state l (for k l)	3.0690	6.8683	0	107	2,550
		$M_{kl} > 0$	5.3057	8.3488	1,075	(NY to FL)	1,475
	P_k	number of people in state k (for i=1, j=1)	1,190	1,352	156	7,974	2,601
	$MPRAT_{kl}$	$=M_{kl}/P_k$ (for k l)	0.0032	0.0070	0	0.1354	2,550
	ATR_k	average state income tax rate in origin (k) state	3.2722	1.9462	0	7.36	2,601
	$DIFTAX_{kl}$	$=ATR_l - ATR_k$	0	2.7797	-7.36	7.36	2,601
	W_k	average income from wages in origin (j) state (for i=1, j=1)	19,486	3,190	12,616	27,382	2,601
	DIFLWAGE	$= \ln(W_l) - \ln(W_k)$	0	0.2331	-0.7749	0.7749	2,601
	AGE	average age in origin state (for i=1, j=1)	36	1.1075	34	39	2,601
	WHITE	proportion of whites in origin state (for i=1, j=1)	0.8533	0.1434	0.1739	0.9884	2,601
	BLACK	proportion of blacks in origin state (for i=1, j=1)	0.1126	0.1311	0	0.8116	2,601

TABLE 3.1 (CONT'D)

(i, j)	Variable Name	Variable Description	Mean	St. Dev.	Min	Max	N
Not married; college degree (i=3, j=0)	M_{kl}	number of people moving from state k to state l (for k l)	1.7647	4.1964	0	67	2,550
		$M_{kl} > 0$	3.8200	5.5027	(1,372)	(NY to CA)	1,178
	P_k	number of people in state k (for i=1, j=1)	389	495	39	2,919	2,601
	$MPRAT_{kl}$	$=M_{kl}/P_k$ (for k l)	0.0056	0.0124	0	0.1364	2,550
	ATR_k	average state income tax rate in origin (k) state	3.65	2.1942	0	7.74	2,601
	$DIFTAX_{kl}$	$=ATR_l - ATR_k$	0	3.1340	-7.74	-7.74	2,601
	W_k	average income from wages in origin (j) state (for i=1, j=1)	27,663	5,041	17,602	36,747	2,601
	DIFLWAGE	$= \ln(W_l) - \ln(W_k)$	0	0.2674	-0.7361	0.7361	2,601
	AGE	average age in origin state (for i=1, j=1)	37	1.4790	33	40	2,601
	WHITE	proportion of whites in origin state (for i=1, j=1)	0.9116	0.0976	0.3972	1	2,601
	BLACK	proportion of blacks in origin state (for i=1, j=1)	0.0603	0.0650	0	0.3333	2,601
Not married; graduate degree (i=4, j=0)	M_{kl}	number of people moving from state k to state l (for k l)	0.8506	2.1753	0	36	2,550
		$M_{kl} > 0$	2.4845	3.1254	(1,677)	(NY to CA)	873
	P_k	number of people in state k (for i=1, j=1)	185	262	12	1,503	2,601
	$MPRAT_{kl}$	$=M_{kl}/P_k$ (for k l)	0.0058	0.0140	0	0.125	2,550
	ATR_k	average state income tax rate in origin (k) state	3.9068	2.3452	0	8.29	2,601
	$DIFTAX_{kl}$	$=ATR_l - ATR_k$	0	3.3498	-8.29	8.29	2,601
	W_k	average income from wages in origin (j) state (for i=1, j=1)	35,811	7,247	16,856	46,717	2,601
	DIFLWAGE	$= \ln(W_l) - \ln(W_k)$	0	0.3323	-1.0194	1.0194	2,601
	AGE	average age in origin state (for i=1, j=1)	41	1.5701	38	45	2,601
	WHITE	proportion of whites in origin state (for i=1, j=1)	0.9153	0.0830	0.525	1	2,601
	BLACK	proportion of blacks in origin state (for i=1, j=1)	0.0500	0.0592	0	0.3115	2,601

TABLE 3.1 (CONT'D)

(i, j)	Variable Name	Variable Description	Mean	Standard Deviation	Min (count)	Max (from *k* to *l*)	N
Overall	M_{kl}	number of people moving from state k to state l (for k l)	2.5777	7.3783	0	291	20,400
	$M_{kl} > 0$		5.3155	9.8848	(10,507)	(NY to FL)	9,893
	P_k	number of people in state k (for i=1, j=1)	1138	2,011	12	20,162	20,808
	$MPRAT_{kl}$	$=M_{kl}/P_k$ (for k l)	0.0035	0.0091	0	0.2335	20,400
	ATR_k	average state income tax rate in origin (k) state	3.3229	2.0616	0	8.45	20,808
	$DIFTAX_{kl}$	$=ATR_l - ATR_k$	0	2.8759	-8.45	8.45	20,808
	W_k	average income from wages in origin (j) state (for i=1, j=1)	28,086	12,338	6,655	68,366	20,808
	DIFLWAGE	$= \ln(W_l) - \ln(W_k)$	0	0.2714	-1.064	1.064	20,808
	AGE	average age in origin state (for i=1, j=1)	41	3.1300	33	47	20,808
	WHITE	proportion of whites in origin state (for i=1, j=1)	0.8838	0.1367	0.1111	1	20,808
	BLACK	proportion of blacks in origin state (for i=1, j=1)	0.0776	0.1165	0	0.8889	20,808
	ED	Share of education expenditures in total government spending	0.3681	0.0719	0.1689	0.4947	20,808
	WSH	Share of welfare expenditures in total government spending	0.1717	0.0483	0.0669	0.2779	20,808
	EMPRATE	Employment rate	0.9309	0.0171	0.8793	0.9578	20,808
	POP	Population in destination state	3,521,610	3,797,199	333,000	2×10^{07}	20,808
	TOTAL	Total state government spending (in millions of dollars)	8,602.84	10,447.09	1,081	59,680	20,808

TABLE 3.2 – THE ESTIMATED AVERAGE INCOME AND SALES TAX RATES BY EDUCATION GROUP

Education	Marital Status	Average Sales Tax Rate	Maximum	Average Income Tax Rate	Maximum
no high school	married	4.20	6.67	2.66	7.07
	single			2.71	5.95
high school	married	3.59	5.78	3.09	7.39
	single			3.27	7.36
college	married	3.48	5.66	3.51	8.00
	single			3.65	7.74
graduate	married	3.16	5.14	3.78	8.45
	single			3.91	8.29

C. THE ECONOMETRIC MODEL

The general specification is:

$$\frac{M_{ijkl}}{P_{ijk}} = G\left(ATR_{ijl}, ATR_{ijk}, ASR_{il}, ASR_{ik}, ED_l, ED_k,\right.$$

$$\left. WSH_l, WSH_k, W_{ijl}, W_{ijk}, EMP_l, EMP_k, POP_l, D_{ijk}, S_k\right)$$

where:

i = education group (less than high school, high school or some college, college, graduate education);
j = marital status (married, not married);
k = origin state;
l = destination state;

M_{ijkl} = number of people from i-education group and j-marital status moving from state k to state l;

P_{ijk} = number of people from i-education group and j-marital status remaining in state k;

ATR_{ijl} = average state income-tax rate for (i,j) education-marital status group in destination state (l);

ASR_{il} = average state sales-tax rate for (i) education group in destination state (l);

ED_l = share of education spending from state expenditures in destination state l;

W_l = share of welfare spending from state expenditures in destination state l;

W_{ijl} = average income from wages for (i,j) education-marital status group in destination state(l);

EMP_l = employment rate in destination state (l);

POP_l = Population in destination state (l);

D_{ijk} = represents a vector of demographic characteristics of people in origin state in (i,j) education-marital status group;

S_k = origin state dummy.

The specific models estimated in this paper include subsets of the control variables described above, depending on the scope of the analysis and identification issues. A few options are available for estimating such an equation. One method is

Ordinary Least Squares (OLS). While the estimation and the interpretation of the marginal effects for the right-hand-side variables are straightforward, the method falls short of accounting for the specific form of the dependent variable. In particular, the dependent variable here is a fraction that takes values between zero and one, with a large concentration of values at zero (since there are no people observed to move between some origin-destination state combinations). In addition, assuming a linear conditional expectation form by using OLS, will result in fitted values that fall outside the [0,1] interval where the dependent variable lies. The reason for this is because the effects of any particular independent variable cannot be constant over its range and at the same time give rise to fitted values that are bound by 0 and 1.

To perform a similar analysis, the migration literature often makes use the log-odds ratio modeled as a linear function. The model involves the assumption that:

$$\ln\left(\frac{m_{ij}}{1-m_{ij}}\right) = \beta X$$

where m_{ij} represents the flow of migration from state i to j and calculated as the number of actual people who move from i to j (similar to M_{ijkl} used in this analysis). Papke and Wooldridge (1993) discuss a few problems arising from using a log-odds model for fractional response variables. One issue regards the fact that the model becomes problematic when m_{ij} takes the extreme values of zero and one, and adjustments need to be made in these instances. Another issue concerns the difficulties involved in retrieving the conditional expectation (E[y|X]) when y equals the log-odds ratio.

Papke and Wooldridge instead propose a quasi-likelihood method, which takes into account the particular form of the dependent variable and does not require transformations for zero and one values. In addition, a special case of this method exploits the fact that the dependent variable is specified as a fraction with a known denominator (the group size P_{ijk} is known). This additional information enhances the efficiency of estimation. Using the variable notation specified above, the method specifies the conditional mean as (leaving out the *ij* subscripts for simplicity):

$$E\left(\frac{M_{kl}}{P_k}\middle| X_k, P_k\right) = G(X_k\beta)$$

where $M_{kl} = \sum_{n=0}^{P_k} M_{k\ln}$, and M_{kln} is an binary variable that indicates whether a person moves from state *k* to state *l*, and is characterized by the Bernoulli distribution:

$$Prob(M_{k\ln} = 1 | X_k, P_k) = G(X_k\beta)$$

From the assumption that M_{kln} is characterized by a Bernoulli distribution, it follows that M_{kl} has a binomial distribution of the form:

$$f(M \mid X_k, P_k) = \binom{P_k}{P_k M}[G(X_k\beta)]^{P_k M}[G(X_k\beta) * (1 - G(X_k\beta))]^{P_k(1-M)}$$

where M = 0, $1/P_k$, $2/P_k$, …, 1

The conditional log-likelihood function is:

$$\ell_k(b) = P_k\{M_k \log[G(X_k b)] + (1 - M_k)\log[1 - G(X_k b)]\}$$

The assumption of a binomial distribution induces a particular form of the conditional variance, which necessitates the estimation of a robust covariance matrix.[80] Statistical software packages like STATA® are programmed to perform such estimations and provide robust standard errors. The estimates will be obtained by using the logit function as $G(\cdot)$. The next section presents the estimates and provides a discussion of the findings.

IV. FINDINGS

A. The Effect of State Taxes

The empirical analysis will focus first on the impact of local income and sales taxes on rates of migration. The main hypothesis to be tested here is:

Hypothesis 1: The income tax burden is negatively related to the propensity of migration.

The models to be estimated will have the form:

$$\frac{M_{ijkl}}{P_{ijk}} = G\left(ATR_{ijl}, ATR_{ijk}, W_{ijl}, W_{ijk}, EMP_l, EMP_k, POP_l, E_i, MS_j, D_{ijk}, S_k\right) \quad (1)$$

The estimates are presented in Table 3.3, columns 1 – 6. Since the estimates have little interpretative value as they are (except for the sign), the marginal effects are also computed, and are presented in square brackets. The first specification attempts to replicate the stylized facts about migration patterns, and obtain a sense of the direction of the effect of income taxes. It appears that the population size of the destination state has

[80] The variance is $\sigma^2\left(1+\frac{1}{P_k}\right)$

the expected positive effect on migration. Also, the fact that the better educated are more mobile is confirmed by the data. The education dummies are positive, and the COLLEGE and GRADUATE dummies are statistically significant, suggesting that these two groups are significantly more mobile than people with less than a Bachelor's degree. The HIGH SCHOOL dummy is statistically insignificant, suggesting that this group does not differ significantly in the propensity to move from the no-high-school group (the control group here is white male household heads with less than a high-school degree).

Just as in other migration studies, the data show that married individuals are less likely to migrate (see, *e.g.*, Graves and Linneman, 1979). Non-whites and non-blacks also seem more likely to migrate. The income-tax differential variable has the expected negative sign, suggesting a higher propensity to move into states with lower income tax burdens than the origin state. However, it is only significant against the one-sided 10% alternative (p-value = 0.15).

A number of studies using micro-data have found that earnings of internal migrants drop right after the move, but tend to recover in time. One explanation for this finding, based on the equilibrium approach to migration, is that wage differentials represent differences in amenities in various areas (Roback, 1982, 1988). The analysis at hand suggests that this is not the case here. The wage differential is positive and statistically significant, suggesting a higher propensity of moving to areas that offer better economic opportunities. This finding may be partially due to the fact that, in this analysis, the wage in the destination place is inferred from earnings of similar individuals, rather than observed for the particular group of migrants. As such, the wage in the destination state represents *potential* rather than *actual* wage. Individual migrants

may take up jobs that pay similar or different wages from the average wage for their education group in their new state of residence. Note that the first regression does not include origin state dummies. However, it is important to control for the environment in the state of origin, in order to hold fixed the constellation of factors that may give rise to out-migration from the state. When the origin state dummies are included (column 2), the partial effect of the wage differential falls substantially (from 0.011 to 0.002). One explanation could be that the wage differential in the first specification serves as a proxy for other elements of the economic environment in the origin state.

The estimate of the state tax differential improves substantially in the second specification. The marginal effect suggests that an increase of one-standard-deviation in the state tax differential above the mean (from 0 to 2 percentage points) is associate with an increase of 0.0003 (0.00015 partial effect * 2) in the migration rate, or about 0.03 of a standard deviation (the standard deviation of MPRAT for the entire sample is 0.009). At first, this effect may appear small in magnitude, but it should be noted that it is of the same order as the effect of the wage differential. A one standard deviation increase in the wage differential (from 0 – 0.27, or 27% since the variable expresses the change in log wages), is associated by an increase in the migration rate of 0.00054 (0.002 partial effect * 0.27 standard deviation). While the order of the effect is the same as the effect of the tax differential, its magnitude is about twice as great.

In this specification, the education dummies remain strongly significant, and their partial effects are increasing in the amount of education. The marginal effect of the GRADUATE dummy suggests that a person with a graduate or professional degree is characterized by a migration rate of 0.02 (or about two standard deviations) higher than a

person with less than a high-school education. It also appears that someone with a graduate degree is twice as likely to migrate as someone with a college degree (the partial effect on the COLLEGE dummy is only 0.009).

It could be argued that the wage differential in the first two specifications controls for too much. The tax variable already includes wage considerations, and the education dummies to a certain degree control for the level of earnings. The third specification, therefore, leaves out the wage differential, and includes the difference in the employment rate as a measure of the difference in economic opportunities presented by the origin and destination states. The qualitative results, however, remain the same. The fourth specification leaves out all labor-market variables, to provide a comparison of the partial effect of the income-tax variable to the ones obtained in the previous models. Surprisingly, in this model the partial effect of the tax variable drops from 0.0001 to 0.00003. This change suggests that the economic environment may interact with the income-tax variable. The rest of the results are essentially unchanged, and so are most of the partial effects.

The effect of the income tax differential ceases to be significant when both sets of state dummies (origin and destination) are included (model 5). In fact, with both sets of state dummies, all the fiscal variables cease to appear statistically significant (also see model 8). It could be the case that the state dummies are partly controlling for the fiscal environment, and when both sets are included, the advantages or disadvantages of the fiscal structure exhibit themselves through the coefficients of the state dummies.

Next the analysis will examine the following hypothesis:

Hypothesis 2: The sales tax burden is negatively related to the propensity of migration.

The models to be estimated will have the form:

$$\frac{M_{ijkl}}{P_{ijk}} = G\left(ASR_{ijl}, ASR_{ijk}, W_{ijl}, W_{ijk}, EMP_l, EMP_k, POP_l, E_i, MS_j, D_{ijk}, S_k\right) \quad (2)$$

The regression results are presented in Table 3.3, columns 6 – 9. Model (6) is similar to (1) with respect to the control variables included, except for the tax variable. Surprisingly, from the estimates of model 6, it appears that the sales-tax differential has a positive sign, and is statistically significant. The partial effect suggests that a difference of one standard deviation in the sales-tax differential (+2 percentage points), will be associated with a higher migration rate to the state with the higher sales tax of about 0.03 (0.015 partial effect*2 percentage points), or about 3 standard deviations above the mean of the migration rate (MPRAT). The fact that the magnitude of the effect is so large makes the results from model (6) less credible. However, the puzzling effect of the sales tax differential is not robust to the form of the specification, and goes away once state dummies and other controls are included. Models (7) and (8) both include state dummies and model (7) includes a wage-differential variable.

TABLE 3.3 –REGRESSION RESULTS ON THE IMPACT OF LOCAL TAXES ON MIGRATION FLOWS

Dependent Variable: MPRAT = M_{ijkl}/P_{ijk} (i=education; j=marital status; k-origin state; l-destination state)					
Model	(1)	(2)	(3)	(4)	(5)
$DIFTAX_{kl}$	-0.01110†	-0.02203	-0.02877	-0.01962	0.00762
(ATR_l- ATR_k)	(0.00773)	(0.00848)***	(0.00806)***	(0.00853)**	(0.02732)
(income tax)	[-0.00005]	[-0.00015]	[-0.00011]	[-0.00003]	[0.000012]
$DIFSTAX_{kl}$	–	–		–	–
(ASR_l- ASR_k)					
(sales tax)					
DIFLWAGE	2.51237	0.29293		–	–
[ln(W_l) –ln(W_k)]	(0.09831)***	(0.09974)***			
	[0.01162]	[0.00201]			
DIFLEMP	–	–	11.91484	–	–
[ln(EMP_l)-ln(EMP_k)]			(0.93799)***		
			[0.04416]		
LPOPNST	0.61668	0.72956	0.77829	0.75382	–
Ln [POP_l]	(0.02091)***	(0.01883)***	(0.01665)***	(0.01682)***	
(pop. in destination state)	[0.00285]	[0.00501]	[0.00288]	[0.00123]	
HIGH SCHOOL	0.07675	-0.85898	-1.55697	-0.86479	-0.86372
	(0.15416)	(0.10638)***	(0.09368)***	(0.10608)***	(0.10324)***
	[0.00036]	[-0.00491]	[-0.00637]	[-0.00117]	[-0.00108]
COLLEGE DEGREE	1.69447	0.98629	-0.01186	0.99408	0.99232
	(0.17111)***	(0.10400)***	(0.02016)	(0.10397)***	(0.10107)***
	[0.01346]	[0.00903]	[-0.00004]	[0.00216]	[0.00199]

TABLE 3.3 (CONT'D)	(1)	(2)	(3)	(4)	(5)
GRADUATE	1.50675	1.74368	1.26506	1.75652	1.75270
	(0.12533)***	(0.08997)***	(0.62498)**	(0.09027)***	(0.08917)***
	[0.01115]	[0.02097]	[0.00469]	[0.00502]	[0.00463]
MARRIED	-2.37875	-1.56560	2.64238	-1.55789	-1.56045
	(0.08692)***	(0.09325)***	(0.74190)***	(0.09404)***	(0.09265)***
	[-0.01379]	[-0.01189]	[0.00979]	[-0.00280]	[-0.00259]
AGE	0.20343	-0.01039	-0.86506	-0.01160	-0.01091
	(0.02188)***	(0.02008)	(0.10588)***	(0.02022)	(0.01992)
		[-0.00002]	[-0.00266]	[-0.00002]	[-0.00002]
BLACK	-0.56111	1.18414	0.99477	1.25117	1.21042
	(0.86713)	(0.62753)*	(0.10374)***	(0.62785)**	(0.62396)
	[-0.00260]	[0.00814]	[0.00493]	[0.00203]	[0.00182]
OTHER	1.73529	2.69703	1.75780	2.64736	2.66214
	(0.34299)***	(0.74904)***	(0.09006)***	(0.74252)***	(0.70128)
	[0.00803]	[0.01853]	[0.01146]	[0.00431]	[0.00400]
ORIGIN STATE DUMMIES	no	yes	yes	yes	yes
DESTINATION STATE DUMMIES	no	no	no	no	yes
CONSTANT	-29.48440	-22.55384	-23.58406	-22.85891	-11.99575
	(1.00267)***	(0.85520)***	(0.85500)**	(0.85279)***	(0.79299)***
Observations	20,400	20,400	20,400	20,400	20,400

Note: Robust standard errors in parentheses; Marginal effects in square brackets.
* significant at 10%; ** significant at 5%; *** significant at 1%;
† p-value = 0.15

TABLE 3.3 (CONT'D)

Model	(6)	(7)	(8)	(9)
$DIFTAX_{kl}$	–	–	–	-0.02734
$(ATR_l - ATR_k)$				(0.00904)***
(income tax)				[-0.00004]
$DIFSTAX_{kl}$	0.14958	-0.01219	0.18371	-0.03126
$(ASR_l - ASR_k)$	(0.01548)***	(0.01636)	(0.10795)	(0.01716)***
(sales tax)	[0.00017]	[-0.00005]	[0.00028]	[-0.00005]
DIFLWAGE	2.42357	0.26422	–	0.29054
$[\ln(W_l) - \ln(W_k)]$	(0.10091)***	(0.10083)**		(0.09993)***
	[0.00268]	[0.00100]		[0.00047]
DIFLEMP	–		–	–
$[\ln(EMP_l) - \ln(EMP_k)]$				
LPOPNST	0.58243	0.73780	–	0.73892
Ln $[POP_l]$	(0.02088)***	(0.01911)**		(0.01912)***
(pop. in destination state)	[0.00064]	[0.00279]		[0.00120]
HIGH SCHOOL	0.03119	-1.56736	-0.85674	-0.85970
	(0.16057)	(0.09329)**	(0.10313)***	(0.10654)***
	[0.00003]	[-0.00654]	[-0.00107]	[-0.00116]
COLLEGE DEGREE	1.66335	-0.00999	0.99993	0.98615
	(0.17826)***	(0.02009)	(0.10087)***	(0.10415)***
	[0.00312]	[0.00436]	[0.00201]	[0.00214]

TABLE 3.3 (CONT'D)	(6)	(7)	(8)	(9)
GRADUATE	1.53219	1.15524	1.76535	1.74291
	(0.12881)***	(0.62691)	(0.08964)***	(0.09001)***
	[0.00273]	[0.01026]	[0.00468]	[0.00496]
MARRIED	-2.35375	2.71905	-1.55939	-1.56499
	(0.08549)***	(0.75041)**	(0.09264)***	(0.09325)***
	[-0.00324]	[-0.00270]	[-0.00259]	[-0.00282]
AGE	0.19543	-0.85874	-0.01044	-0.01063
	(0.02225)	(0.10626)**	(0.01983)	(0.02009)
			[-0.00002]	
BLACK	-0.39454	0.98582	1.27806	1.18346
	(0.87472)	(0.10381)**	(0.61944)**	(0.62798)
	[-0.00044]	[0.00496]	[0.00192]	[0.00193]
OTHER	1.92097	1.74158	2.41629	2.73447
	(0.31843)***	(0.08989)**	(0.68759)***	(0.75200)***
	[0.00212]	[0.01150]	[0.00363]	[0.00445]
ORIGIN STATE DUMMIES	no	yes	yes	yes
DESTINATION STATE DUMMIES	no	no	yes	no
CONSTANT	-28.67232	-22.68659	-12.02946	-22.67907
	(1.02037)***	(0.85263)**	(0.79031)***	(0.85422)***
Observations	20,400	20,400	20,400	20,400

Note: Robust standard errors in parentheses; Marginal effects in square brackets.
* significant at 10%; ** significant at 5%; *** significant at 1%

Model (9) includes both the income-tax and the sales-tax differentials. Since all taxes are bound to be considered simultaneously by migrants, rather than one at a time, including both tax variables makes more sense. In model (9), both tax variables have negative signs and are statistically significant. The partial effects of the two tax variables are comparable in magnitude (0.00004 for the income tax and 0.00005 for the sales tax.) Their negative signs suggest that higher local taxes in the host state discourage migration to that state. A one-standard-deviation increase in each tax (or 2 percentage points each, since the standard deviations are the same for both tax variables), is associated with a decline in the rate of migration of 0.00018 (0.00004*2 +0.00005*2), or two percent of a standard deviation. On the other hand, a one-standard-deviation increase in the wage-differential variable, or 27%, appears to be associated with a 0.00013 increase in the rate of migration (0.00047 partial effect *0.27 standard deviation), or 1.4% of a standard deviation. In light of this comparison, the marginal effect of the two local taxes combined appears quite substantial.

Another variable that merits consideration is the MARRIED indicator. As claimed by Mincer and evidenced in the literature, it appears that married heads of households are less likely to migrate. The estimated partial effect suggests that married people are associated with migration rates of about of 0.003 - 0.014 lower than non-married people.

Throughout most of the analysis presented in Table 3.3, the HIGH SCHOOL dummy assumes a negative sign, suggesting that people with a high school diploma or some college are actually less mobile than people with less than a high-school education. It will be seen in the rest of the analysis that follows, that this effect is quite robust to the

particular specification chosen. In model (9), the partial effect calculated for this variable suggests that people with a high-school diploma and/or some college, are associated with a migration rate of 0.0012 lower than people with less than high-school education (or 13% of a standard deviation of MPRAT). The reason why this effect comes through may be found in the welfare migration literature. Since people with less than a high-school degree may be more likely to receive welfare, they may also be more likely to respond to differences in the generosity of welfare programs across states. The literature has documented welfare-induced migration, although the different analyses examined do not include a breakdown by education level of the population prone to such migration. Rather, the welfare-induced migration is often evidenced among the poor or non-whites. The data used in this chapter will not allow any deeper analysis of this hypothesis, since the focus of the study is the other end of the spectrum, namely skilled labor. Efforts were made to leave out of the sample people who might respond to welfare incentives, by leaving out female heads of households.

From the analysis conducted thus far, it can be concluded that both hypotheses, (1) and (2), speculating a negative effect of local taxes on migration rates, are confirmed. The evidence confirming the second hypothesis is not very robust, but the measure of the sales-tax variable is quite rough compared to the income-tax variable, and as a result may include large measurement errors. Since higher local income taxes appear to affect migration rates negatively, and since these taxes depend largely on earnings, which in turn are a direct function of human capital, it can be inferred that human capital is more responsive to such tax-rate differentials. This statement cannot hold in the case of the sales tax, since from the estimates presented here, it appears that this tax is regressive,

and, as a result, the higher-skilled labor would be less sensitive to higher sales taxes than less-skilled labor. The next section investigates the impact of local spending policies on migration rates.

B. The Effect of Education Spending

This section will employ the following general model to carry out the empirical analysis:

$$\frac{M_{ijkl}}{P_{ijk}} = G\left(ED_l, ED_k, WSH_l, WSH_k, W_{ijl}, W_{ijk}, EMP_l, EMP_k, POP_l, D_{ijk}, S_k\right) \quad (3)$$

Note that, since the focus of this section is local spending, the model does not include any tax variables. In the literature, studies that use tax collections at the state level as a measure of the tax burden cannot include both measures of spending and taxes in the same equation. Because this study estimates taxes at the individual level, an equation including both sets of taxes and spending variables can be estimated. Such estimation is carried out toward the end of this section.

Local spending in education and welfare (ED, and WSH) will be expressed as shares of total local government spending. The reason why shares rather than per-capita figures are preferred for this analysis is that the actual distribution of fiscal benefits among the population is very skewed, and averages (such as per-capita figures) would give little, if any, useful information about the policies and preferences of local governments with respect to various spending categories. In addition, states that are generous in their spending are bound to distribute more per-capita fiscal benefits across the board, thus obscuring the relative importance of the different spending categories.

The shares of each of the spending categories would give a better sense of the importance that the particular state places on that category. This would be especially true in the case when the tax burden is held constant. Since a bigger share of a smaller pie may be less attractive than a smaller share of a bigger pie, in the regression analysis below, total spending and the tax burden are included in the list of control variables.

The effect of education spending on the migration of skilled labor is more ambiguous than the effect of taxes. Arguments supporting opposing claims can be raised. At one end is the argument that maintains higher education spending to be more attractive to high-skilled individuals. There are two reasons why this relationship would be expected. One has to do with the externality argument, claiming that everyone prefers a better-educated public. It is only natural to extend this hypothesis to claiming that the better educated would place a higher value on a better educated public, and the externalities associated with a higher average level of education. Another reason that supports such a hypothesis involves dynastic utility functions. If the welfare of the children enters the utility function of the parents, a state with better education funding would be more appealing to the parents, because it would provide a higher expected utility for their children, all else equal. This effect would be increasing in the amount of education of the parent, if it is assumed that better-educated parents have a higher taste for education.

At the other end of the spectrum lies the argument derived from the tax competition literature. This argument would claim that high-skilled labor relocates to take advantage of fiscal benefits. The spillovers associated with any skilled individual leaving one jurisdiction and joining another create inefficiencies in a system of

decentralized local governments, and public goods, (such as education in the case of Justman and Thisse, 2000), are underprovided. High-skilled individuals do not take into account the extra fiscal burden they impose on the origin state when they move. The reduced tax base in the origin state caused by such a move, will either be translated into higher taxes for everyone to fund a fixed level of spending, or in reduced education spending, as claimed by Justman and Thisse.

Paired with a life-cycle approach, this argument would translate to individuals relocating at different points in their life-cycle as their needs for public spending on education, and in general for net fiscal benefits, change. For any individual, net fiscal benefits change over the life-cycle from positive in the early years, to negative when they start earning higher wages and no longer qualify for government assistance. For individuals who intend to invest in human capital, it pays to behave strategically. In the early years, when the individual is investing heavily in human capital, states with higher education expenditures will be offering better (and less expensive) opportunities to invest in human capital. Such states, however, will be associated with higher income and other local taxes to finance the higher level of spending on education. When the human-capital investment period is complete, a state with lower taxes and fewer fiscal amenities becomes more attractive to the now high-skilled individuals. Therefore, it could be possible to observe high skilled individuals migrating toward states with lower education spending.

Since prior studies have shown that higher spending on education attracts higher rates of migration, the third hypothesis will be formulated as follows, keeping in mind that the alternative is just as likely:

Hypothesis 3: High-skilled labor is more likely to migrate towards states where education spending constitutes a higher share of total government spending.

Estimates of model (3) are presented in Table 3.4. All regressions include origin state dummies. The first specification only includes a difference in the shares of education spending (in logarithmic form) as the measure of interest. The positive sign and the significance of this variable suggest that states with higher shares of education spending attract more migration. Since, at this point, the regressions do not include controls for tax burden, the variable DIFLEDSH represents primarily the degree of priority that education spending receives in a particular state compared to other spending categories. Interestingly, the interactions of this variable with COLLEGE and GRADUATE have a negative sign and are highly significant. The estimated partial effects appear in square brackets. Because, by construction, the variables measuring the differences in benefits (DIFLEDSH, DIFLWSH, and DIFLTOT) have a zero mean, the partial effects of the interaction terms between these variables and the education dummies measure the partial effect around zero. Since this information is not very useful, other partial effects are estimated one standard deviation above zero (for each respective difference variable above), and they appear in Table 3.5. Here, partial effects are estimated for specific interesting combinations of education categories. The matrices presented include the marginal effect on the probability of migration when moving from one education category to another. They are all calculated at one standard deviation above the mean of the variables representing differences in state shares of education spending and total spending. Case (1) in Table 3.5 corresponds to the estimates obtained in the first

regression in Table 3.4. The estimated effects suggest that a one-standard-deviation increase in education spending shares is associated with lower migration rates for college graduates of about 0.0027 when compared to people with less than a high-school education, and 0.0044 lower than people with a high-school education. People with a graduate degree have lower migration rates attributed to education spending shares of about 0.013, 0.016, and 0.008 when compared to people with less than a high-school education, with a high-school diploma, and college, respectively. While these estimates may appear small, it should be noted that, when compared to the migration rates for these education groups, they are quite noteworthy. For the case of college graduates, the differential in migration rates due to education spending shares when compared to people with less than high school is about 25% of one standard deviation of MPRAT, whereas compared to people with a high-school diploma, the migration rate difference attributed to education spending represents 41% of a standard deviation.[81] For people with a graduate degree, the difference in migration rates due to disparities in education spending represent 113%, 139%, and 70% of one standard deviation in MPRAT. It should be noted that these estimates correspond to the case in which the difference in the shares of education spending between the origin and destination state is about one standard deviation above the mean. In contrast, the partial effect on the employment difference suggests a higher migration rate of 0.0014 (or 15% of a standard deviation of MPRAT) associated with a one-standard-deviation increase in the employment rate change (0.05 partial effect * 0.027 standard deviation of DIFEMP). In light of this comparison, the additional effect of the share of education spending on high-skilled labor appears quite

[81] The standard deviation of MPRAT for college graduates who report to have changed state of residence is 0.0107. For people with a graduate degree, this measure is 0.0115.

dramatic. So, while a tendency is observed to migrate towards states with higher shares of education spending, the better educated are much less likely to do so.

TABLE 3.4 –REGRESSION RESULTS ON THE IMPACT OF LOCAL SPENDING ON MIGRATION RATES

Dependent Variable: MPRAT $=M_{ijkl}/P_{ijk}$ (i=education; j=marital status; k-origin state; l-destination state)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$DIFTAX_{kl}$	–	–	–	–	-0.05614	-0.03801	–
$(ATR_l - ATR_k)$					(0.00873)***	(0.00890)***	
				–	[-0.00021]	[-0.00014]	
$DIFSTAX_{kl}$	–	–	–		-0.06741	-0.05817	–
$(ASR_l - ASR_k)$				–	(0.01739)***	(0.01704)***	
					[-0.00025]	[-0.00022]	
DIFLTOT	–	0.28349	0.20490	0.27383	0.51877	0.33119	0.09056
$[\ln(TOT_l) - \ln(TOT_k)]$		(0.09151)***	(0.09530)**	(0.09603)***	(0.08971)***	(0.09027)***	(0.07771)
		[0.00107]	[0.00076]	[0.00101]	[0.00193]	[0.00125]	[0.00033]
DIFLEDSH	1.40588	1.36488	1.60634	1.58266	1.71591	1.67998	–
$[\ln(ED_l) - \ln(ED_k)]$	(0.18613)***	(0.20656)***	(0.21044)***	(0.19725)***	(0.19640)***	(0.21002)***	
	[0.00509]	[0.00516]	[0.00602]	[0.00587]	[0.00640]	[0.00632]	
DIFLWSH	–	–	–	–	–	–	-0.91180
$[\ln(WSH_l) - \ln(WSH_k)]$							(0.12541)***
							[-0.00341]
HI* DIFLEDSH	-0.22909	-0.23234	-0.28791	-0.22653	-0.23824	-0.29903	–
	(0.21712)	(0.21983)	(0.22318)	(0.21580)	(0.21516)	(0.22282)	
	[-0.00083]	[-0.00088]	[-0.00107]	[-0.00084]	[-0.00089]	[-0.00112]	
COL* DIFLEDSH	-0.73768	-0.75380	-0.86336	-0.74329	-0.75234	-0.87487	–
	(0.18929)***	(0.19078)***	(0.19245)***	(0.18753)***	(0.18644)***	(0.19172)***	
	[-0.00267]	[-0.00285]	[-0.00323]	[-0.00276]	[-0.00280]	[-0.00329]	
GRAD* DIFLEDSH	-1.20063	-1.21597	-1.35771	-1.20034	-1.22179	-1.37804	–
	(0.17365)***	(0.17527)***	(0.17869)***	(0.17230)***	(0.17134)***	(0.17805)***	
	[-0.00435]	[-0.00460]	[-0.00509]	[-0.00445]	[-0.00455]	[-0.00518]	

TABLE 3.4 (CONT'D)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
HI * DIFLWSH	–	–	–	–	–	–	0.13557 (0.13659) [0.00050]
COL * DIFLWSH	–	–	–	–	–	–	0.14157 (0.14511) [0.00052]
GRAD * DIFLWSH	–	–	–	–	–	–	0.20811 (0.14627) [0.00077]
HI * DIFLTOT	–	-0.06462 (0.04038) [-0.00024]	-0.07192 (0.04085) [-0.00026]	-0.06341 (0.03954) [-0.00024]	-0.06101 (0.03903) [-0.00023]	-0.07140 (0.04068) [-0.00027]	-0.08968 (0.04480)** [-0.00033]
COL * DIFLTOT	–	0.03409 (0.04037) [0.00013]	0.01380 (0.04127) [0.00005]	0.03477 (0.03955) [0.00013]	0.04368 (0.03914) [0.00016]	0.01944 (0.04120) [0.00007]	0.00477 (0.04604) [0.00001]
GRAD * DIFLTOT	–	0.09232 (0.04198)** [0.00035]	0.07093 (0.04282) [0.00026]	0.09299 (0.04120)** [0.00034]	0.10180 (0.04080)** [0.00038]	0.07605 (0.04276) [0.00029]	0.05740 (0.04802) [0.00021]
DIFLEMP $[\ln(EMP_l) - \ln(EMP_k)]$	13.80462 (0.96800)*** [0.04996]	–	–	13.73387 (0.96745)*** [0.05092]	14.56998 (0.94364)*** [0.05431]	–	11.13680 (0.96464)*** [0.04166]
DIFLWAGE $[\ln(W_l) - \ln(W_k)]$	–	–	0.59884 (0.12105)*** 0.00224	–	–	0.58460 (0.12112)*** [0.00220]	
LPOPNST Ln $[POP_l]$	0.78184 (0.01668)*** [0.00283]	0.44638 (0.07950)*** [0.00169]	0.48944 (0.08143)*** [0.00183]	0.47792 (0.08679)*** [0.00177]	0.24838 (0.08051)*** [0.00093]	0.37924 (0.07711)*** [0.00143]	0.78632 (0.06350)*** [0.00294]

TABLE 3.4 (CONT'D)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
MARRIED	-1.54348	-1.53623	-1.54583	-1.53650	-1.53000	-1.54014	-1.55048
	(0.08989)***	(0.09030)***	(0.08870)***	(0.08996)***	(0.08987)***	(0.08866)***	(0.09648)***
	[-0.00616]	[-0.00640]	[-0.00639]	[-0.00627]	[-0.00628]	[-0.00638]	[-0.0064]
AGE	-0.01976	-0.02282	-0.02151	-0.02270	-0.02492	-0.02320	-0.01479
	(0.01939)	(0.01941)	(0.01909)	(0.01935)	(0.01938)	(0.01912)	(0.02078)
	[-0.00007]	[-0.00009]	[-0.00008]	[-0.00008]	[-0.00009]	[-0.00009]	[-0.00005]
BLACK	0.50689	0.41948	0.22108	0.42877	0.47346	0.24677	1.17061
	(0.56599)	(0.58433)	(0.57851)	(0.58155)	(0.58524)	(0.58070)	(0.64585)
	[0.00183]	[0.00159]	[0.00082]	[0.00159]	[0.00176]	[0.00093]	[0.00437]
OTHER	1.77151	2.02755	2.00295	2.04014	2.09480	2.04998	3.08621
	(0.72581)**	(0.71516)***	(0.71716)***	(0.71293)***	(0.71395)***	(0.71853)***	(0.74332)***
	[0.00641]	[0.00767]	[0.00751]	[0.00756]	[0.00781]	[0.00771]	[0.01154]
HIGH SCHOOL	-0.94033	-0.89618	-0.88666	-0.89629	-0.90374	-0.89276	-0.81451
	(0.09923)***	(0.09666)***	(0.09637)***	(0.09646)***	(0.09693)***	(0.09671)***	(0.10348)***
	[-0.00279]	[-0.00280]	[-0.00275]	[-0.00274]	[-0.00278]	[-0.00277]	[-0.00256]
COLLEGE	0.91357	0.87244	0.86585	0.87296	0.86570	0.85928	0.96243
	(0.09671)***	(0.09803)***	(0.09795)***	(0.09774)***	(0.09842)***	(0.09848)***	(0.10501)***
	[0.00430]	[0.00424]	[0.00415]	[0.00415]	[0.00413]	[0.00413]	[0.00476]
GRADUATE	1.73271	1.65102	1.64057	1.65079	1.64855	1.63841	1.68210
	(0.08111)***	(0.08414)***	(0.08405)***	(0.08382)***	(0.08421)	(0.08431)***	(0.09498)***
	[0.01093]	[0.01055]	[0.01034]	[0.01033]	[0.01037]	[0.01035]	[0.01075]
CONSTANT	-23.02769	-17.49291	-18.14999	-18.33191	-14.83369	-16.42756	-23.21456
	(0.82807)***	(1.42776)***	(1.43915)***	(1.52490)***	(1.44289)***	(1.38630)***	(1.24540)***
OBSERVATIONS	20,400	20,400	20,400	20,400	20,400	20,400	20,400

Note: Robust standard errors in parentheses; Marginal effects in square brackets; All regressions include origin state dummies.
* significant at 10%; ** significant at 5%; *** significant at 1%

TABLE 3.5 PARTIAL EFFECTS OF INTERACTION TERMS COMPUTED AT ONE STANDARD DEVIATION ABOVE THE MEAN OF DIFLEDSH AND DIFLTOT

Case (1) (Table 3.4, specification (1); Partial effects of EDUC*DIFLEDSH)

To: / From:	< High School	High School	College	Graduate
< High School	0	0.00053	-0.00265	-0.01270
High School		0	-0.00437	-0.01550
College			0	-0.00840

Case (2) (Table 3.4, specification (2); Partial effects of EDUC*DIFLEDSH and EDUC * DIFLTOT)

To: / From:	< High School	High School	College	Graduate
< High School				
EDUC*DIFLEDSH	0	0.00085	-0.00415	-0.02074
EDUC * DIFLTOT	0	0.00024	0.00019	0.00157
High School				
EDUC*DIFLEDSH		0	-0.00690	-0.02519
EDUC * DIFLTOT		0	0.00031	0.00191
College				
EDUC*DIFLEDSH			0	-0.01405
EDUC * DIFLTOT			0	0.00107

Case (3) (Table 3.4, specification (3); Partial effects of EDUC*DIFLEDSH and EDUC * DIFLTOT)

To: / From:	< High School	High School	College	Graduate
< High School				
EDUC*DIFLEDSH	0	0.00102	0.00398	-0.01997
EDUC * DIFLTOT	0	0.00026	0.00006	0.00104
High School				
EDUC*DIFLEDSH		0	-0.00705	-0.02480
EDUC * DIFLTOT		0	0.00011	0.00130
College				
EDUC*DIFLEDSH			0	-0.01371
EDUC * DIFLTOT			0	0.00072

Case (4) (Table 3.4, specification (6); Partial effects of EDUC*DIFLEDSH and EDUC * DIFLTOT)

To: / From:	< High School	High School	College	Graduate
< High School				
EDUC*DIFLEDSH	0	0.00130	-0.00488	-0.02462
EDUC * DIFLTOT	0	0.00031	0.00011	0.00136
High School				
EDUC*DIFLEDSH		0	-0.00868	-0.03061
EDUC * DIFLTOT		0	-0.00019	0.00169
College				
EDUC*DIFLEDSH			0	-0.01693
EDUC * DIFLTOT			0	0.00093

The second specification includes a variable that controls for total local government spending. It appears that higher local spending is associated with a higher migration rate. The estimated partial effect on the total spending differential, suggests that a state with local spending of one standard deviation higher than the origin state will be associated with an in-migration rate of 0.0015 higher, or 16% of a standard deviation of MPRAT (0.0011 partial effect * 1.33 standard deviation of DIFLTOT). Even when holding fixed total government expenditures, the better educated seem less likely to migrate to states with higher shares of education spending. In fact, the estimated partial effects on the COLLEGE and GRADUATE indicators have almost doubled in magnitude from the previous specification (Table 3.5, Case (2)). It appears that the likelihood of a college graduate to migrate to a state with higher shares of education spending is 0.004 lower than that of someone with less than a high-school education and 0.007 lower than someone with a high school degree. For those college graduates who change states of residence during the period under consideration, these figures represent 37% and 65% of a standard deviation in migration rates for this group. At the same time, college graduates are more likely to migrate to states with higher total spending, as evidenced by the positive coefficient on the interaction term COLLEGE * DIFLTOT. The partial effects presented in Table 3.5, Case (2), suggest that the migration rates associated with one-standard-deviation increase in total government spending are about 0.00019 higher when compared to people with less than a high-school education, and 0.00031 when compared to people with a high-school diploma. For people with graduate degrees, a one-standard-deviation increase in the share of education spending decreases the rates of migration by about 0.021, 0.025, and 0.014 when compared to the group without a high-

school diploma, the group with a high-school diploma, and the group with a college degree, respectively. There is some indication that higher total spending increases the propensity of people with graduate or professional degrees to migrate, as indicated by the positive and significant coefficient on the interaction term GRADUATE*DIFLTOT. The estimated partial effect on this interaction term suggests that, holding the share of education expenditure constant, a one-standard-deviation difference in total spending is associated with a 0.00157 (or 14% of a standard deviation) increase in the migration rate for people with graduate degrees, as compared to people with less than high-school education.

Note that the second regression does not include a measure of the difference in labor-markets opportunities among the states. Therefore, the third specification includes a difference in wage rates. The inclusion of this variable is expected to decrease the relative importance of the fiscal variables, because, after all, labor-market incentives constitute stronger motivations for migrating than local fiscal amenities. As a matter of fact, the marginal effect of the difference in total spending drops from 0.0011 to 0.0008. However, the marginal effect of the difference in education spending shares increases from 0.005 to 0.006. The marginal effects of the interactions of education shares with the education dummies decrease very slightly, as evidenced by the numbers in Table 3.5, Case (4). Therefore, the observation of interest remains that holding fixed the potential wage that can be earned at the new location, as well as total government spending, a higher share of education spending is associated with lower migration rates from people with a college or graduate degree.

The fourth specification replaces the wage differential with a difference in employment rates, which is interpreted as a proxy for labor-market opportunities available in the origin and host states. This regression is intended as a robustness check, and it appears that the switch in labor-market variables does not influence the findings substantially. The partial effects of the variables of interest change very slightly, and the qualitative results remain essentially the same.

The fifth regression includes both sets of fiscal variables, taxes and spending. Interestingly, all the fiscal variables have the expected signs and are significant statistically. In effect, the partial effects of the state taxes have increased drastically when compared to the estimates in Table 3.3. The marginal effect of the income-tax differential suggests an increase in the rate of migration of about 0.0004, or 4.4% of a standard deviation of MPRAT (0.0002 partial effect* 2 percentage points). The estimates in Table 3.3, column 9, suggested a decline in the rate of migration of about half as much (only 2% of a standard deviation) for the same change in the tax rate differential. The same can be said about the sales-tax rate. Its estimated partial effect suggests that a state with a sales tax of two percentage points lower will be attracting migration at a rate of about 0.0005 higher (or 5.56% of a standard deviation in MPRAT). The partial effects of the spending variables also increase in value, but not quite as drastically as the effect of taxes. These changes suggest that all the fiscal variables interact, and states with the same amount of spending but with lower income and sales taxes are highly attractive to migrants (especially when compared to the case where spending is allowed to vary, as in equation 9, table 3.3).

The sixth equation is offered for comparison reasons, as it replaces the wage differential with the difference in employment rates. Results seem to be robust to this change, with minor changes in the partial effects. The partial effects of interest between education categories and differences in education spending shares and total spending are calculated and presented in Table 3.5, Case (4). It appears that after controlling for the tax burden, the negative effect of education spending shares on migration rates of the better educated are even more pronounced. For people with a college degree, the partial effect on COLLEGE*DIFLEDSH has increased from 0.004 to 0.005 (in absolute value) as compared to people with less than high school education. When compared to people with high school or some college, the marginal effect of the interaction term has increased from 0.007 to 0.009 in absolute value. The same holds for people with a graduate degree, who, when compared to the group with less than high-school education, have estimated partial effects that increase in absolute value from 0.02 to 0.025. When compared to the group that has a high-school diploma or some college education, the partial effect of the interaction experiences a similar increase in absolute value from 0.025 to 0.031. Finally, opposed to the group with college degrees, people with graduate degrees appear to have marginal migration rates that are 0.017 lower (as compared to the 0.014 estimated when the tax burden was not included in the regression).

Finally, the last specification includes welfare spending. It appears that states that spend a higher share of their budget on welfare are less attractive to everyone in the sample. Because the data were constructed in such a way as to exclude potential welfare recipients, it is hard to obtain a strong effect of this variable on the different education groups. In effect, none of the interactions of the education dummies with the welfare

variable are significant, and the fact that the welfare difference appears negative indicates that the effort to exclude potential welfare recipients was successful.

In light of these findings, it can be concluded that higher total local spending and a higher share of education spending are associated with higher migration rates. However, people with a college or graduate degree appear less likely to migrate in states that place more emphasis on education spending. This refutes hypothesis 3 above. The reason why these findings are observed may have to do with the fact that a higher share of education funding signifies more spending on public education only. If indeed the utility function of the better educated includes the utility of the children, the existence of private schooling makes public spending on education a superfluous expense to the extent that the private system is chosen. This high-skilled portion of the sample is less likely to benefit from education expenditures, while they are likely to contribute more to such spending through their higher income-tax burden. Note that the observed effect of the share of education spending on the higher skilled in the sixth equation is net of estimated taxes. Another interpretation, therefore, would be that holding constant the tax burden, the better educated tend to move to states with a lower share of government spending on public education. The other possibility why such findings emerge was discussed earlier as exhibiting migration features described in tax competition studies. Because the regression analysis fails to infer causality one way or another, it could be that the better-educated receive education in states with higher education spending and then migrate toward states with lower spending (and as a result lower taxes). Such behavior would manifest itself in the form of lower migration rates of skilled labor towards states with large shares of education spending. Evidence supports this hypothesis, but is far from

being conclusive. One way to pinpoint this effect would be to concentrate on young people between 18-30 years of age, at the time when investment in higher education begins and relocation afterwards may occur.

V. Conclusion

Literature on human-capital migration has generated a number of ambiguous results on the direction of migration and composition of migrants. This study estimates the impact of fiscal variables on migration rates, with a focus on the implications for high-skilled labor. Data from the 1990 Census of the Population were used in the empirical analysis.

Effort was made to obtain education-specific tax variables. Since prior studies (which have reached ambiguous results regarding the effect of taxes on migration) have used taxes at the government level, obtaining individual-level measures of taxes appears to have reduced measurement error and improved estimation.

The findings suggest that high-skilled labor is more mobile, and it is characterized by higher migration rates toward states with lower income-tax and sales-tax rates. At the same time, a higher share of government spending on education appears to attract higher rates of migration. The better-educated are characterized by lower rates of migration towards states with higher shares of education spending. Among the competing explanations for this phenomenon, is the one stemming from the tax competition literature claiming that individuals will move in response to differences in fiscal

environments among similar jurisdictions. Since high-skilled individuals observed here have already acquired their human capital, moving to states with higher education amenities is only going to benefit them if they derive utility from education prospects for their children. The existence of a private school system may make public spending on education less attractive to potential high-skilled migrants.

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