ESSAYS IN LABOR ECONOMICS AND SYNTHETIC DATA METHODS

A Dissertation

Presented to the Faculty of the Graduate School

of Cornell University

In Partial Fulfillment of the Requirements for the Degree of

Doctor of Philosophy

by

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August 2009

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Cornell University 2009

Three topics are investigated in these chapters: the causes and consequences of lateral job mobility within firms, the impact of incentives on human behavior in the context of capital punishment and deterrence, and the development of new synthetic data methods for confidentiality protection of public use data. The extent and importance of lateral mobility is not well-established in economics and Chapter 1 contributes new and important findings to the literature. Using a panel of more than 500 firms and 48,000 white-collar workers, I find relatively high rates of lateral mobility, that this mobility is statistically different from other transitions, and that the compensation growth associated with lateral mobility is economically meaningful. I also investigate the relationships between worker performance, compensation growth and job mobility. Even when controlling for productivity differences, significant earnings growth occurs directly through the change in jobs. The results provide some evidence that the observed lateral mobility may be the result of job rotation. In light of continued debate of whether capital punishment deters crime, Chapter 2 revisits my previous work on this issue and shows that the deterrence results hold under alternative measurements of key variables, multiple statistical specifications and subsets of the data. Chapter 3 develops methodology that solves the need for statistical agencies to suppress certain data items because releasing those cells to the public yields a risk of exposing someone’s personal information. I show that the synthetic data adequately protect the confidential data and are superior in terms of its analytical validity.

BIOGRAPHICAL SKETCH

Before attending Cornell University, Kaj Gittings attended the University of Colorado at Denver where he received a Master's of Arts degree in Economics in 2002 and a Bachelor's of Arts in Economics in 2000. While attending the University of Colorado at Denver, he was honored as the 2002 Outstanding Graduate Student in the College of Arts and Sciences in part due to his research on the economics of capital punishment. This work was his first publication and was published in the *Journal of Law and Economics* in 2003. The article eventually led to another piece of research on capital punishment that became the second chapter of his dissertation. Before leaving Cornell, he accepted a tenure-track position as Assistant Professor in the Department of Economics at Louisiana State University.

This dissertation is dedicated to my grandmother, Lorraine Gittings, who passed away on March 10, 2005.

ACKNOWLEDGMENTS

I owe a great deal of gratitude to my Chair, Jed DeVaro, for his time and effort in advising me through the completion of this dissertation. He taught my first Labor Economics class and is responsible for sparking my interest in the field of Personnel Economics. His encouragement and advice were invaluable contributors to both my research and my overall experience as a graduate student at Cornell. I would also like to thank my other two committee members, John Abowd and Kevin Hallock, for their roles in advising my work. John Abowd also provided my research support.

The second chapter of my dissertation is coauthored with Naci Mocan, who is currently a Professor at Louisiana State University. He was also my advisor at the University of Colorado at Denver where he held a previous position while I was working on my Master's degree. He has been my *Hocam* for a long time now, and a person from which I have learned far more than just economics. He became a dear friend and now a colleague at LSU. I cannot thank him enough for everything his has done over the years.

The third chapter of the dissertation is coauthored with John Abowd, Bryce Stephens and Lars Villhuber and I am grateful for their contributions.

Two of my graduate student colleagues also deserve recognition. Thanks to Andrew Dillon and Ian Schmutte for all the great conversations about the economics of this and that, as well as the memorable times outside of work. I would not have survived living in Ithaca without the two of them. Of course, I would also like to thank my family for their encouragement and support.

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CHAPTER 1

LATERAL MOBILITY ON THE CAREER PATH WITHIN FIRMS

1.1 Introduction

The extent and importance of lateral mobility within firms is not well-established in economics. This is in part due to a lack of appropriate data sets to address the issue and in part due to the lack of existing theoretical models to motivate researchers to ask such questions. While some evidence on lateral mobility does exist in the empirical literature, the lateral transition rates found vary substantially across studies: e.g., 1.6% in Baker, Gibbs and Holmstrom (1994a and 1994b), 14% in Gibbs and Hendricks (2004), and up to 65% (not including exits) found in the firm studied by Dohmen et al. (2004). Furthermore, the few studies that uncover lateral mobility in the data are based on single-firm analyses and therefore the representativeness of their results is unclear. One advantage my paper has over previous work is that I use a panel data set of approximately 48,000 white-collar workers and 500 firms over the eight-year period 1981-1988. The surveyed firms cover a broad spectrum of industries and vary in terms of size, profits and other characteristics. As a comparison to the papers cited above, conditional on remaining in the same firm, the average annual lateral mobility rates using this more heterogeneous sample is about 35% under generous definitions of lateral mobility, or as low as 11.4% under more stringent requirements.

In this paper, I describe the hierarchy, compensation structure, and mobility patterns of white-collar workers over a broad set of firms. I find relatively high rates of lateral mobility in this large sample of firms, that this mobility is statistically different from other transitions in a number of ways, and that the compensation growth associated with lateral mobility is economically meaningful. I motivate the main analysis by considering the firm's job assignment problem in an empirical setting, and highlight the role of transition costs associated with changing jobs. Components of job- and task-specific capital could be important determinants of job mobility and worker outcomes (Gibbons and Waldman, 2004 and 2006), and should affect the probability of promotion, demotion or a lateral move differently. For example, a worker's seniority in the firm may make her more productive in the current job, but many years of seniority could also increase the productivity costs of changing jobs if seniority also captures the effects of job tenure and task-specific capital, both of which may be lost or under-utilized in a new position. Seniority may also make changing jobs less costly, since seniority can involve at least some knowledge of many positions in the firm even if the worker has not previously worked in those jobs. The firm's decision to retain the worker in the same job, promote, demote, or laterally move her to another position will be affected by which of these factors dominate. I show how these effects that determine lateral mobility are different from those that determine promotions, demotions or staying in the same job.

I also investigate the relationships between worker performance, compensation growth and the various mobility types. While data on relative or absolute performance are not available, I construct two measures of relative performance using bonus data that are unique to this paper. In standard statistical models of earnings growth, a positive and significant coefficient on a promotion or lateral move variable indicates that there are positive earnings associated with that transition, controlling for the other factors in the model. In other words, wages are, to at least some degree, attached to jobs. However, if worker performance is the mechanism sorting workers into jobs to begin with, then that positive coefficient may reflect productivity differences rather than differences in the wages between the two jobs. I show that while worker performance has a strong, positive relationship to real earnings growth, the relationships between the transitions and real earnings growth remain when including measures of relative performance. That is, even when controlling for productivity differences, significant earnings growth occurs directly through the change in jobs. The result is that, even though the real earnings growth associated with lateral transitions is only 20% to 36% the size of the change for promotions, it is 1.6% to 3.0% greater than for those who stay in the same job. Alternatively, demotions involve a 4.5% decrease in real earnings growth. This result reinforces the notion that jobs within reporting levels are heterogeneous, and that the observed wage variation within reporting levels is in part due to the fact that the jobs within those levels are simply different.

The job-to-job mobility analysis suggests that workers in these data are highly mobile - that is, job changes in the past are strong predictors of job changes in the future. Specifically, there is no real serial correlation for demotions and promotions, but lateral transitions with a job title change do predict future lateral transitions with a job title change (similar to job rotation). A lateral transition that does not involved a job title change (a simple transfer) is the least likely transition to be followed by another job change of any kind.

I conclude the empirical work by investigating the determinants of lateral mobility and other job transition rates at the firm-level. Analyses using single-firm case studies are unable to exploit the kind of firm-level heterogeneity that I have with these data. While the explanatory power of the models is modest, there are firm-specific factors that impact the rate of lateral mobility such as the average seniority and education distribution of the workforce, and whether the firm is vertically or horizontally organized. These results provide some evidence that the lateral mobility observed in these data may be the result of job rotation. Most likely, there are firm-specific and unobserved personnel policies that dictate these mobility rates such as a policy that, conditional on the firm and workforce characteristics, maps the firm's turnover rate into a decision to hire from the outside (low intra-firm mobility rates) or fill vacancies from within (high intra-firm mobility rates).

Another unique aspect of this study is the manner in which transitions are identified. Each firm details its own organizational and hierarchical structure in a way that is consistently reported across firms. The amount of detail allows for the identification of up to eighteen different mobility types depending on the worker's location in the hierarchy. This information eliminates the need to empirically define each firm-specific hierarchy through transition matrices or the wage structure, and allows for the data to be meaningfully compared across firms. The interesting aspect of the way firms report their hierarchical structure is that there is more detail available than simply defining a single file job ladder through reporting levels as in previous work. In addition to the worker's reporting level in the firm, I also know the business unit (profit center) in which the worker is employed and, therefore, the hierarchies I incorporate are more complex than those used in other studies.

1.2 Literature Review

In a pair of seminal papers using the personnel records of management employees in a large US firm, Baker, Gibbs and Holmstrom (1994a, 1994b) found that lateral mobility was virtually nonexistent in the single firm they investigate.[1] This is important for two reasons. The first is that the their findings have spawned a growing literature that aims to develop theoretical models consistent with their empirical results (e.g. Gibbons and Waldman, 1999; Gibbons and Waldman, 2006), and secondly, there have since been a number of studies that look for (in)consistencies between other individual firms and Baker, Gibbs and Holmstrom (1994a, 1994b) (e.g., Lima, 2000; Seltzer and Merrett, 2000; Treble et al., 2001; Dohmen et al., 2004; Gibbs and

[1] See Table 1 of Baker, Gibbs and Holmstrom (1994a).

Hendricks, 2004; Lin, 2005). Few papers make any notable mention of lateral mobility within firms, the exceptions being Dohmen et al. (2004), Gibbs and Hendricks (2004), and more recently Acosta (2006).

One form of lateral mobility often cited as a potentially important progressive human resource practice is job rotation.[2] Several establishment-level surveys find that the level of job rotation among US firms is nontrivial and that firms are adopting job rotation at an increasing rate. For example, Gittleman et al. (1998) use the BLS Survey of Employer Provided Training and find that in 1993 about 13 percent of private sector establishments of any size and 24 percent of establishments with 50 or more employees integrated job rotation as a regular practice. Similarly, the Educational Quality of Workforce Survey in 1994 estimated that of private sector establishments with 20 or more employees, 18 percent of non-managerial workers were involved in job rotation nationally.[3] Also employing a nationally representative sampling frame, Osterman (1994) conducted a survey revealing that 27 percent of private sector establishments with 50 or more employees rotated at least half of their core employees in 1992, but the proportion of establishments rotating its workforce jumps to 43 percent if the penetration requirement is relaxed to any number of core employees.[4,5] Osterman repeats the analysis using 1997 data (Osterman, 2000) and finds that the proportion rotating at least half of its core employees doubled to more than 55 percent.

[2] I discuss the relevance of job rotation to lateral mobility below in more detail. For references on the determinants of the adoption of job rotation at the firm-level see Eriksson and Ortega (2006) and Eriksson (2000).

[3] This was a nationally representative survey of private sector establishments conducted by the US Census Bureau in conjunction with the University of Pennsylvania's Center on the Educational Quality of the Workforce.

[4] The author reports that restricting the sampling frame to those establishments with 50 or more employees results in only 10 percent of all establishments nationally, yet those same establishments employ 51 percent of non-agricultural US employment

[5] Core employees were defined as "the largest group of non-supervisory, non-managerial workers at this location who are directly involved in making the product or in providing the service at your location" [p.175]. The core group could be either blue or white collar workers.

Given the frequency with which firms rotate their workers suggested by these surveys, it is surprising that substantial lateral mobility is not widely found in personnel datasets used in economics. The establishment-level research by Osterman, Gittleman and others suggests that job rotation is becoming a prevalent human resource practice among firms in the US economy. Since job rotation is only a particular form of lateral mobility, one should expect to observe lateral transitions in many personnel datasets, yet the existing literature in economics shows mobility patterns in firms that are not always consistent with these surveys.[6] For example, the firm analyzed by Baker, Gibbs and Holmstrom (1994a, 1994b) exhibited almost no lateral mobility (1.6% of all transitions including promotions, demotions, lateral moves and exits) and demotions are even rarer.[7] One potential explanation for the lack of lateral transitions in their data is that even though the surveys mentioned above reflect blue and white-collar workers, the questions are intended to refer to the non-management portion of the workforce; whereas the personnel records available to Baker, Gibbs and Holmstrom (1994a, 1994b) are of management employees in the firm. However, the data employed by Acosta (2006) and Gibbs and Hendricks (2004) are comprised of the personnel records for both managerial and clerical workers and exhibit substantial lateral mobility (about 14 percent of transitions when including exits as an outcome and about 32 percent of transitions conditional on the move being a promotion, demotion or lateral move).[8,9] On the other extreme, the personnel records analyzed by Dohmen et al. (2004) cover both production and managerial

[6] To identify estimated mobility rates that are consistent across papers it was sometimes necessary to calculate these numbers from various pieces of information in the papers' text. I note those instances where the stated mobility rates are not presented directly by the authors.
[7] Page 931 of Baker, Gibbs and Holmstrom (1994b).
[8] The 14 percent is estimated from the bottom panel of their Table 3 on p.79. In an attempt to be consistent with the lateral move rate of 1.6% in Baker, Gibbs and Holmstrom (1994a, 1994b), a transition is defined as a demotion (1.5%), promotion (28.9%), lateral move (14.1%) or exit (55.5%). Conditional on remaining with the firm, lateral moves therefore represent 32% of transitions.
[9] Blue-collar (hourly) workers are not included in the analysis.

employees and yield perhaps the largest lateral mobility rates in the literature (65 percent of all transitions including promotions, demotions and lateral moves but not exits).[10]

1.2.1 Theoretical Rationale for Lateral Mobility within Firms

There is little economic theory that incorporates lateral mobility as an important facet of careers. An exception is Demougin and Siow (1994) where unskilled workers may move laterally if they fail an on-the-job training program. Due to the prospects of larger future earnings associated with successful training, young unskilled workers who enter the training program accept a smaller wage than those who are not trained. Unsuccessful trainees who then move laterally experience a pay increase because the wage paid to trainees is smaller than the wage earned by the unskilled workforce. However, the pay increase is smaller than for those who successfully complete the training and are promoted. In this model, lateral mobility occurs only in firms with fast-track promotion regimes and not firms that practice up-or-out policies.

There are other economic explanations why lateral mobility may exist. Gibbons and Waldman (2006) discuss the notion of task-specific capital and point out the way in which jobs are sequenced is important when considering the job ladder. When changing jobs, some task-specific capital may go unutilized in the new job and therefore standard career paths should be structured to minimize this loss. Depending

[10] The 65 percent is also calculated via information found in the text: they observe 5,704 promotions and 1,627 demotions (p.201) and 13,636 lateral transitions (p.206). Unfortunately, I was not able to determine the precise number of exits in their data to calculate a lateral mobility rate that is comparable to Baker, Gibbs and Holmstrom (1994a, 1994b). The authors acknowledge that they may be overestimating the number of lateral moves: about 25% of the total number of lateral transitions involve a change in job code and different field of activity while the remaining 75% change jobs but stay in the same hierarchical level and field of activity. I explore the differences between these two types of lateral moves in the empirical sections.

on the level of specialization of jobs within a firm, this concept gives rise to the possibility that it may be optimal to train workers as generalists rather than specialists by having them gain experience in a variety of positions before promotion.

It is hypothesized that firms choose to incorporate job rotation as a human resource practice for one of three reasons: (i) employee learning, (ii) employer learning or (iii) employee motivation.[11] Regardless of the reason for adoption, this form of job mobility is considered to be distinct from a promotion although employee learning and employer learning may require rotation as a prerequisite to promotion. Ortega (2001) notes two empirical regularities concerning the adoption of job rotation: job rotation is negatively correlated with average tenure at the firm and positively correlated with the use of new technologies. The negative relationship with tenure is consistent with both employee and employer learning but not motivation. The idea behind the model by Ortega (2001) is that when a firm observes a worker in a single job it observes only one (but reliable) signal of the match quality whereas if the worker is rotated among jobs the firm observes more information. In this way there is a tradeoff between the quality of the signal and the number of signals. A primary result of the model is that the benefits of job rotation are more pronounced when there is greater uncertainty about worker abilities and job profitability. Therefore, firms rotate young or new workers more often those with more experience and innovative firms with evolving job components or users of new technology are more likely to rotate than firms that continue to use existing technology.

[11] See Table 2 of Eriksson and Ortega (2004) for a review of the findings on why firms adopt job rotation.

1.3 Data and Descriptive Analysis

The data used in this paper consist of a yearly panel of approximately 500 firms and 48,000 workers over the eight-year period 1981-1988.[12] These data were collected by a consulting firm through surveys that asked a variety of questions about the firm's compensation policies while gathering detailed information on workers' pay and individual characteristics. While participation was voluntary, each firm also paid a fee to the consulting company in order submit its personnel data - the return being a comparative analysis of the firm's compensation policies. The fact that the survey required a participation fee and resulted in a consultation report means that participating firms had incentives to report reliable data.

The panel is relatively short, with a maximum spell of employment of eight years, but unlike many single-firm data sets, the personnel records for every worker employed at the firm are not available. Instead, each respondent (the firm) was asked to report information on 75 or more employees, which tend to be upper-level management and not lower-level workers. The sample is not nationally representative, but the firms do cover a broad spectrum of industries and vary in terms of size, ownership type, and other economic characteristics such as sales and profits.

Substantial information is included about each worker such as base and bonus pay, age, education, year of hire, reporting level, the business unit for which the employee works, characteristics of the position, including eligibility for various compensation plans, board membership, and the total number of employees supervised.[13] Firm-level information on profits, sales, total employment, industry,

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[12] These data have also been used in other contexts by Abowd (1990), Leonard (1990) and Belzil and Bognanno (2004, 2005).

[13] The total number of employees supervised includes all employees beneath the worker in that particular profit center, not just those under direct supervision.

organizational type (public, private, subsidiary, US or foreign owned) and variables describing the firm's compensation policies are also merged with the worker data.

The data are heavily concentrated in manufacturing, capturing more than 70 percent of firm-year observations. The second largest industry group is Transportation and Utilities (9.6%) followed by Services (5.4%) and Finance, Insurance and Real Estate (5.1%). Most firms are US owned and publicly traded (64%) but there are privately owned parent companies (10.3%) as well as public and private subsidiaries (11.2 and 2.2 percent, respectively). About 12 percent of the firms are foreign owned.

Overall, these firms are large in terms of employment and other economic characteristics, but there exists substantial heterogeneity in terms of growth and profitability over the sample period. The median firm employs a total 12,000 workers, but the average firm is much larger (26,000 workers). Similarly, in 1980 dollars, the median (average) firm had \$1.2 (\$4.6) billion in sales and \$44 (\$248) million in profits.[14] Interestingly, while the median firm remained relatively stable in terms of employment levels, 25 percent of the firms experienced employment reductions of at least 21 percent while another 25 percent of firms grew by 20 percent or more. Most firms grew in terms of both sales and profits but there were losers as well. Twenty-five percent of the firms experienced non-negligible declines in profits or sales.

Table 1.1 displays the sampling characteristics of the data. Repeated observations on workers is essential for analyzing job transitions: 70 percent of the firms appear at least twice while 35% of the firms appear for five or more years. Panel B shows that companies appearing for longer spells also tended to report information on more workers. While some firms reported data on a relatively small number

[14] Profits are defined as net income including all after-tax earnings. Sales include service and rental revenues but exclude non-operating revenues and any excise taxes collected by manufacturers.

Table 1.1
Sampling Characteristics of the Data

Panel A: Distribution of Companies By Spell in the Data

Years Observed in the Data	Frequency	Percent
1	235	29.5
2	141	17.7
3	91	11.4
4	47	5.9
5	60	7.5
6	53	6.7
7	47	5.9
8	122	15.3

Panel B: Distribution of the Average Number of Executives Reported per Company By Length of Company Spell

# Years Company is in Data	Executives Reported per Year	Std. Dev.	<1%	5^{th} %	10^{th} %	25^{th} %	50^{th} %	75^{th} %	90^{th} %	95^{th} %	>99%
1	54.3	45.1	1	6	7	19	46	77	107	132	213
2	64.8	59.0	7	8	14	31	54	81	122	148	258
3	66.9	54.2	6	16	27	39	62	81	96	108	459
4	81.2	83.4	16	19	26	40	62	91	116	213	475
5	77.1	33.0	13	30	40	56	74	93	124	138	193
6	83.9	45.9	19	34	42	58	78	93	130	168	304
7	88.7	50.3	15	38	42	56	76	100	169	226	239
8	87.4	42.9	13	31	41	61	82	107	140	175	209

Note: Each Observation is the average number of executives reported over the spell for a single company.

workers, others report information on several hundred. Typically, firms appearing for at least five years provided data on 74 to 82 workers per year.

1.3.1 The Hierarchy

An advantage of the data I use is that they contain detailed information about the organizational structure which is consistently reported across firms. As a result, no additional information is needed to identify the hierarchy by using empirical instruments such as job transition matrices or wage deciles. The availability of this information is a crucial aspect of the analysis as it allows for the identification of a large number of sophisticated mobility patterns including promotions, demotions, stayers, and several potentially different forms of lateral mobility. Note that the data do not include a variable specifically labeling every job transition as a promotion or demotion, as in Gibbs and Hendricks (2004); instead, the data provide a blueprint of the firm in which I am able to identify movements within and between sections.[1]

Firms report three pieces of information about every worker that identifies a location and position in the organizational structure: the worker's *reporting level*, *organizational unit level* and a *job code* identifier, which I summarize as $(RL_{ijt}, UNIT_{ijt}, JC_{ijt})$. These three variables allow me to completely identify job transitions within the firm and are defined as follows:

- Reporting Level: The reporting level is the number of levels away from the Board of Directors. All positions reporting directly to the CEO are in reporting level 2; all positions reporting directly to those positions are in reporting level 3, and so on.

[1] While Gibbs and Hendricks (2004) know the transition type, they do not have information about reporting levels and the overall hierarchy. On the other hand, the Portuguese data used by Lima (2000) include a variable that indicates the date of last promotion and a level assigned to the worker.

- Organizational Unit Level: The organizational unit level counts the number of major organizational units between the Board of Directors and the worker's organizational unit. An organizational unit is a company, group, division, sales region, or manufacturing facility that the company counts as a separate profit center. Executives with responsibilities that span the entire corporation are considered corporate positions and are a unit level 1. In an organization where a division manager reports to a group executive who reports to a company president and COO, the division manager is unit level 3, the group executive is unit level 2 and the president and COO is unit level 1.

- Job Code: A numeric code that best describes a worker's responsibilities.[2]

The meaning of the job code identifier is straightforward and the reporting level is how one naturally thinks of slicing a hierarchy into a single file job ladder. The organizational unit level deserves some discussion since the additional dimension of detail allows us to partition each firm's hierarchy into potentially interconnected branches and identify more complex transitions. Essentially, a change in a worker's organizational unit often implies a change in workplace location but it can also represent a change in the worker's responsibility. Consistency within this structure implies that an executive's organizational unit should be weakly higher (closer to the CEO) than her reporting level; that is, reporting levels can overlap across organizational units, but it is unlikely to observe a manager that has an organizational unit further from the CEO than her subordinates. Table 1.2 displays how

[2] More generally we would consider an additional state variable that distinguishes new hires, incumbents and exits, but the data do not provide adequate information to properly identify new hires and exits.

organizational units map into reporting levels. The cells below the diagonal are sparsely populated, which provides assurances that the hierarchies are consistently reported across firms. The table also confirms that several reporting levels exist within each organizational unit.

Note that with information on organizational units, the hierarchy is no longer a one dimensional ladder; instead, the hierarchy more naturally reflects the complexities of an organization's structure and incorporates possible career paths. This additional information offers an opportunity to distinguish among a number of different types of transitions that would otherwise be undetectable or appear homogenous in other data sets. For example, a lateral move within a reporting level in the same organizational unit might be different than a lateral move within the same reporting level but to a different office in another organizational unit.

Stayers and movers are identified in each period by changes in one or any of the three position variables. Therefore, $(\Delta RL_{ijt+1}, \Delta UNIT_{ijt+1}, \Delta JC_{ijt+1})$ contains all the information needed to distinguish the different possible transitions between periods t and t+1. Let ΔRL^{+}_{ijt+1} denote a rise in reporting level closer to the CEO, ΔRL^{-}_{ijt+1} a fall in reporting level away from the CEO and ΔRL^{0}_{ijt+1} as no change in reporting level. Let $\Delta UNIT^{+}_{ijt+1}$, $\Delta UNIT^{-}_{ijt+1}$, and $\Delta UNIT^{0}_{ijt+1}$ be denoted in the same way. We cannot determine from ΔJC_{ijt+1} alone whether or not it represents a rise or fall in the hierarchy and so denote ΔJC^{1}_{ijt+1} as a change in job title and ΔJC^{0}_{ijt+1} as no change in job title.

This framework yields 18 different mobility types including workers who stay in the same job (3×3×2 possibilities). This is not only difficult to analyze in a tractable manner but presents substantial ambiguity concerning the interpretation and meaningfulness of each type of transition.

Table 1.2
How Does a Company's Unit Level Map Into Reporting Level?

	Reporting Level of Company						
	1	2	3	4	5	6	7 or Below
Unit Level of Company							
1	3.59	14.3	31.53	31.86	14.19	3.63	0.9
2	0.01	7.65	27.08	34.31	20.81	7.73	2.41
3	0	0.77	12.84	34.22	31.58	14.86	5.73
4	0	0.05	1.79	17.04	34.27	28.09	18.76
5	0	0.05	2.37	9.19	27.92	36.2	24.28
6	0	0	0	0	3.29	31.09	65.62
7 or Below	0.14	0.58	1.58	2.3	1.44	0.14	93.81
Total Observations:	2,426	13,405	41,068	60,965	47,075	23,939	12,269

Note: Each observation is an executive-year. The cells represent percentages and the rows sum to 100. The table shows that multiple reporting levels exist within each unit level. Consistency in the reporting of worker positions requires unit levels to be weakly closer to the top of the hierarchy than reporting levels. Therefore, cells in the table should be mostly populated in the upper triangular matrix represented by the shaded area.

Therefore, to collapse the 18 different transitions I utilize two assumptions to classify promotions, demotions, lateral moves and those who are retained in the same job.

1. A change in reporting level is a more valuable (but not exclusive) identifier to sort promotions, demotions and lateral moves than a change in organizational unit level.
2. A change in job title signals a significant change in position.

Accepting these two statements allows me to collapse the 18 mobility types into 8 transitions (and another labeled *stayer*) by what amounts to collapsing some of the movements across unit levels and putting more (but not all) emphasis on changes in job titles and reporting levels. These transition types, the labels I give them and a description of each transition are displayed in the Appendix as Table APP1.[1] *Promotions* are those movements up the reporting level that accompany a change in job title, regardless of the worker's organizational unit. *Demotions* are movements with a change in job title and a move down the reporting level, but can involve any change in unit level as well. I label four transitions as potentially different forms of lateral mobility: *Lateral-strict*, *Lateral-up*, *Lateral-down* and *Lateral-same job*. The common component here is that they are all transitions within the same reporting level. Additionally, each of first three involves a change in job title but the difference lies with the new position's organizational unit: *Lateral-strict* involves no change in unit level, *Lateral-up* a rise in unit level closer to headquarters and *Lateral-down* a fall in unit level. *Lateral-same job* is a transition within a reporting level that constitutes a change in unit level but no change in job title. *Lateral-same job* could be interpreted literally as a transfer to another office with the same duties. *Level-up same job* is a transition up the reporting level without a change in job title but can involve any

[1]It is possible for changes in reporting or unit levels to span more than a single level. I do not distinguish between such multi-level moves in this paper.

change in unit level. *Level-down* same job is similar to *Level-up* same job except the difference is a fall in reporting level. Finally, *Stayers* are those workers who exhibit no change in reporting level or unit level nor do they change job titles.

Table 1.3 presents the overall distribution of the nine mobility types by reporting level. Workers defined as Stayer represent 62.3 percent. Considering only position changes, promotions account for 8.8% of transitions, demotions 5.6%, and lateral transitions involving a job title change, as a group, represent 16.5%. Lateral moves without a job title change account for 18.4% of job transitions. Transitions involving no change in job title but cross levels either up or down (Level-down same job and Level-up same job) account for a large fraction of transitions at 50.5%.

1.3.2 A Descriptive Analysis of Job Mobility and Pay

The relationships between worker salaries and job mobility shed important insights concerning how these transitions are similar and different. Table 4 displays the distribution of the growth in nominal base pay associated with each type of transition. Some interesting patterns emerge from this table. Those transitions labeled Promotion stand out with a median promotion yielding a 15% increase in base pay. The median increase in base pay for Lateral-same job, Level-up-same job, Level down-same job, Stay and Demotions is roughly the same around 7.5%, but the median increase for Lateral-strict, Lateral-up and Lateral-down are all slightly higher at 9 to 10 percent. Consistent with previous findings, declines in nominal wages are rare; however, 10% of demotions involve nominal pay declines of 18% or more. Although the average percentage change is positive at 5.6%, the average monetary change in base pay for demotions is -$1,200. There are also reductions in base pay for all transitions (including promotions) at the first percentile or less. To the extent that changes in base pay are good metrics for sorting these transitions, Table 1.4 suggests

Table 1.3
Distribution of Job Transitions by Reporting Level

	Promote	Demote	Lateral - Strict	Lateral - Up	Lateral - Down	Lateral - Same Job	Level Up - Same Job	Level Dn - Same Job	Stayer	N
Reporting Level										
1	7.78	0.00	2.11	0.00	0.00	0.00	0.00	0.00	90.11	1,659
2	6.52	0.16	6.04	1.71	1.33	1.36	10.45	0.03	72.39	8,591
3	5.08	0.84	5.01	1.02	0.99	4.14	12.56	2.16	68.21	24,951
4	3.06	1.93	4.78	1.07	1.06	6.95	11.06	6.69	63.40	34,821
5	1.96	3.22	3.75	0.96	0.85	9.06	9.19	13.65	57.36	25,334
6	1.48	3.94	2.82	1.00	0.70	11.60	7.11	18.98	52.37	11,651
7 or Below	0.83	4.02	1.96	0.32	0.36	10.62	5.60	27.46	48.82	5,553
Total Obs:	3,738	2,392	4,856	1,153	1,049	7,805	11,355	10,065	70,147	112,560
% of Total Obs	3.3	2.1	4.3	1.0	0.9	6.9	10.1	8.9	62.3	100.0
Percent of Non-stayer Transitions:	8.8	5.6	11.4	2.7	2.4	18.4	26.8	23.7	100.0	--

Note: Row percentages sum to 100. All but one of the Lateral-Strict transitions in reporting level 1 are between titles “CEO” and “Chairman of the Board (not CEO

Table 1.4
Percentage Changes in Nominal Base Pay by Transition Type

Mobility	Mean	Obs	<1%	5^{th} %	10^{th} %	25^{th} %	50^{th} %	75^{th} %	90^{th} %	95^{th} %	>99%
Promote	22.38	3,738	-25.00	0.00	0.30	7.69	14.94	25.93	44.00	66.67	171.54
Demote	5.57	2,391	-70.00	-43.47	-18.03	0.00	7.74	13.80	23.20	32.02	66.67
Lateral - Strict	10.67	4,856	-16.49	0.00	0.00	5.24	9.00	14.41	22.38	29.03	50.91
Lateral - Up	11.71	1,153	-43.01	-2.04	0.00	4.94	9.76	17.71	28.80	38.89	68.60
Lateral - Down	13.14	1,049	-39.13	-4.14	0.00	5.00	10.00	18.42	29.87	42.86	96.67
Lateral - Same Job	7.76	7,805	0.00	0.00	0.00	4.01	7.11	10.14	15.00	19.65	31.71
Level Up - Same Job	8.94	11,355	-0.28	0.00	0.00	5.00	7.81	11.24	17.38	23.15	38.89
Level Down - Same Job	8.34	10,057	-1.31	0.00	0.00	5.00	7.57	10.71	15.10	20.21	36.14
Stayer	7.88	70,135	-0.02	0.00	0.00	4.49	7.32	10.47	15.00	19.44	31.58

Note: Percent differences are relative to previous year's earnings. Example: The transition *Promote* is related to an average increase in base pay of 22.38%.

that promotions and demotions are entirely different from the other transitions but the distributions for Lateral-same job, Level-up-same job and Level down-same job appear relatively the same.

Baker, Gibbs and Holmstrom (1994a) showed that promoted workers tended to come from the top end of the reporting level's pay distribution before promotion and enter the lower end in the new level, but promoted workers exit and enter all parts of the old and new levels' pay distribution. Gibbs and Hendricks (2004) performed a similar analysis that included demotions and lateral transitions. They report that promoted workers entered the lower end of the new salary range, demoted workers were more often moved into the higher end of the new salary range, and lateral transitions resulted in a new salary levels somewhere in the middle. To investigate whether these same patterns exist in my data, I performed a similar analysis and present the results in Tables 1.5 and 1.6. For each firm, I calculated the firm-specific distributional ranking of real base pay at the 10, 25, 50, 75, 90 and 90th percentiles for every reporting level (across years) and assigned workers to the relevant position in the firm-level-specific salary distributions. For those transitions which did not involve a change in reporting level, the new and old reporting level percentiles are the same and therefore a change in ranking occurs because of an increase in pay within that level. For example, in panel A only 5.78% of promoted workers had a salary in the new reporting level at the 90th percentile or higher, and in panel B, 25.44% of promotions exited the top decile of the former pay distribution.

The most obvious pattern is that of demotions - demotions come from the bottom part of their former salary distribution (34.3% from the bottom quartile) and enter the top in the new level (49.6% into the upper quartile). Promotions stem heavily from the upper part of the pay distribution (46.7% from the upper quartile) but salaries are more evenly distributed in the new level. The lateral transitions involving a change in job

Table 1.5
Location in Reporting Level Base Pay Distribution by Transition Type

Mobility	Location in Reporting Level's Pay Distribution *After* Transition						
	< 10th Percentile	10-25th Percentile	25-50th Percentile	50-75th Percentile	75-90th Percentile	>90th Percentile	N
Promote	10.14	14.24	25.64	28.83	15.36	5.78	3,738
Demote	4.11	7.75	15.38	23.13	22.55	27.07	2,391
Lateral - Strict	4.20	10.18	21.11	27.54	21.03	15.94	4,856
Lateral - Up	3.12	5.46	16.13	26.11	26.97	22.20	1,153
Lateral - Down	4.10	6.67	20.11	30.70	23.83	14.59	1,049
Lateral - Same Job	6.43	11.17	22.79	27.19	18.86	13.56	7,805
Level Up - Same Job	13.46	18.65	28.14	24.45	10.96	4.34	11,355
Level Down - Same Job	5.09	8.67	19.04	25.40	19.88	21.92	10,057
Stayer	7.52	13.53	25.08	26.69	16.27	10.91	70,135

Note: The percentiles are calculated individually for each firm and for every level (the percentiles are not year specific). Base Pay is in real 1980 dollars.

Table 1.6
Location in Reporting Level Base Pay Distribution by Transition Type

	Location in Reporting Level Pay Distribution *Before* Transition						
Mobility	< 10^{th} Percentile	10-25^{th} Percentile	25-50^{th} Percentile	50-75^{th} Percentile	75-90^{th} Percentile	>90^{th} Percentile	N
Promote	4.98	7.46	16.91	23.92	21.29	25.44	3,738
Demote	14.67	19.65	26.46	24.54	10.20	4.47	2,391
Lateral - Strict	8.82	12.94	23.85	26.47	18.00	9.93	4,856
Lateral – Up	4.94	8.15	20.99	31.05	20.90	13.96	1,153
Lateral - Down	6.96	12.77	24.31	28.98	17.54	9.44	1,049
Lateral - Same Job	9.51	14.08	23.63	26.49	15.70	10.59	7,805
Level Up - Same Job	6.21	9.45	19.16	24.54	19.30	21.34	11,355
Level Down - Same Job	17.74	21.07	29.23	20.85	7.98	3.14	10,057
Stayer	11.33	16.02	26.00	24.92	13.82	7.91	70,135

Note: The percentiles are calculated individually for each firm and for every level (the percentiles are not year specific). Base Pay is in real 1980 dollars.

title (Lateral-strict, Lateral-up and Lateral-down) are more evenly spread in both the new and old pay distributions but occur less often from the bottom quartile. Lateral-same job exhibits patterns very similar to those who Stay. The patterns for Level-up-same job and Level down-same job appear similar to promotions and demotions, respectively, but this result should be observed with caution.

The descriptive analysis presented above provides hints that some of these transitions are more similar than others. The relationships between job mobility and outcomes such as salary growth and the number of employees supervised offer somewhat conflicting evidence as to how the various lateral transitions are related. The one point that seems to be clear is that a change in job title matters. Lateral transitions involving a job change result in larger salary increases accompanied by greater supervisory responsibilities than lateral transitions which do not. A worker's rank in reporting level is also important. Job changes that cross reporting levels (promotions and demotions) are different from job changes within reporting levels. I explore these connections in greater detail later in the paper with a focus on statistically identifying meaningful differences and similarities between these transitions.

1.4 Motivational Framework

To fix ideas and motivate the investigation of the relative importance of lateral mobility to other mobility outcomes, I expand on a framework similar to Kwon and Milgrom (2004) that considers the firm's assignment problem in an empirical setting. Suppose that in each period t, there is an outcome of interest to the firm, $Y_{it}(k_{it})$, for every worker i under job assignment k_{it}. For now, assume that $Y_{it}(k_{it})$ is the expected productivity of worker i in period t under job assignment k_{it}, and that the worker's expected productivity net of transition costs, $TC_{it}(k_{it},k_{it-1})$ is given[1] by

$$Y_{it}(k_{it}) = \eta^k + \varphi_g{}^k EXP_{it} + \varphi_f{}^k SEN_{it} + \gamma^k \Delta SPAN_{it}(k_{it},k_{it-1}) + \alpha^k SPAN_{it-1}(k_{it-1}) + X_{it}\beta^k - TC_{it}(k_{it},k_{it-1}) + \varepsilon_{it}(k_{it}) \quad (1.1)$$

where k_{it} is an assignment choice for worker i in period t: promote (k_{it} =P); demote (k_{it} =D); lateral move (k_{it} =L for lateral moves with a change in job title and k_{it} =L' for lateral moves without a job title change); or retain in the same job (k_{it} =S).[2] EXP_{it} measures the general labor market experience of worker i in period t. SEN_{it} measures the seniority of worker i in period t as years of tenure; $SPAN_{it-1}(k_{it-1})$ is the span of control for worker i at the end of the previous period $t-1$, measured by the number of subordinates; $\Delta SPAN_{it}(k_{it},k_{it-1})$ is change in the span of control of worker i under assignment choice k_{it}; X_{it} is a vector of worker and firm characteristics associated with worker i in period t; $TC_{it}(k_{it},k_{it-1})$ is the transition cost associated with worker i under assignment choice k_{it}; and $\varepsilon_{it}(k_{it})$ is a stochastic disturbance, interpreted as a matching component for worker i under job assignment k_{it}.

In this framework, the transition costs are a key component to the firm's assignment decision. While $TC_{it}(k_{it},k_{it-1})$ is not observable to the econometrician, it is likely a function of many of the same observed components that determine productivity. For example, consider the decision to reassign a worker from job A to

[1] The vector X contains the following controls: year indicators; firm, industry, and ownership indicators; firm size, sales and profits; worker education and reporting level in the firm; and indicators for whether the worker in the new position is bonus eligible, eligible for long-term incentive plans or is on the board of directors.

[2] To simplify the problem conceptually and computationally, the assignment choice "Lateral w/ Job Title Change" aggregates the following mobility types found in appendix table APP1: Lateral-Strict, Lateral-Up, and Lateral-Down. The assignment choice "Lateral w/o Job Title Change" aggregates the following mobility types: Lateral-Same Job, Level-Up Same Job, Level-Dn Same Job. Given the descriptive results in the previous section, this is a reasonable aggregation and is consistent with the two assumptions that collapsed the original 18 mobility types to 9 transitions.

job B. Regardless of whether or not a worker has ever worked in job B, if the worker has many years of seniority at the firm she is likely to have at least some knowledge about job B, making the job transition less costly than it would be for a worker with less seniority. On the other hand, a long tenure in job A would result in the loss or under-utilization of job-specific capital after reassignment to job B. As another example, the degree to which the tasks performed in the two jobs are similar should determine the transition costs.

Kwon and Milgrom (2004) model the transition costs as a function of firm and/or occupation tenure but do not account for the change in the wage bill associated with job assignments, such as promotion. In an environment where firms optimally assign workers to jobs that maximize expected productivity, this may be appropriate. However, since it is standard to assume the firm's goal is to maximize profits it is necessary to discuss the wage bill associated with job re-assignments. In the absence of incentive issues related to internal labor markets and firm-specific productivity effects (i.e., when the effect of SEN_{it} captures only job- or task-specific skills that are transferable), then the equilibrium wage offer is equal to the worker's marginal product of labor (Lazear and Oyer, 2007). In the framework above, productivity is a direct function of the job assignment, k_{it}, and so this means that the equilibrium offer is an assignment-wage pair (k_{it},W_{it}). Including the change in the wage bill as part of the transition costs may only require a reinterpretation of $Y_{it}(k_{it})$ as the expected profits produced by worker *i* under assignment k_{it} in period *t*.

To estimate equation (1.1) (where the firm's problem is to assign workers to jobs that maximize the expected productivity net of transition costs), I assume a setting that incorporates the following assumptions and timing. At the beginning of period *t*, workers accumulate any productivity-enhancing skills and make their resumes public, and firms with full and symmetric information about workers, with no incentive policies in place, make job-specific wage offers on an open spot market. Wages are, in essence, predetermined for any assignment option, and leaving them out of the transition costs therefore accounts only for the productivity costs of the job change. At the end of period t, firms assign workers to positions and production takes place.[3] That is, the firm chooses the assignment $k_{it}{*}$ for worker i in period t such that $Y_{it}(k_{it}{*})=max[Y_{it}(k_{it})]$ for k=P,D,L,L',S.[4]

The expected productivity is a latent variable, and a Type I extreme value distributional assumption on $\varepsilon_{it}(k_{it})$ implies a multinomial logit model where the parameter vector $\Theta^k=(\eta^k,\varphi_g^{\ k},\varphi_f^{\ k},\alpha^k,\gamma^k)$ is specific to assignment choice k.[5] Given the framework, the parameter $\varphi_g^{\ k}$ on variable EXP_{it} has the standard interpretation of the return to general human capital accumulation, but the parameter $\varphi_f^{\ k}$ requires discussion. The standard interpretation is that SEN_{it} measures firm-specific human capital accumulation of worker i in period t, but in the context of job mobility it is important to note that seniority also captures components of job tenure and task-specific capital accumulation. While these latter two components are thought to improve one's productivity, some or all of those improvements are lost or underutilized when one switches jobs. The extent to which the return to firm-specific capital accumulation does or does not dominate the loss of job/task specific components is important to the firm's decision making process. The sign of the

[3] I abstract from between-firm transitions such as dismissing workers or hiring from the outside, because the data do not provide adequate information to indicate whether a newly observed worker was recently hired or was simply reported by the firm for the first time. Similarly, the reason the worker exits the data is not known.

[4] Operationally, there is a latent index $Y_{it}(k_{it}{*})$ such that if $Y_{it}(k_{it}{*})$- $Y_{it}(j)\geq 0$ for k≠j then $Y_{it}(k_{it})$=1 and $Y_{it}(j)$=0. Therefore, $Y_{it}(k_{it})$ is a dummy variable equal to 1 if k is chosen and 0 otherwise.

[5] I do not assume or constrain the expected contribution to output, $Y_{it}(k_{it})$, to be non-negative, since the transition costs associated with some assignment options could outweigh the productive abilities of the worker. Furthermore, some assignment options may be feasible but destructive such as promoting a newly hired college graduate to CEO of a large corporation.

coefficient of SEN_{it} along with a strict interpretation of the model will provide insights to that issue.

The span of control is a measure of the worker's command over firm resources, accounting both for situations in which a worker's output is a function of the productivity of her subordinates and those in which managers are strictly coordinators.[6] Note the identity used in equation (1): $SPAN_{it}(k_{it}) = (\Delta SPAN_{it-1}(k_{it-1}) + \Delta SPAN_{it}(k_{it},k_{it-1}))$. The span of control is decomposed in this way to explicitly account for the change in the command over firm resources associated with changing jobs. The change in span of control may also be a proxy for the change in responsibility or difficulty between the positions.

Consider the following specification for the transition costs associated with changing jobs from periods *t-1* to *t*:[7]

$$TC_{it}(k_{it},k_{it-1}) = a^k + b^k SEN_{it} + c^k\, \Delta SPAN_{it}(k_{it},k_{it-1}) + \nu_{it}(k_{it}) \qquad (1.2)$$

Here the transition cost is assumed to be solely a function of seniority, the change in span of control and a random component assumed to follow a Type I extreme value distribution. While there may be other important factors related to the transition cost, this specification is sufficient to illustrate the issues underlying the standard estimation of the assignment problem when transition costs are considered. Note the parameter b^k in equation (2). As mentioned above, seniority captures both firm-specific and job-specific capital. If firm-specific capital accumulation makes job

[6] Waldman (1984) considers the case where managers are strictly coordinators and total production is simply the product of the number of laborers the manager supervises and the output per laborer (Assumption 9', p. 100). He shows (Theorem 3) that the firm's choice for N, the number of subordinates, is increasing in the ability of the manager who supervises them. Rosen (1982) builds a model with recursive production technology where higher ability managers are placed in positions that have greater influence and greater span of control.

[7] For workers who stay in the same job, I assume $TC(k)_{it}$=0.

transitions less costly, it should pull this parameter in the negative direction whereas if the loss of important job-specific components dominates we would expect $b^k > 0$. If an increase in the number of subordinates represents an increase in responsibility or in the complexity of the new position, we would expect $c^k > 0$.

Substituting equation (1.2) into (1.1) gives the specification for the latent variable, $Y_{it}(k_{it})$, that leads to the empirical model I estimate:

$$Y_{it}(k_{it}) = (\eta^k - a^k) + \varphi_g{}^k EXP_{it} + (\varphi_f{}^k - b^k)SEN_{it} + \alpha^k SPAN_{it-1}(k_{it-1}) + (\gamma^k - c^k)\Delta SPAN_{it}(k_{it}, k_{it-1}) + X_{it}\beta^k - TC_{it}(k_{it}, k_{it-1}) + (\varepsilon_{it}(k_{it}) - v_{it}(k_{it})) \tag{1.3}$$

Equation (1.3) shows that the multinomial logit parameters for the intercept, seniority and the impact of a change in span of control reflect both the returns to those variables that directly affect productivity as well as the effect of transition costs associated with changing jobs. Each equation for promotions, demotions and lateral moves produces its own set of parameter estimates, and the signs and magnitudes of these estimates shed light on the differences across the various types of job transitions. For example, suppose the estimated coefficient for SEN_{it} in the promotion equation is positive, i.e. $(\varphi_f{}^P - b^P) > 0$. Since human capital theory predicts the productivity gains to seniority ($\varphi_f{}^P$) to be positive, $(\varphi_f{}^P - b^P) > 0$ implies either that $b^P < 0$ (seniority reduces the transition cost of promotion) or that $\varphi_f{}^P > b^P > 0$ (overall productivity returns to seniority dominate any costs associated with the promotion that are related to seniority, such as the loss in job-specific capital). The estimates of $\varphi_f{}^k - b^k$ in the demotion equation and in both equations for lateral moves have analogous interpretations, and comparisons of $\varphi_f{}^k - b^k$ across all four equations provide information about the relative importance of the independent variables to the firm's job assignment decision relative to stayers.

Note that identification of the multinomial logit requires that the parameters of the base equation (i.e., the one corresponding to the worker staying in the same job) be normalized to zero, and therefore the parameters for promotions, lateral moves, and demotions measure effects that are relative to those for workers who stay in the same job. Hence, the magnitude and sign of the estimate of $\varphi_f^k - b^k$ in, say, the promotion equation is not simply the net effect of an addition year of seniority on the probability of promotion but rather the net effect relative to staying in the same job.

1.5 Empirical Results

Table 1.7 displays the parameters of interest for the multinomial logit from Equation (1.3) and accounting for the wage bill associated with each transition.[8] The first thing to note is that general labor market experience has a negative and significant estimated coefficient in the equations for promotions and lateral moves and is essentially zero for demotions. The estimate, denoted by the parameter φ_g^k appears only in the expected productivity equation, and one might expect this coefficient to be positive if increases in general human capital accumulation should make one more productive. However, EXP_{it} does not appear in the transition cost equation only for expositional purposes. General labor market experience might affect transition costs in multiple ways. For example, more experienced (older) workers may have already peaked or have found good long term job matches, whereas younger workers are still moving up the career path or are being reassigned to better matches. It could also be more difficult for older workers to make significant job changes if younger workers

[8] The counterfactual wage a worker would have received under a different assignment is clearly not available. Therefore, to capture the impact of the prices of promotions, demotions and lateral moves on the probabilities of various job assignments, I calculated the mean change in real wages associated with each transition separately for every firm-reporting level. There is not enough variation to compute these averages each year, therefore the mean is calculated across all workers in that firm-reporting level who had the same transition.

are more flexible and adaptive. In either case, if those effects outweigh the productivity gains from experience, it would drive the estimated coefficient on experience in the negative direction. Note also that this coefficient in the promotions equation is more than twice its magnitude in the equation for lateral moves that entail a change in job title, and exceeds by an even greater margin the coefficient in the equation corresponding to other lateral transitions. This suggests that while more experienced workers are less likely to be both promoted or laterally moved than retained in the same job, younger workers are more likely to be moved laterally than older workers.

The coefficient on seniority $\varphi_f^k - b^k$ reflects the productivity effect of seniority net of transition costs. This estimated coefficient is positive and significant only for promotions and lateral moves with job title changes, although the maginitudes are small. The (unreported) log-odds ratios are only 1.015 for promotions and 1.005 for the lateral transitions, which means that an additional year of seniority limitedly improves the likelihood of promotion or lateral mobility. This positive estimate of seniority implies that $\varphi_f^k - b^k > 0$. As discussed above, this could lead to two interpretations conditional on the hypothesis that $\varphi_f^k \geq 0$: either seniority makes job transitions less costly ($b^k < 0$), or the overall productivity returns to seniority dominate any detrimental job-specific costs $\varphi_f^P > b^P > 0$ associated with the transition.

Similarly, the coefficient on the change in span of control (γ^k-c^k) is positive and significant for promotions and lateral moves with a job title change but negative and significant for demotions and is essentially zero for lateral transitions without a job title change. The positive coefficients suggest that the added productivity associated with providing a worker with additional resources (subordinates) outweighs the costs involved with the job change for promotions and lateral moves. This is not the case

Table 1.7
Multinomial Logit Estimates from Equation (1.3) – Including the Expected Price of Transition

Variable	**Estimated Parameters**	**Promote**	**Demote**	**Lateral w/ Job Title Change**	**Lateral w/o Job Title Change**
Intercept	$\eta^k - a^k$	2.935*** (0.0001)	-3.670*** (0.0001)	1.343*** (0.0005)	-1.283*** (0.0001)
Experience (EXP_{it})	ϕ_g^k	-0.059*** (0.0001)	-0.0006 (0.866)	-0.025*** (0.0001)	-0.004*** (0.001)
Seniority (SEN_{it})	$\phi_f^k - b^k$	0.014*** (0.0001)	-0.004 (0.197)	0.006*** (0.0008)	0.0002 (0.867)
Span of Control ($SPAN_{it-1}$)	α^k	-0.005* (0.076)	0.025*** (0.0001)	0.009*** (0.001)	-0.007*** (0.003)
Change in Span of Control ($\Delta SPAN_{it}$)	$\gamma^k - c^k$	0.065*** (0.0001)	-0.028*** (0.004)	0.051*** (0.0001)	-0.0002 (0.976)
Mean Price of Promotion	--	0.0006 (0.724)	0.005 (0.276)	0.01*** (0.00001)	0.007*** (0.0001)
Mean Price of Demotion	--	0.000009 (0.993)	-0.003*** (0.006)	-0.001 (0.161)	0.0005 (0.313)
Mean Price of Lateral Move with Change in Job	--	-0.0002 (0.942)	-0.02*** (0.0001)	0.0003* (0.078)	0.013*** (0.0001)
Mean Price of Lateral Move with *no* Change in Job Title	--	0.010** (0.022)	0.011 (0.286)	-0.010** (0.012)	-0.020*** (0.0001)
Mean Price of Retaining the worker in the Same Job	--	0.0008* (0.068)	-0.010 (0.346)	-0.0004 (0.440)	-0.01*** (0.009)

Table 1.7 (Continued)

Number of observations	86,178
Log-Likelihood	-82,804.03

Note: p-values are in parentheses below the coefficient estimates. The assignment choice "Lateral w/ Job Title Change" aggregates the following mobility types found in Tables 9-12: Lateral-Strict, Lateral-Up, and Lateral-Down. The assignment choice "Lateral w/o Job Title Change" aggregates the following mobility types: Lateral-Same Job, Level-Up Same Job, Level-Down Same Job. The model also includes the following controls: year indicators; firm, industry, and ownership indicators; firm size, sales and profits; worker education and reporting level in the firm; and indicators for whether the worker in the new position is bonus eligible, eligible for long-term incentive plans or is on the board of directors.

for demotions as the negative coefficient indicates that the costs associated with increasing the resources of a demoted worker outweighs any productivity gains.

Some interesting patterns appear in the price coefficients. For example, the Mean Price of Promotion variable has no impact on the probability of promotion or demotion but is positive and significant in the models for both lateral moves with and without a job title change. Holding constant the firm and worker characteristics that are also in the model, this suggests that as promotions get more expensive, workers are more likely to receive lateral assignments than stay in the same job. This could be because those workers would normally be considered for promotion under lower promotion wage spreads and are instead laterally moved. Furthermore, when the wage spread for demotions increases, workers are less likely to be demoted. Put differently, when wage reductions for demotions are large in absolute value, the probability of being demoted increases relative to staying in the same job. Also, when the price of lateral moves that entail a job title change increases, workers are more likely to be moved laterally without a job title change rather than remaining in the same position.

The motivational framework of this section highlights several important points for the empirical work in the rest of the paper. Job changes are not costless, and these costs as well as the returns to standard human capital characteristics should be expected to vary across the range of potential assignment options available to the firm. This can be seen empirically by the varying magnitudes and signs of the coefficients across equations for the various job transitions in the multinomial logit models. Additionally, lateral moves with a job title change and promotions look more similar to one another than to those who stay in the same job or to the other transitions, while demotions appear distinctly different from any of the mobility outcomes. Lastly, incorporating the monetary costs of job assignments suggests that there may be substitution across jobs when the cost of one type of assignment becomes more or less expensive. The remainder of the paper investigates the relationships between worker outcomes and job mobility in detail, with an emphasis on understanding the relative importance of lateral mobility compared to other job assignment options such as promotion, demotion or retention in the same job.

1.5.1 The Determinants of Pay

I now investigate the relationship between job mobility and career outcomes by estimating the determinants of pay. Following Abowd et al. (1999), consider a standard wage equation with matched employer-employee data that has the form:

$$y_{ijt}=JOB_{ijt}\Omega + X_{ijt}\beta + F_{jt}\delta + \theta_i + \eta_t + \varepsilon_{ijt} \tag{1.4}$$

where y_{ijt} is the logarithm of annual real base pay (or base + bonus pay) of individual *i* at firm *j* in year *t*; JOB_{ijt} is a vector of characteristics related to the job individual *i* holds in period *t*; X_{ijt} is a vector of other characteristics related to the individual; F_{jt} is a vector of firm characteristics; θ_i is the individual effect; η_t are year effects; and ε_{ijt} is the error term.[1]

The structure of the data does not allow me to track the mobility of individuals across firms, and so the firm and worker effects cannot be disentangled and are forced to collapse into a single heterogeneity component. One consequence of this is that the individual effects cannot be interpreted as ability or other aspects specific to the individual that are constant over time; instead, when θ_i is included it also captures the pure firm effect and its relationship between the variables in X and the design matrices that establish the person and firm effects. Thus, θ_i should be interpreted more

[1] I assume the errors are i.i.d. and normally distributed.

generally as an individual effect that includes those components associated with both the worker and the firm that are constant over time.[2]

Another concern is that an individual's job is possibly endogenous to pay. This would be especially problematic if promotions or one's position in the hierarchy simply reflected a justification for the level of compensation. That appears not to be the case, as Tables 1.5 and 1.6 showed that promotions and other transitions often come from all parts of the pay distribution. To the extent that the heterogeneity component θ_i captures those aspects in the residual related to a worker's job (conditional on the variables in X_{ijt} and F_{jt}), assuming that JOB_{ijt} and pay are exogenous may be justified; however, this is unlikely as there are important time varying factors such as individual performance that are omitted from equation (1.4). These data do not include an explicit measure of worker performance, but the bonus pay data may contain useful information. In the next subsection, I construct two novel measures of relative performance using bonus data on workers and explore the impact of omitting that information on the estimates obtained here.

Table 1.8 displays the regression results for the logarithm of real annual base pay estimated by ordinary least squares. Column I includes only indicators for the worker's position in the hierarchy while columns II to VI progressively add more controls: column II adds year effects and dummy variables for the firm's SIC division as well as dichotomous indicators for firm ownership (public, private, parent company, subsidiary); column III adds additional variables to net out time-varying firm-specific heterogeneity in firm size, sales and profits; column IV adds additional variables to control for other aspects related to the worker's job; column V adds

[2] A second consequence is that the resulting estimates of Ω,β and δ are potentially biased without the ability to include a separate firm effect. Unfortunately, there is no solution to this problem with these data, but the impact of such an omission poses an interesting question for future research as richer data sets become available.

variables to control for human capital characteristics such as education, general labor market experience and tenure at the firm; and column VI includes the individual heterogeneity component.

There are several interesting aspects to this table. One is that the organizational unit level has explanatory power even when controlling for the reporting level, although these effects are mitigated in column VI when the individual effect is included in the model. Secondly, across all specifications, the coefficients on reporting level descend monotonically from the highest reporting level. These same coefficients also remain relatively unaffected when controlling for firm-specific attributes but are substantially reduced when other characteristics related to the worker's job are included. Once human capital characteristics are included in the model (column V) the coefficients on reporting levels are reduced from column I by 30-40%. The most significant impact on the reporting level estimates, organizational unit and other job variables occurs when the individual effect is added. These results suggest that while jobs (especially rank) are important in determining worker pay, human capital characteristics and especially those time-invariant traits specific to the individual play a large role.[3]

1.5.2 Pay and Performance

As previously noted, a worker's job in the firm may be endogenous to pay. Without proper instruments I assumed that conditional on other factors in the model, JOB_{ijt} is exogenous. While it is possible that including the individual effect may have satisfied that requirement by controlling for those factors that are time-invariant, there could be time-varying variables omitted from the model that might lead to a violation

[3] When the dependent variable is the logarithm of real (base + bonus) pay, the overall results are qualitatively the same except, as expected, bonus pay is significantly larger at higher reporting levels.

Table 1.8

Dependent Variable: Log of Real Base Pay

Variable	I	II	III	IV	V	VI
Reporting Level 1	2.020***	2.034***	2.143***	1.537***	1.364***	0.477***
	(0.010)	(0.009)	(0.009)	(0.009)	(0.009)	(0.007)
Reporting Level 2	1.102***	1.113***	1.224***	0.873***	0.777***	0.198***
	(0.006)	(0.006)	(0.005)	(0.005)	(0.006)	(0.003)
Reporting Level 3	0.620***	0.626***	0.728***	0.470***	0.414***	0.100***
	(0.005)	(0.005)	(0.004)	(0.004)	(0.005)	(0.003)
Reporting Level 4	0.380***	0.382***	0.472***	0.273***	0.239***	0.056***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)
Reporting Level 5	0.241***	0.237***	0.307***	0.159***	0.139***	0.034***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.002)
Reporting Level 6	0.125***	0.121***	0.158***	0.079***	0.072***	0.018***
	(0.005)	(0.005)	(0.004)	(0.004)	(0.004)	(0.002)
Organizational Unit 1	0.048	0.024	0.155***	0.156***	0.066**	-0.033*
	(0.033)	(0.033)	(0.030)	(0.034)	(0.032)	(0.020)
Organizational Unit 2	0.157***	0.141***	0.279***	0.245***	0.147***	-0.006
	(0.033)	(0.033)	(0.030)	(0.034)	(0.032)	(0.019)
Organizational Unit 3	0.107***	0.089**	0.250***	0.199***	0.120***	-0.018
	(0.033)	(0.033)	(0.030)	(0.034)	(0.032)	(0.019)
Organizational Unit 4	-0.015	-0.009	0.163***	0.123**	0.076**	-0.031
	(0.033)	(0.033)	(0.030)	(0.034)	(0.032)	(0.019)
Organizational Unit 5	0.040	0.038	0.208***	0.138***	0.096***	-0.021
	(0.034)	(0.033)	(0.031)	(0.034)	(0.032)	(0.019)
Organizational Unit 6	0.044	0.047	0.133***	0.048	0.013	-0.020
	(0.037)	(0.036)	(0.033)	(0.037)	(0.035)	(0.020)
Experience	--	--	--	--	0.034***	0.073***
	--	--	--	--	(0.0005)	(0.0013)
Experience Squared	--	--	--	--	-0.0005***	-0.0005***
	--	--	--	--	(9.6×10^{-6})	(0.00001)
Seniority	--	--	--	--	-0.0023***	0.0028***
	--	--	--	--	(0.0003)	(0.00001)

Table 1.8 (Continued)

Seniority Squared	--	--	--	--	0.0001***	0.00004***
	--	--	--	--	($7.7\text{x}10^{-6}$)	($1.1\text{x}10^{-7}$)
Schooling = 16 Years	--	--	--	--	0.164***	--
	--	--	--	--	(0.003)	--
Schooling = 17, 18 Years	--	--	--	--	0.228***	--
	--	--	--	--	(0.003)	--
Schooling > 18 Years	--	--	--	--	0.265***	--
	--	--	--	--	(0.004)	--
# Employees Supervised)	--	--	--	0.0098***	0.010***	0.0053***
	--	--	--	(0.0012)	(0.001)	(0.0011)
Bonus Eligible	--	--	--	0.175***	0.141	0.046
	--	--	--	(0.002)	(0.003)	(0.002)
Long-term Incentive	--	--	--	0.268***	0.233***	0.032***
	--	--	--	(0.002)	(0.002)	(0.001)
On Board of Directors	--	--	--	0.049***	0.045***	0.045***
	--	--	--	(0.0004)	(0.0004)	(0.0004)
Total Sales (per $100,000)	--	--	0.002***	0.001***	0.001***	0.001***
	--	--	(0.00002)	(0.00002)	(0.00002)	(0.00002)
Total Profits (per $100,000)	--	--	0.002***	0.002***	0.001***	-0.002***
	--	--	(0.0004)	(0.0004)	(0.0004)	(0.0004)
Total Employees	--	--	0.002***	0.001***	0.001***	0.001***
	--	--	(0.00003)	(0.00002)	(0.00002)	(0.00002)
Firm Ownership and Industry Effects		YES	YES	YES	YES	--
Year Effects		YES	YES	YES	YES	YES
Worker Effects						YES
Number of Observations:	177,637	177,637	177,637	177,637	177,637	177,637
R-Squared	0.386	0.428	0.515	0.609	0.663	0.985

Notes: Models are estimated using OLS. Standard errors are reported below the regression coefficient. *** indicates significance at the 0.01 percent or better. ** indicates significance between 0.01 and 0.05 percent and * indicates significance between 0.05 and 0.10.

of the conditional exogeneity assumption. One obvious exclusion would be individual performance.

Neither relative nor absolute performance ratings are available in these data. However, the bonus awards reported for each individual are supposed to reflect performance in the current year. This provides an opportunity to construct several measures of individual performance relative to other workers in the firm. To do so, I estimate the following models using OLS:

$$Bonus_{ijt}=\alpha PROFITS_{jt} + FIRM_j + REPORTINGLEVEL_{ijt}\Pi + \varepsilon_{ijt} \qquad (1.5)$$

$$Bonus_{ijt}=\alpha PROFITS_{jt} + FIRM_j + REPORTINGLEVEL_{ijt}\Pi + \qquad (1.6)$$
$$+ FIRM_j * REPORTINGLEVEL_{ijt}\Psi + \nu_{ijt}$$

The difference between an individual's actual bonus received and the predicted bonus creates a measure of relative performance ($PERFORMANCE_{ijt}=Bonus_{ijt} - Predicted\ Bonus_{ijt}$). Since portions of worker bonuses may be due to overall firm performance and not necessarily individual performance, I include a measure of firm profits to net out the effect of time-varying firm performance that would equally affect bonus awards of all workers in a firm. For example, the measure resulting from equation (1.5) reflects the deviation in the individual's bonus award above (or below) that of the average worker in her same firm and position in the hierarchy. Equation (1.6) refines the model in equation (1.5) by forcing the reporting level effects to be firm-specific. In each case, the performance measures obtained reflect relative performance.

Table 1.9 displays the results of models similar to columns V and VI in Table 7 but also includes one of the two constructed measures of relative performance.

Since bonuses are required to construct these measures, the sample is restricted only to those workers the firm indicates are eligible to receive them. Also, since performance is constructed with bonus data the regressions, this table utilizes only base pay as the dependent variable (rather than base + bonus pay). Column I reports the results for a benchmark specification in line with that of column V in Table 7. Columns II and III include the performance measure from Equation (5) (Performance A), and columns IV and V the measure from Equation (4) (Performance B). Columns III and V include the individual effect while the others do not.

In each case, the performance measures have positive and significant effects on base pay. For example, a bonus award of $10,000 above the predicted bonus for a worker in the same firm and reporting level is related to a 3.1% higher salary (Performance B and Column IV). Also, the inclusion of the performance measures increases the size of the reporting level coefficients. Note that the inclusion of the reporting level dummies means that the effect of performance relates to the within-reporting-level variation in pay. A standard omitted variable bias argument states that good performance relative to workers in a similar job is negatively correlated with being in a high reporting level. These results then imply that being a standout in top positions is more difficult than outperforming ones peers in lesser jobs.

The other variables in columns II and IV are largely unaffected by including performance. Unlike the results in Medoff and Abraham (1980, 1981), including relative performance has very little impact on the experience, seniority and education coefficients. If anything, inclusion of the performance measures slightly reduces the coefficients of the schooling coefficients. If the constructed measures are good approximations of relative performance, these estimates cannot reject the human capital explanation that senior workers in the same position are paid more because they are more productive, not only because they are more senior.

Table 1.9
(Dependent Variable: *Log* Real Base Pay)
Including Performance

	I	II	III	IV	V
Variable	Benchmark	Performance A (Eqn 5)		Performance B (Eqn 6)	
Performance Measure	--	0.026***	0.0056***	0.031***	0.0053***
	--	(0.0003)	(0.0001)	(0.0003)	(0.0001)
Reporting Level 1	1.403***	1.528***	0.514***	1.472***	0.502***
	(0.010)	(0.010)	(0.008)	(0.009)	(0.008)
Reporting Level 2	0.806***	0.835***	0.208***	0.820***	0.201***
	(0.006)	(0.006)	(0.004)	(0.006)	(0.004)
Reporting Level 3	0.440***	0.452***	0.100***	0.446***	0.094***
	(0.006)	(0.005)	(0.003)	(0.005)	(0.003)
Reporting Level 4	0.260***	0.266***	0.053***	0.264***	0.049***
	(0.005)	(0.005)	(0.003)	(0.005)	(0.003)
Reporting Level 5	0.148***	0.151***	0.029***	0.152***	0.026***
	(0.005)	(0.005)	(0.003)	(0.005)	(0.003)
Reporting Level 6	0.078***	0.079***	0.012***	0.081***	0.010***
	(0.006)	(0.005)	(0.003)	(0.005)	(0.003)
Unit Level 1	0.124***	0.142***	-0.031	0.156***	-0.026
	(0.038)	(0.037)	(0.022)	(0.036)	(0.022)
Unit Level 2	0.223***	0.249***	0.002	0.233***	0.001
	(0.038)	(0.037)	(0.022)	(0.036)	(0.022)
Unit Level 3	0.193***	0.217***	-0.012	0.207***	-0.012
	(0.038)	(0.036)	(0.022)	(0.036)	(0.022)
Unit Level 4	0.155***	0.177***	-0.021	0.169***	-0.021
	(0.038)	(0.036)	(0.022)	(0.036)	(0.022)
Unit Level 5	0.163***	0.186***	-0.010	0.174***	-0.012
	(0.038)	(0.037)	(0.022)	(0.036)	(0.022)
Unit Level 6	0.044	0.057	-0.004	0.047	-0.005
	(0.041)	(0.039)	(0.022)	(0.039)	(0.022)
Experience	0.034***	0.034***	0.078***	0.034***	0.077***
	(0.0006)	(0.0005)	(0.0014)	(0.0005)	(0.0014)
Experience Squared	-0.0005***	-0.0005***	-0.0006***	-0.0005***	-0.0006***
	(0.00001)	(0.00001)	(0.00001)	(0.00001)	(0.00001)

Table 1.9 (Continued)

Seniority	-0.0026***	-0.0026***	0.0011	-0.0027***	0.0011
	(0.0003)	(0.0003)	(0.0008)	(0.0003)	(0.0008)
Seniority Squared	0.0001***	0.0001***	0.00008***	0.0001***	0.00008***
	($8.3x10^{-6}$)	($8.0x10^{-6}$)	(0.00001)	($8.0x10^{-6}$)	(0.00001)
Schooling = 16 years	0.151***	0.149***	--	0.145***	--
	(0.003)	(0.003)	--	(0.003)	--
Schooling = 17,18 years	0.213***	0.210***	--	0.205***	--
	(0.003)	(0.003)	--	(0.003)	--
Schooling >=19 years	0.244***	0.242***	--	0.240***	--
	(0.004)	(0.004)	--	(0.004)	--
# Employees Supervised	0.010***	0.006***	0.005***	0.007***	0.005***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Incentive Eligible	0.223***	0.217***	0.027***	0.210***	0.027***
	(0.002)	(0.002)	(0.001)	(0.002)	(0.001)
Job is On Board of Directors	0.044***	0.040***	0.013***	0.039***	0.014***
	(0.0005)	(0.0005)	(0.0003)	(0.0005)	(0.0003)
Total Sales	0.001***	0.001***	0.0005***	0.001***	0.0005***
(per $100,000)	(0.0003)	(0.00003)	(0.00003)	(0.00003)	(0.00003)
Total Profits	0.009***	0.006***	-0.002***	0.006***	-0.002***
(per $100,000)	(0.0005)	(0.0005)	(0.0002)	(0.0005)	(0.0002)
Total Employees	0.002***	0.002***	-0.00002	0.002***	-0.00003
	(0.00003)	(0.00003)	(0.00003)	(0.00003)	(0.00003)
Ownership and Industry	YES	YES	--	YES	--
Year Effects	YES	YES	YES	YES	YES
Individual Effects			YES		YES
Number of Observations:	116,743	116,743	116,743	116,743	116,743
R-Squared	0.633	0.660	0.984	0.663	0.984

Notes: Models are estimated using OLS. Standard errors are reported below the regression coefficient. *** indicates significance at the 0.01 percent or better. ** indicates significance between 0.01 and 0.05 percent and * indicates significance between 0.05 and 0.10. The benchmark model does not include a performance measure.

Finally, columns III and V add individual effects to the models in columns II and IV. While the impact of relative performance is still positive and significant, the magnitudes of the performance variables decreases substantially. Without individual effects, a \$10,000 deviation from the predicted bonus was associated with 2.6% (Column II) or 3.1% (Column III) increase in pay, but the inclusion of individual effects reduces theses magnitudes to around 0.5%. There are aspects related to worker performance measured by the individual effects. Unfortunately, we cannot assume the individual effects capture only time-invariant traits related to the worker such as innate ability because they also reflect the omitted firm effects, but the relationship between ability and performance as a plausible explanation for the positive correlation is interesting.

1.5.3 Job Mobility and Earnings Growth

First differencing Equation (1.4) yields the estimation model for earnings growth:

$$\Delta y_{ijt} = \Delta JOB_{ijt}\Omega + \Delta X_{ijt}\beta + \Delta F_{jt}\delta + v_{ijt} \qquad (1.7)$$

The vector ΔJOB_{ijt} maps the hierarchical position indicators into transitions such as promotion, demotion, lateral move and stayer, which were discussed in the previous section. The individual effect differences out along with the education indicators, and experience and tenure become vectors of ones that are absorbed in the intercept. Similarly those time-invariant variables associated with the firm (industry and ownership dummies) also drop out when first differencing. The rest of the time-varying variables are first-differenced in the standard way. As with equation (1.4) it is possible that ΔJOB_{ijt} (promotions, demotions, etc.) is endogenous to earnings growth, but without the availability of good instruments, I assume that, conditional on the other right-hand-side variables, $E(\Delta JOB_{ijt}v_{ijt})=0$. To help facilitate the validity of this assumption, models are also estimated that reintroduce the individual effect, anticipating that it will capture those remaining factors in the residual that pose endogeneity concerns.

Table 1.10 presents the OLS regression results for this model when the dependent variable is the first difference of log base pay.[4] Similar to Table 1.8 the columns from I to V introduce additional controls where the only difference between column IV and V is that column V reintroduces the individual effect to the model. The variables of interest are at the top of the table (Promotion, Demotion,...,Level Down-Same Job) where the base category for these dummy variables are those workers who stay in the same job. Promotions, Demotions, Lateral-Strict, and Lateral-Down are consistently and significantly different from those who stay in the same job. Lateral-Up, Lateral-Same Job and Level-Up Same Job are statistically different from those who stay in the same job, while Level Down-Same Job is associated with essentially the same earnings growth as stayers. Inclusion of the individual effect does remove the statistical significance for the transition Lateral-Same Job.

The economic significance of these estimates relative to stayers is important for Promotions, Demotions and lateral transitions that entail a change in job title (Lateral-Strict, Lateral-Up and Lateral-Down). For example, the change in pay associated with the transitions that do not include a change in job title is either zero or is estimated to be between -0.4% and 0.9%, but the change in pay for lateral moves with a job title change is much higher relative to stayers (1.5% to 3.4%).[5]

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[4] Using changes in log base plus bonus pay as the dependent variable yielded similar results in the analysis that follows.

[5] I focus on the models with full controls in Columns IV and V to get the range of estimated coefficients.

Table 1.10
Dependent Variable: Changes in Log Base Pay

Variable	I	II	III	IV	V
Promotion	0.100***	0.100***	0.098***	0.095***	0.082***
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
Demotion	-0.054***	-0.055***	-0.057***	-0.059***	-0.052***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Lateral -- STRICT	0.022***	0.021***	0.021***	0.020***	0.015***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
Lateral -- UP	0.024***	0.023***	0.022***	0.021***	0.016***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Lateral -- DOWN	0.035***	0.034***	0.034***	0.034***	0.030***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)
Lateral – SAME JOB	-0.001	-0.003***	-0.003***	-0.004***	0.002
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
Level Up – SAME	0.009***	0.009***	0.010***	0.009***	0.007***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Level Down – SAME	0.003***	0.003***	0.002***	0.001	0.007***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Δ # Employees Supervised (in thousands)	--	--	--	0.005***	0.004***
	--	--	--	(0.0001)	(0.0001)
New Job is Bonus Eligible	--	--	--	0.025***	0.028***
	--	--	--	(0.002)	(0.003)
New Job is Long-term Incentive Eligible	--	--	--	0.023***	0.018***
	--	--	--	(0.002)	(0.002)
New Job is On Board of Directors	--	--	--	0.069***	0.049***
	--	--	--	(0.004)	(0.005)
Δ Total Sales	--	--	0.001***	0.001***	0.0001***

Table 1.10 (Continued)

(per $100,000)	--	--	(0.0003)	(0.00002)	(0.00005)
	--	--	(0.0003)	(0.00002)	(0.00005)
	--	--	(0.0002)	(0.00004)	(0.0002)
Δ Total	--	--	0.0001***	-0.00003	-0.0002***
Employees (in thousands)	--	--	(0.00003)	(0.00004)	(0.00005)
Year Effects		YES	YES	YES	YES
Individual Effects					YES
Number of Observations:	100,534	100,534	100,534	100,534	100,534
R-Squared	0.052	0.063	0.076	0.108	0.508

Notes: Models are estimated using OLS. Standard errors are reported below the regression coefficient. *** indicates significance at the 0.01 percent or better. ** indicates significance between 0.01 and 0.05 percent and * indicates significance between 0.05 and 0.10.

Alternatively, the pay change for Promotions relative to those who stay in the same position is 8.2% to 9.5%, and for demotions it is -5.2% to -5.9%. Therefore, workers who are demoted in these data are penalized heavily on average, Promotionhave earnings increases of 2.7 to 6.7 times the size of lateral transitions involving job changes, and while lateral transitions with no job change are sometimes statistically different from those who stay in the same job, these transitions are associated with little change in pay over stayers when not estimated to be zero.

Table 1.10 distinguishes the different types of transitions from those who stay in the same job. Table 1.11 displays the results for tests of the equality of the coefficients across mobility types. In no case are promotions or demotions statistically the same as any of the other transitions but the lateral transitions are sometimes indistinguishable from one another. Lateral transitions with a job title change are more similar to other lateral moves with a job title change but not lateral moves which keep the same job title. The same is true for lateral transitions with no change in job title.

As mentioned before, relative performance may be an important determinant of both earnings growth and job mobility. If worker performance is the mechanism sorting workers into jobs to begin with, then some or all of the observed pay growth associated with the changing jobs (i.e., the coefficients on the transition variables in the models of pay growth) may be due to differences in worker performance and not the pay difference in jobs themselves. Table 1.12 displays the results when including the two performance measures in the model. The sample is again restricted to workers who are bonus eligible, and the configuration of the table is similar to that in Table 1.9 The results are not much different from models without performance. The coefficients on the transition variables are largely unaffected, and including performance does not significantly help or hinder the ability to distinguish the mobility types, except that lateral transitions with no change in job title appear more alike. This can be seen by .

Table 1.11
Testing the Equality of the Mobility Coefficients
(Dependent Variable: Changes in *Log* Base Pay)
Ho: $\Omega_i=\Omega_j$ for $i,j=1, \ldots ,9$ and $i \neq j$

Mobility$_i$ versus Mobility$_j$		I	II	III	IV	V
Stay	Promotion	<.0001	<.0001	<.0001	<.0001	<.0001
Stay	Demotion	<.0001	<.0001	<.0001	<.0001	<.0001
Stay	Lateral – STRICT	<.0001	<.0001	<.0001	<.0001	<.0001
Stay	Lateral -- UP	<.0001	<.0001	<.0001	<.0001	<.0001
Stay	Lateral -- DOWN	<.0001	<.0001	<.0001	<.0001	<.0001
Stay	Lateral – SAME JOB	0.176	0.004	0.008	0.002	0.307
Stay	Level Up – SAME JOB	<.0001	<.0001	<.0001	<.0001	<.0001
Stay	Level Down – SAME JOB	0.000	0.007	0.032	0.316	0.000
Promotion	Demotion	<.0001	<.0001	<.0001	<.0001	<.0001
Promotion	Lateral – STRICT	<.0001	<.0001	<.0001	<.0001	<.0001
Promotion	Lateral -- UP	<.0001	<.0001	<.0001	<.0001	<.0001
Promotion	Lateral -- DOWN	<.0001	<.0001	<.0001	<.0001	<.0001
Promotion	Lateral – SAME JOB	<.0001	<.0001	<.0001	<.0001	<.0001
Promotion	Level Up – SAME JOB	<.0001	<.0001	<.0001	<.0001	<.0001
Promotion	Level Down – SAME JOB	<.0001	<.0001	<.0001	<.0001	<.0001

Table 1.11 (Continued)

Demotion	Lateral – STRICT	<.0001	<.0001	<.0001	<.0001	<.0001
Demotion	Lateral -- UP	<.0001	<.0001	<.0001	<.0001	<.0001
Demotion	Lateral -- DOWN	<.0001	<.0001	<.0001	<.0001	<.0001
Demotion	Lateral – SAME JOB	<.0001	<.0001	<.0001	<.0001	<.0001
Demotion	Level Up – SAME JOB	<.0001	<.0001	<.0001	<.0001	<.0001
Demotion	Level Down – SAME JOB	<.0001	<.0001	<.0001	<.0001	<.0001
Lateral – STRICT	Lateral -- UP	0.5378	0.4994	0.7649	0.7875	0.8743
Lateral – STRICT	Lateral -- DOWN	<.0001	<.0001	<.0001	<.0001	0.0001
Lateral – STRICT	Lateral – SAME JOB	<.0001	<.0001	<.0001	<.0001	<.0001
Lateral – STRICT	Level Up – SAME JOB	<.0001	<.0001	<.0001	<.0001	<.0001
Lateral – STRICT	Level Down – SAME JOB	<.0001	<.0001	<.0001	<.0001	<.0001
Lateral -- UP	Lateral -- DOWN	0.0036	0.0031	0.0016	0.0006	0.0025
Lateral -- UP	Lateral – SAME JOB	<.0001	<.0001	<.0001	<.0001	<.0001
Lateral -- UP	Level Up – SAME JOB	<.0001	<.0001	<.0001	<.0001	0.0128

Table 1.11 (Continued)

Lateral -- UP	Level Down – SAME JOB	<.0001	<.0001	<.0001	<.0001	0.0126
Lateral -- DOWN	Lateral – SAME JOB	<.0001	<.0001	<.0001	<.0001	<.0001
Lateral -- DOWN	Level Up – SAME JOB	<.0001	<.0001	<.0001	<.0001	<.0001
Lateral -- DOWN	Level Down – SAME JOB	<.0001	<.0001	<.0001	<.0001	<.0001
Lateral – SAME JOB	Level Up – SAME JOB	<.0001	<.0001	<.0001	<.0001	0.0031
Lateral – SAME JOB	Level Down – SAME JOB	0.0004	<.0001	0.0003	0.0012	0.0043
Level Up – SAME JOB	Level Down – SAME JOB	<.0001	<.0001	<.0001	<.0001	0.9498

Note: The p-values in this table are for F-tests that the two coefficients in the model are equal, separate tests for each model I to VII. Shaded areas are where the p-value >= 0.05; that is, when we cannot reject the null hypothesis that the coefficients are equal. Columns I through Column V are the tests for the corresponding columns in Table 17A.

Table 1.12

Dependent Variable: Changes in *Log* Base Pay (*Including Performance)*

	I	II	III	IV	V
Variable	Benchmark	Performance A		Performance B	
Performance Measure	--	0.0030***	0.0024***	0.0029***	0.0021***
	--	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Promotion	0.097***	0.096***	0.083***	0.095***	0.082***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Demotion	-0.044***	-0.049***	-0.045***	-0.048***	-0.045***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Lateral -- STRICT	0.020***	0.020***	0.016***	0.020***	0.016***
	(0.001)	(0.001)	(0.002)	(0.001)	(0.002)
Lateral -- UP	0.022***	0.021***	0.018***	0.022***	0.019***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Lateral -- DOWN	0.035***	0.035***	0.031***	0.034***	0.030***
	(0.003)	(0.003)	(0.004)	(0.003)	(0.004)
Lateral – SAME JOB	-0.004***	-0.003**	0.002	-0.003**	0.002
	(0.001)	(0.001)	(0.002)	(0.001)	(0.002)
Level Up – SAME JOB	0.009***	0.014***	0.011***	0.013***	0.010***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Level Dn – SAME JOB	0.002	-0.001***	0.005***	-0.0003	0.006***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Change in # Employees Supervised	0.005***	0.005***	0.002***	0.003***	0.002***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
New Job is Long-term Incentive Eligible	0.021***	0.017***	0.010***	0.017***	0.010***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
New Job is On Board of Directors	0.068***	0.066***	0.043***	0.066***	0.043***
	(0.004)	(0.004)	(0.005)	(0.004)	(0.005)
Δ Total Sales	0.001***	0.001***	0.00002	0.001***	0.00002
	(0.00003)	(0.00003)	(0.00005)	(0.0005)	(0.00005)

Table 1.12 (Continued)

Δ Total Profits	-0.003***	-0.003***	-0.001***	-0.003***	-0.0005**
	(0.00002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)
Δ Total Employees	-0.0001***	-0.00007***	-0.00002***	-0.00007***	-0.00009***
	(0.00003)	(0.00003)	(0.00001)	(0.00003)	(0.00005)
Year Effects	X	X	X	X	X
Individual Effects			X		X
Number of Observations:	85,253	85,253	85,253	85,253	85,253
R-Squared	0.101	0.098	0.500	0.098	0.500

Notes: Models are estimated using OLS. Performance A corresponds to performance measure generated from equation (5) in the text, and Performance B is the performance measure from equation (6). Standard errors are reported below the regression coefficient. *** indicates significance at the 0.01 percent or better. ** indicates significance between 0.01 and 0.05 percent and * indicates significance between 0.05 and 0.10. The benchmark model does not include a performance measure.

Table 1.13
Testing the Equality of the Mobility Coefficients
(Dependent Variable: Changes in *Log* Base Pay)
Including Performance
Ho: $\Omega_i = \Omega_j$ for $i,j=1, \ldots ,9$ and $i \neq j$

Mobility$_i$ versus Mobility$_j$		I	II	III	IV	V
		Bench	Performance A		Performance B	
Stay	Promotion	<.0001	<.0001	<.0001	<.0001	<.0001
Stay	Demotion	<.0001	<.0001	<.0001	<.0001	<.0001
Stay	Lateral – STRICT	<.0001	<.0001	<.0001	<.0001	<.0001
Stay	Lateral -- UP	<.0001	<.0001	<.0001	<.0001	<.0001
Stay	Lateral -- DOWN	<.0001	<.0001	<.0001	<.0001	<.0001
Stay	Lateral – SAME JOB	0.004	0.013	0.166	0.014	0.165
Stay	Level Up – SAME JOB	<.0001	<.0001	<.0001	<.0001	<.0001
Stay	Level Down – SAME JOB	0.162	0.296	<.0001	0.732	<.0001
Promotion	Demotion	<.0001	<.0001	<.0001	<.0001	<.0001
Promotion	Lateral – STRICT	<.0001	<.0001	<.0001	<.0001	<.0001
Promotion	Lateral -- UP	<.0001	<.0001	<.0001	<.0001	<.0001
Promotion	Lateral -- DOWN	<.0001	<.0001	<.0001	<.0001	<.0001
Promotion	Lateral – SAME JOB	<.0001	<.0001	<.0001	<.0001	<.0001
Promotion	Level Up – SAME JOB	<.0001	<.0001	<.0001	<.0001	<.0001
Promotion	Level Down – SAME JOB	<.0001	<.0001	<.0001	<.0001	<.0001
Demotion	Lateral – STRICT	<.0001	<.0001	<.0001	<.0001	<.0001

Table 1.13 (Continued)

Demotion	Lateral -- UP	<.0001	<.0001	<.0001	<.0001	<.0001
Demotion	Lateral -- DOWN	<.0001	<.0001	<.0001	<.0001	<.0001
Demotion	Lateral – SAME JOB	<.0001	<.0001	<.0001	<.0001	<.0001
Demotion	Level Up – SAME JOB	<.0001	<.0001	<.0001	<.0001	<.0001
Demotion	Level Down – SAME JOB	<.0001	<.0001	<.0001	<.0001	<.0001
Lateral – STRICT	Lateral -- UP	0.6167	0.8110	0.5394	0.5986	0.4299
Lateral – STRICT	Lateral -- DOWN	<.0001	<.0001	0.0002	<.0001	0.0005
Lateral – STRICT	Lateral – SAME JOB	<.0001	<.0001	<.0001	<.0001	<.0001
Lateral – STRICT	Level Up – SAME JOB	<.0001	0.0001	0.0329	<.0001	0.0098
Lateral – STRICT	Level Down – SAME JOB	<.0001	<.0001	<.0001	<.0001	<.0001
Lateral -- UP	Lateral -- DOWN	0.0006	0.0003	0.0089	0.0016	0.0219
Lateral -- UP	Lateral – SAME JOB	<.0001	<.0001	<.0001	<.0001	<.0001
Lateral -- UP	Level Up – SAME JOB	<.0001	0.0120	0.0556	0.0017	0.0180
Lateral -- UP	Level Down – SAME JOB	<.0001	<.0001	0.0003	<.0001	0.0004
Lateral -- DOWN	Lateral – SAME JOB	<.0001	<.0001	<.0001	<.0001	<.0001
Lateral -- DOWN	Level Up – SAME JOB	<.0001	<.0001	<.0001	<.0001	<.0001

Table 1.13 (Continued)

Lateral -- DOWN	Level Down – SAME JOB	<.0001	<.0001	<.0001	<.0001	<.0001
Lateral – SAME JOB	Level Up – SAME JOB	<.0001	<.0001	<.0001	<.0001	<.0001
Lateral – SAME JOB	Level Down – SAME JOB	0.0010	0.1915	0.1421	0.0773	0.0654
Level Up – SAME JOB	Level Down – SAME JOB	<.0001	<.0001	0.0004	<.0001	0.0097

Note: The p-values in this table are for F-tests that the two coefficients in the model are equal, separate tests for each model I to VII. Shaded areas are where the p-value >= 0.05; that is, when we cannot reject the null hypothesis that the coefficients are equal. The benchmark model does not include a performance measure. Columns I through Column V are the tests for the corresponding columns in Table 18A.

comparing the tests of equality presented in Table 1.13 to those in 1.11. The results in Table 1.12 suggest that the effect of changing jobs results in real pay growth evenwhen accounting for worker productivity. That is, wages are, at least to some extent, attached to jobs.

1.5.4 Job-to-Job Mobility

A potentially importance aspect of lateral mobility is to what extent these transitions affect future job changes. Table 1.14 displays basic multinomial logit estimates that predict the current period's transition as a function of previous transitions and the worker and job characteristics in the previous period. The base category is whether or not the worker remained in the same job in the previous period. The results are surprising and highlight the amount of mobility in these data - largely, mobility of any kind predicts mobility in the future. There is no noticeable serial correlation in promotions or demotions, but promotions and demotions each predict the other. Also, a lateral move with a job title change is strongly associated with future promotions and to a lesser extent future demotions and additional lateral moves with a job title change. Lateral transitions without a job title change have the weakest predictive power.

1.5.5 Firm-level Analysis of Job Mobility

Beyond understanding the role of lateral mobility for workers, it is also of interest to understand what types of firms experience more lateral mobility than others. The job transition rates estimated across single-firm case studies vary substantially from firm to firm, and the current information about the firm-specific determinants of lateral mobility at the firm-level pertains only to the adoption of job rotation (Eriksson and Ortega, 2004; Eriksson 2000; Osterman 1994, 2000). Investigating the role of

Table 1.14

Multinomial Logit Estimates For Mobility Outcomes
(Including Performance)

Variable	Mobility Outcome: Promotion	Demotion	Lateral w/ Job Title Change	Lateral w/o Job Title Change
Relative Performance	1.02***	0.999***	0.999***	0.999***
Promotion (t-1)	0.80	1.37***	1.17***	1.07
Demotion (t-1)	1.71***	0.83	1.48***	1.19***
Lateral w/ Job Change (t-1)	1.56***	1.26***	1.28***	1.12***
Lateral w/o Job Change (t-1)	1.11**	1.13**	1.05	0.93***
Reporting Level 1 or 2 (t-1)	0.11***	4.80***	1.30**	0.46***
Reporting Level 3 (t-1)	0.31***	4.22***	1.20*	0.63***
Reporting Level 4 (t-1)	0.51***	3.17***	1.31***	0.76***
Reporting Level 5 (t-1)	0.64**	2.36	1.13***	0.87**
Reporting Level 6 (t-1)	0.80**	1.76	1.10	0.98
# Employees Supervised (t-1) (in thousands)	1.00***	1.00	1.00***	1.00
Bonus Eligible (t-1)	1.32***	1.00	1.21***	0.97
Long-term Incentive Eligible (t-1)	1.31***	0.99	1.08***	0.99
On Board of Directors (t-1)	2.77***	0.41***	0.76***	0.39***
Schooling = 16 Years	1.18***	0.99	0.97	1.00
Schooling = 17, 18 Years	1.27***	1.03	1.03	0.94**
Schooling > 18 Years	1.11	0.75***	0.90**	0.85***

Note: The performance measure is estimated from equation 6 in the text. The base category consists of those workers who stay in the same job. The numbers reported are relative risk ratios. The models also include controls for firms size, sales, profits, firm ownership, industry and year dummies. *** indicates significance at the 0.01 percent or better. ** indicates significance between 0.01 and 0.05 percent and * indicates significance between 0.05 and 0.10. The Benchmark model does not include a performance measure.

firm-level heterogeneity on job mobility rates is not feasible with single-firm data sets but the data I use pertains to large set of firms that are heterogeneous across a broad set of characteristics.

To investigate the determinants of job mobility rates at the firm-level, I use the worker-level model estimated in Table 1.7 as a benchmark and aggregate those worker-level variables to the firm-level. The only worker-level variable that I could not aggregate directly was the worker reporting level. Instead, I include a variable for the total number of hierarchical levels in the firm to capture the extent to which the firm has a more vertical or horizontally oriented hierarchy. I also include firm-specific variables such as firm size, profits, sales and included industry and ownership indicators.[1]

The model is estimated with OLS and the results are presented in Table 1.15. Firms reporting a high proportion of workers with the equivalent of a Ph.D. education have much lower job mobility rates overall (the base category is the proportion of workers with less than a college degree). Firms with a high number of college graduates have lower demotion rates and higher rates of retaining in the same job. The employee learning hypothesis of job rotation suggests that rotating workers is an effective way to train employees, specifically as managers. Firms with a large proportion of workers with the equivalent of a masters degree experience higher rates of lateral mobility (with a job title change). A very senior workforce is negatively

[1] Recall that a limitation to the data is that I do not have information pertaining to all workers at the firm; instead, firms report information on workers they wish to be appraised and who tend to be sampled more frequently at the top of the hierarchy, down to as many as 12 reporting levels below the CEO. In aggregating the individual-level transitions to firm-level transition rates, I used the following selection rule for including workers in the rate calculations: Let $n(L)$ denote the number of workers in the reporting level under consideration and $n(L+1)$ the number of workers in the reporting level directly beneath level L. Here, L=1 represents the CEO and those workers reporting directly to the CEO have L=2. If $n(L)-n(L+1)\leq 0$, I included the $n(L)$ workers in the rate calculation. If $n(L)-n(L+1)>0$ I excluded those workers in level L+1 and all levels below L+1. Therefore, the mobility rates I analyze represent the mobility rates of the most reliable sample available for which I am relatively confident of having the universe of workers in levels L,L-1,...,2,1. Each observation is therefore a firm-year.

Table 1.15
The Determinants of Firm-level Job Mobility Rates

Variables	Dependent Variable: Job Mobility Rates Promote	Demote	Lateral w/ Job Change	Lateral w/o Job Change	Stay
Intercept	5.51	6.290**	10.34**	32.07***	45.79***
	(4.36)	(2.886)	(4.711)	(12.301)	(15.021)
Mean Experience	-0.007	-0.035	-0.100	0.068	0.075
	(0.097)	(0.064)	(0.104)	(0.273)	(0.333)
Mean Seniority	0.044	0.008	0.048	-0.359**	0.259
	(0.063)	(0.041)	(0.068)	(0.177)	(0.216)
% of Workforce w/ Schooling =16 Years of	-0.025	-0.061***	0.0048	-0.099	0.180***
	(0.024)	(0.016)	(0.026)	(0.069)	(0.084)
% of Workforce w/ Schooling =17 or 18 Years	0.014	-0.026	0.084***	-0.125*	0.052
	(0.026)	(0.017)	(0.028)	(0.072)	(0.088)
% of Workforce w/ Schooling >18 Years	-0.070**	-0.050**	-0.050	-0.299***	0.469***
	(0.031)	(0.020)	(0.033)	(0.086)	(0.105)
# of Hierarchical Levels in the Firm	0.047	0.035	-0.207*	0.578**	-0.454
	(0.109)	(0.072)	(0.118)	(0.308)	(0.376)
Average # of Subordinates per Worker	0.383***	0.341***	-0.08	-0.302	-0.342
	(0.140)	(0.092)	(0.151)	(0.395)	(0.481)
% of Workforce Bonus Eligible	0.0072	0.0062	0.001	0.042	-0.056*
	(0.0092)	(0.0061)	(0.010)	(0.026)	(0.032)

Table 1.15 (Continued)

% of Workforce Eligible for Long-Term Incentives	0.0085	0.0088**	0.0133*	-0.024	-0.0069
	(0.0066)	(0.0044)	(0.0071)	(0.019)	(0.023)
% of Workforce on the Board of Directors	0.0096	-0.012	-0.018	-0.137***	0.158***
	(0.013)	(0.0083)	(0.014)	(0.036)	(0.043)
Company Sales (in $100,000)	0.003	0.0005	-0.0008	0.018	-0.020
	(0.006)	(0.004)	(0.006)	(0.017)	(0.020)
Company Profits (in $100,000)	-0.048	-0.004	-0.045	-0.303	0.399
	(0.091)	(0.060)	(0.099)	(0.258)	(0.314)
Company Total Employees	-0.019**	-0.016***	-0.004	0.045**	-0.007
	(0.008)	(0.005)	(0.009)	(0.023)	(0.029)
Public Company, US Owned	-2.613*	-0.068	-0.113	-2.36	5.153
	(1.468)	(0.971)	(1.585)	(4.145)	(5.054)
Private Company, US Owned	-2.433	-0.248	-0.380	-6.23	9.291*
	(1.596)	(1.056)	(1.723)	(4.507)	(5.495)
Subsidiary Company, US Owned	5.587**	1.415	-0.085	-0.398	-6.518
	(2.741)	(1.813)	(2.959)	(7.739)	(9.435)
Mining	-.373	0.077	-0.166	1.128	-0.665
	(0.939)	(0.621)	(1.013)	(2.650)	(3.231)
Construction	2.287	0.583	-0.471	4.046	-6.446
	(1.628)	(1.077)	(1.758)	(4.598)	(5.605)
Transportation and Communications	-0.044	0.752*	1.147*	0.822	-2.677
	(0.638)	(0.422)	(0.689)	(1.801)	(2.196)
Wholesale Trade	-1.256	-0.672	-0.583	0.245	2.266
	(1.425)	(0.942)	(1.538)	(4.023)	(4.904)

Table 1.15 (Continued)

Retail Trade	-0.417	0.428	0.585	-1.099	0.503
	(1.657)	(1.096)	(1.788)	(4.678)	(5.703)
Finance	0.0406	-0.756	0.896	1.149	-1.694
	(0.898)	(0.594)	(0.969)	(2.534)	(3.090)
Services	-0.389	-0.430	1.663*	-2.759	1.915
	(0.913)	(0.604)	(0.986)	(2.579)	(3.144)
N	1453	1453	1453	1453	1453
R-Squared	0.037	0.035	0.037	0.089	0.082

Note: Models are estimated using OLS. An observation is a firm-year. The dependent variables are a percent ranging from 0-100. The independent variables measured as a percent have the same scaling. * indicates significance between .10 and 0.05; ** indicates significance between 0.05 and 0.01; *** indicates significance at better than 0.01.

correlated with the rate of lateral transitions that do not involve a change in job title. If those transitions are the result of job rotation, then this would be consistent with previous empirical results on the determinants of job rotation (Eriksson and Ortega, 2004; Ortega, 2001). Since the models control for firm size, the number of hierarchical levels accounts for the vertical (large number of levels) or horizontal (few levels) structure of the hierarchy. Firms that are more vertically oriented have fewer lateral transitions with job title changes and more lateral transitions without a job title change (transfers). This confirms expectations. For a given number of workers in a firm, a large number of hierarchical levels means there are fewer types of jobs in each level on average, whereas an accountant or marketing agent may be needed across several different business units or departments.

It should be noted that these models explain surprisingly little variation in firm-level mobility rates (R-squared between 0.03 and 0.09). This suggests there are important determinants that explain job mobility that are omitted from the model and captured in the residuals. One example would be a hypothetical variable called personnel policy that dictates the mobility patterns, conditional on the firm's workforce and company characteristics, but is unobserved to the econometrician. Another example would be a policy that dictates staffing decisions based on the firm's turnover rate (e.g., hire from the inside and high mobility rates or hire from the outside with low mobility rates). The firm's distribution of worker ability may also play a role, as well as its individual production technology. Therefore, the residuals of this model provide interesting information about how promotion rates, demotion rates and lateral mobility rates are correlated within firms.

Table 1.16 presents the cross-equation correlation matrix of the residuals from the models estimated in Table 1.15 along with significance tests for whether the correlation coefficient is equal to zero. Unobserved factors that explain promotion

Table 1.16
Cross Equation Correlations for the Residuals of the Model in Table 1.15

	Promote	Demote	Lateral w/ Job Change	Lateral w/o Job Change	Stay
Promote	1.000	0.187***	0.209***	0.164***	-0.527***
Demote		1.000	0.150***	0.029	-0.318***
Lateral w/ Job Change			1.000	-0.111***	-0.312***
Lateral w/o Job Change				1.000	-0.839***
Stay					1.000

Note: Null hypothesis is for $\rho=0$. * indicates significance between .10 and 0.05; ** indicates significance between 0.05 and 0.01; *** indicates significance at better than 0.01.

rates are correlated with all other transition rates, but the correlation between promotions and lateral transitions with a job title change is the strongest. Those factors that explain demotion rates are uncorrelated with lateral mobility without a job title change. Also, unobservables that explain lateral transitions with job title changes are negatively correlated with lateral transitions that have no change in job title (transfers). It appears that the residuals reflect unobservable factors that are capturing overall high or low mobility rates in firms; that is, firms that have high rates of job mobility for one type of transition, have high mobility rates in general.

1.6 Conclusion

Lateral mobility within firms is not a well-understood topic in the careers and organizations literature. Yet, the increasing use of job rotation as a human resource practice found in representative surveys such as Osterman (1994, 2000), and the relatively high lateral mobility rates found in this paper across a broad set of firms, suggest that more work is needed to understand both the mechanisms that generate these transitions as well as the impact they have on worker careers. Factors such as the roles of slot constraints, task- and job-specific capital, worker behavior and preferences over jobs, and the firm's organizational structure which may comprise of individual and separate career paths are all important aspects related to lateral mobility within firms that require more thought.

The motivational framework in this paper specifically highlights the role of transition costs in the firm's decision to assign workers to positions. Job changes are not costless, and these costs as well as the returns to standard human capital characteristics should be expected to vary across the range of potential assignment options available to the firm. Furthermore, in the presence of transition costs and lateral mobility, the firm's promotion decision is unlikely to be independent of the alternative assignment options in the choice set. In such cases, an economic model of the firm's assignment problem and its empirical implementation requires a more sophisticated set up that accounts for these interdependencies and the ability to substitute one assignment alternative for another.

A primary finding of this paper is that lateral job mobility is associated with significantly higher real earnings growth over those workers who are retained in the same job, but the growth in earnings is less than for those who are promoted. One potential explanation of this finding is that if worker performance is the mechanism sorting workers into jobs to begin with, then this positive relationship may reflect productivity differences rather than differences in the wages between the two jobs. While worker performance has a separate and strong, positive relationship to real earnings growth, the relationships between lateral mobility and real earnings growth remain when including measures of relative performance. That is, even when controlling for productivity differences, significant earnings growth occurs directly through the within-reporting-level change in jobs. This result reinforces the notion that jobs within-reporting levels are heterogeneous, and that the observed wage variation within reporting levels is in part due to the fact that the jobs within those levels are simply different.

Analyses of firm-level job mobility rates suggest there are firm-specific factors that impact the rate of lateral mobility such as the average seniority and education distribution of the workforce, and whether the firm is vertically or horizontally organized. These results provide some evidence that the lateral mobility observed in these data may be the result of job rotation. Since the explanatory power is relatively modest, it is likely that there are also important firm-specific and unobserved personnel policies that dictate these mobility rates such as a policy that, conditional on the firm and workforce characteristics, maps the firm's turnover rate into a decision to hire from the outside (low intra-firm mobility rates) or fill vacancies from within (high intra-firm mobility rates).

The job-to-job mobility analysis suggests that workers in these data are highly mobile - that is, job changes in the past are strong predictors of job changes in the future. Specifically, there is no real serial correlation for demotions and promotions, but lateral transitions with a job title change do predict future lateral transitions with a job title change (similar to job rotation). A lateral transition that does not involved a job title change (a simple transfer) is the least likely transition to be followed by another job change of any kind.

Finally, while the simplified structure of some organizations may negate the need to account for job transitions within levels, the hierarchical structure of other organizations can be very sophisticated. A unique aspect of this study is that the level of detail firms provide about their organizational structure yields a hierarchy that is more complex than a single file job ladder based on reporting alone, and identifies a number of job transitions that would otherwise be undetectable in other data sets. This paper shows that those transitions are meaningful. It is of interest, especially for future theoretical work, to understand the implications of modeling the internal workings of the firm in a way that incorporates more realistic hierarchies than a one dimensional job ladder and accounts for separate career paths and possible transitions between them.

CHAPTER 2

THE IMPACT OF INCENTIVES ON HUMAN BEHAVIOR: CAN WE MAKE IT DISAPPEAR? THE CASE OF THE DEATH PENALTY

2.1 Introduction

Economists are interested in the investigation of human behavior and how individuals respond to prices and incentives. Economic theory, which demonstrates an inverse relationship between the price of a commodity and its consumption, suggests that an increase in the price or cost of a behavior leads to a reduction in the intensity of that behavior. Therefore, as economic analysis of consumer behavior is applicable to any commodity ranging from apples to cars, it is also applicable to any type of human behavior, ranging from drunk driving to sexual activity to marital dissolution. Based on economic theory, an immense amount of empirical research has investigated the extent to which individuals alter their behavior in response to increases in the relevant "prices" that may impact that behavior.

2.1.1 Rationality and Reaction to Incentives

One common argument made by non-economists against the economic approach to human behavior is that people are not *rational enough* to behave according to the predictions of economic theory when it comes to behaviors such as smoking, consumption of alcohol and illicit drugs, sexual activity and crime. However, an enormous empirical literature in economics has demonstrated that even these behaviors are responsive to prices and incentives. For example, consumption of cigarettes declines when cigarette prices rise (e.g., Becker, Murphy and Grossman, 1994; Yurekli and Zhang 2000; Gruber, Sen, and Stabile, 2003), alcohol consumption is curtailed when alcohol prices are increased (e.g., Farrell, Manning and Finch 2003, Manning, Blumberg and Moulton 1995), drug use responds to variations in drug prices (e.g., van Ours 1995; Saffer and Chaloupka 1999; Grossman 2005), pregnancies and childbearing are influenced by state and federal policies that alter the costs (e.g. Mellor 1998; Lundberg and Plotnick 1995), and the timing of births within a year is responsive to the tax benefit of having a child (Dickert-Conlin and Chandra, 1999). Such results hold true even in sub-populations such as adolescents, who are thought to be present-oriented and less rational (e.g., Pacula et al. 2001; Gruber and Zinman 2001; Grossman and Chaloupka 1998; Grossman et al. 1994; Lundberg and Plotnick 1990), and among individuals with mental health problems (Saffer and Dave 2005). In a different vein, research in experimental economics has demonstrated that individuals respond to changes in prices as predicted by economic theory, and even children behave rationally when modifying their behavior in response to variations in prices (Harbaugh et al. 2001).

The same results are obtained from analyses of the response of criminal activity to the relevant costs and benefits. The pioneering work of Becker (1968) indicated that criminal activity should decline as the "price" of such activity increases. Empirical analyses testing the economic model of crime have demonstrated that illicit behavior indeed responds to incentives and sanctions. For example, Jacob and Levitt (2003) showed that incentives for high test scores motivated teachers and administrators to cheat on standardized tests in Chicago public schools. Corman and Mocan (2000, 2005) and DiTella and Schargrodsky (2004) demonstrated that increased arrests and more police officers reduce crime. Levitt (1998a) showed that juvenile crime goes down when punishment gets stiffer. Grogger (1998) and Mocan and Rees (2005) found that the extent of criminal involvement among high school students is influenced by both economic conditions and deterrence. Corman and

Mocan (2005) and Hansen and Machin (2002) showed that criminal activity reacts to increases in the minimum wage. Similarly, it has been shown that prison crowding, which generates early release of prisoners, has a significant impact on crime rates (Levitt 1996).

One specific sub-analysis in this domain has received significant attention. Specifically, the extent to which murder rates respond to deterrence was first investigated theoretically and empirically by Ehrlich (1973, 1975, 1977a), who found a deterrent effect of capital punishment. Some analysts questioned the robustness of the results (Hoenack and Weiler 1980; Passell and Taylor, 1977), and Ehrlich and others responded to these criticisms (Ehrlich 1977b, Ehrlich and Mark 1977, Ehrlich and Brower 1987, Ehrlich and Liu 1999). In a recent article Donohue III and Wolfers (2006) focused on a number of recent papers that reported a deterrent effect of death penalty on murder, and stated that the findings of these papers were not robust. The purpose of this paper is to provide a new and detailed analysis of the impact of leaving death row (executions, commutations and other removals from death row) on state murder rates. Specifically, we make various attempts to eliminate the deterrent effect of capital punishment and investigate if and under what conditions one succeeds in eliminating the impact of leaving death row on the murder rate.

As we demonstrate in detail below, the signaling effect of leaving death row and its impact on homicide is robust. Although the impact of executions sometimes disappears when one estimates specifications which are inconsistent with theory, the impact of commutations remains significant even in those models. Furthermore, as summarized in Table 2.13 and detailed in the paper, in many cases the deterrence results do not disappear even under many specifications that have no theoretical foundation.

2.2 *Data and the Empirical Model*

The data set used in the paper is the same one as employed by Donohue and Wolfers (2006) and Mocan and Gittings (2003). One distinguishing feature of the data set is that it contains the entire history of death sentences between 1997 and 1997, including the exact month of removal from death row and the reason for it (execution, commutation, etc.), for each death row inmate. The data on state-level crimes, arrests, prison population, prison deaths, and other state characteristics such as the unemployment rate, urbanization, racial composition of the state, and other attributes are compiled from various sources (see Mocan and Gittings 2003, p. 474-76).

The investigation of the impact of deterrence on homicide is carried out by estimating models of the following form:

$$M_{it} = D_{it-1}\,\alpha + X_{it}\,\beta + \mu_i + \eta_t + P_{it} + \varepsilon_{it,} \qquad (2.1)$$

where M_{it} is the murder rate in state *i* and year *t*. The vector **X** contains state characteristics that may be correlated with criminal activity, including the unemployment rate, real per capita income, the proportion of the state population in the following age groups: 20-34, 35-44, 45-54 and 55 and over, the proportion of the state population in urban areas, the proportion which is black, the infant mortality rate, the party affiliation of the governor, and the legal drinking age in the state. Theoretical and empirical justification for the inclusion of these variables can be found in Levitt (1998a) and Lott and Mustard (1997). The variable μ_i represents unobserved state-specific characteristics that impact the murder rate, which are controlled for by state fixed-effects, and η_t represents common year effects. To control for the impact of the 1995 Oklahoma City bombing, a dummy variable is included which takes the value of one in Oklahoma in 1995 and zero elsewhere. The models also include state-specific time-trends represented by P_{it}. Following Levitt (1998a) and Katz, Levitt and Shustorovich (2003), we also include the number of prisoners per violent crime and the prison death rate (a measure of prison conditions) as two additional measures of deterrence.

2.2.1 Measurement of risks (increase and decrease in the cost of murder)

The vector ***D*** represents deterrence variables, and includes the probability of apprehension, the probability of sentencing given apprehension, as well as various probabilities pertaining to leaving death row, conditional on sentencing. Note that execution is not the only outcome for prisoners on death row. During the period of 1977-97 (the time period analyzed in this paper), 17 percent of inmates who completed their duration on death row were executed while the other 83 percent left for other reasons (e.g. commutation of the sentence, sentence or conviction being overturned, sentence being found unconstitutional). This information allows for an investigation as to how the murder rate reacts to an increase in the price of crime (executions) as well as a decrease in the price of crime (commutation, and all removals other than executions and deaths).

From a theoretical point of view, it is important to carefully consider the timing of events. The probability of apprehension is a measure of the risk of getting caught, given that a murder is committed. Because the unit of analysis is state-year, this probability is measured as the proportion of murders cleared by an arrest in a particular state and year; i.e. $ARRATE_t = (AR_t / MUR_t)$, where AR_t is the number of murder arrests in a state in year t (state subscript is dropped for ease of exposition), and MUR_t stands for the number of murders in year t. The second risk variable is the probability of receiving a death sentence given that a murder arrest took place. After a person is arrested for murder, he/she does not automatically end up on death row;

instead, a trial takes place in which not all defendants are found guilty nor do they all receive a capital sentence. Therefore, one can calculate the probability of being found guilty and sentenced to death, conditional on being arrested for murder. The average duration between the date of a murder arrest and the date on which an inmate is sentenced to death is more than one year.[1] Thus, the risk of receiving the death sentence is defined as the number of death sentences handed out in a year divided by the number of murder arrests two years prior. That is, $SENTRATE_t = (SENT_t/AR_{t-2})$, where $SENT_t$ represents the number of death sentences handed out in year *t*.

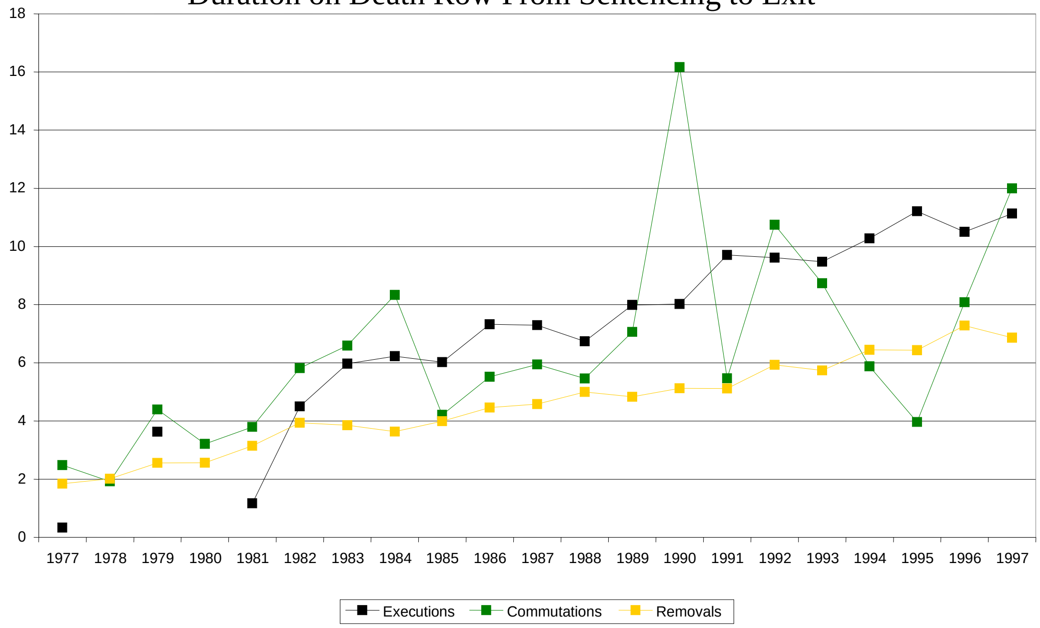
Following Mocan and Gittings (2003), three death penalty-related deterrence variables are created. When constructing the capital punishment variables it is useful to realize that if a person receives the death sentence, he/she is not executed instantly; instead, it has been demonstrated that the average duration from sentencing to execution (across states) is about six years during the period studied in this paper (Bedau 1982, Dezhbakhsh, Rubin and Shepherd 2003, Mocan and Gittings 2003, Argys and Mocan 2004). As was done in Mocan and Gittings (2003), this information suggests that the risk of execution should be calculated as the number of executions divided by the proper cohort of death sentences six years earlier; i.e. $EXEC_t/SENT_{t-6}$. Also, about 83 percent of the inmates are removed from death row for reasons other than execution. One such reason is commutation, where the inmate is granted clemency and the sentence is changed to a prison term, typically life. Because commutation implies a reduced risk of death, and therefore a reduced cost of committing murder, an increase in the probability of commutations should theoretically increase the murder rate. The same argument is true for all removals from death row (other than executions and other deaths while on death row). Figure

[1] For example, a person who is arrested in October 1990, is likely to receive a death sentence after February 1992.

2.1 displays the average duration on death row by execution, commutation and other removals from death row and shows that the proper cohort to use in calculating the risk of commutation and risk of removal is about the same as that for executions.[2]

Not all previous research has considered the relevant cohorts when calculating these risk variables. For example, Donohue III and Wolfers (2006) employ the data and methods of Mocan and Gittings (2003), but they create these variables as the ratio of executions (or removals) in a given year to the number of death sentences *in that*

Figure 2.1
Duration on Death Row From Sentencing to Exit



same year, i.e. as ($EXEC_t/SENT_t$) or ($REMOVE_t$ / $SENT_t$). These variables have no real meaning because the numerator and denominator of the ratio have no connection to each other: employing the ratio of executions in year *t* to the death sentences in year *t* incorrectly assumes that execution of each inmate takes place in the same year of sentencing. removals from death row and shows that the proper cohort to use in

[2] Note that the duration on death row for removals other than execution is less than that for executions and approximately 5 years on average. For this reason, Mocan and Gittings (2003) used the sentencing cohort 5 years ago in models that include removals; that is, ($EXEC_t/SENT_{t-5}$), or ($REMOVE_t$ / $SENT_{t-5}$).

calculating the risk of commutation and risk of removal is about the same as that for executions.[3]

Not all previous research has considered the relevant cohorts when calculating these risk variables. For example, Donohue III and Wolfers (2006) employ the data and methods of Mocan and Gittings (2003), but they create these variables as the ratio of executions (or removals) in a given year to the number of death sentences *in that same year*, i.e. as ($EXEC_t/SENT_t$) or ($REMOVE_t$ / $SENT_t$). These variables have no real meaning because the numerator and denominator of the ratio have no connection to each other: employing the ratio of executions in year *t* to the death sentences in year *t* incorrectly assumes that execution of each inmate takes place in the same year of sentencing.

Although calculating the risks this way is not sensible, it would be reasonable to ask if the results were sensitive to variations in their proper measurement. Specifically, we consider variations in the probability of execution, the probability of commutation, and the probability of removal from death row in three different dimensions and investigate if these variations make the deterrence results disappear. We deviate from the existing analyses of Mocan and Gittings (2003) ($EXEC_t/SENT_{t-6}$) and Donohue II and Wolfers (2006) ($EXEC_t/SENT_t$) and vary the sentencing cohort of the risk variables. For this exercise, we calculate the risks of execution, commutation and removals as ($EXEC_t/SENT_{t-5}$), ($COMM_t/SENT_{t-5}$), ($REMOVE_t/SENT_{t-5}$), assuming a five-year wait on death row, and ($EXEC_t/SENT_{t-4}$), ($COMM_t/SENT_{t-4}$), ($REMOVE_t/SENT_{t-4}$), assuming a four-year wait.

[3] Note that the duration on death row for removals other than execution is less than that for executions and approximately 5 years on average. For this reason, Mocan and Gittings (2003) used the sentencing cohort 5 years ago in models that include removals; that is, ($EXEC_t/SENT_{t-5}$), or ($REMOVE_t$ / $SENT_{t-5}$).

The discussion above concerns variations in the denominator of the risk variable, but proper measurement of the numerator is important as well. If executions, commutations or removals from death row send signals to potential criminals, then the timing of the signal needs to be addressed. An advantage of these data is the availability of the date of each execution and removal, which enables one to create execution, commutation and removal measures that are consistent with theory. Mocan and Gittings (2003) considered a monthly adjustment to the capital punishment events where executions, commutations and removals are prorated based on the month in which they occurred. For example, an execution which took place in January of 1980 can have an impact on the murder rate for the full year of 1980. However, if the execution took place in November 1980, it will have a trivial impact on the 1980 murder rate. Rather, the impact of this November execution on murder will primarily be felt in 1981. Thus, this November execution counts as 2/12 of an execution for 1980 and 10/12 of an execution for 1981. The same algorithms are applied for commutations and removals. We call these the first measure of executions, commutations and removals (EXEC, COMM, REMOVE).[4]

The second dimension to vary the measurement of the risk variables is through the numerator. We consider a means of allocating the capital punishment events that uses a coarser algorithm than previously described: If an execution took place within the first three quarters of a year, we attributed that execution to the same year. If the execution took place in the last quarter of a year (October-December) we attributed that execution to the following year under the assumption that the relative impact on murders would be felt in the following year. The same was done for removals and

[4] This is the measure employed by Mocan and Gittings (2003), and also Donohue III and Wolfers (2006).

commutations. We name these the second measures of executions, commutations and removals (EXEC2, COMM2 and REMOVE2).[5]

The third dimension in which we vary the risk measures is by experimenting with the wide range of other denominators to calculate the risk of leaving death row. Some of these measures have been used previously in the literature (e.g., executions per state population, executions per prison population) while others haven't, such as the total number of inmates on death row. Despite the fact that the measurement of these particular risk variables is inherently flawed, we incorporate them into the analysis to further examine the robustness of the results. Beyond measurement issues associated with the risk probabilities, we push the robustness check further by estimating these models across different samples (e.g. dropping various states) and using alternative weighting schemes.

2.3 Results

We estimate various versions of Equation (2.1). Following Corman and Mocan (2000), Levitt (1998a), Katz Levit and Shistorovich (2003), and Mocan and Gittings (2003), the deterrence variables are lagged by one year to minimize the concerns of simultaneity. For example, if the risk variable is ($EXEC_t/SENT_{t-5}$), its lagged value is employed in the regressions [i.e. $(EXEC_t/SENT_{t-5})_{-1} = (EXEC_{t-1}/SENT_{t-6})$]. The models are estimated by weighted-least squares, where the weights are state's share in the U.S. population. Robust standard errors, which are clustered at the state level, are reported in parentheses under the coefficients. In the interest of space, only the coefficients and standard errors pertaining to executions, commutations and removals are reported.

[5] In sensitivity tests below, we also employ other measures such as raw counts.

Table 2.1 displays the results where the first measures of execution, commutation and removal are employed. The top panel of Table 2.1 measures the relevant risks as ($EXEC_t/SENT_{t-5}$), ($COMM_t/SENT_{t-5}$), ($REMOVE_t/SENT_{t-5}$). That is, it calculates the rates of execution, commutation and removal per death sentences imposed 5 years earlier (assuming that the average duration on death row is 5 years). The models presented in the middle panel of Table 2.1 are identical, except, the average duration on death row is assumed to be 4 years. Thus, the variables are calculated as ($EXEC_t/SENT_{t-4}$), ($COMM_t/SENT_{t-4}$), and ($REMOVE_t/SENT_{t-4}$).[6]

A number of aspects of the results in Table 2.1 are noteworthy. First, the point estimates are very robust between specifications reported in the top two panels. Second, the execution rate has a negative and statistically significant impact on the murder rate. Third, the commutation and removal rates have positive impacts on the murder rate. Fourth, these results are consistent with the specifications reported in Mocan and Gittings (2003), despite utilizing different sentencing cohorts as the denominator.

The bottom panel of Table 2.1 displays the results of the model estimated by Donohue III and Wolfers (2006) using the same data. In this specification, the execution, commutation and removal rates are calculated by dividing executions, commutations and removals in a year to the number of death sentences *in that same year*. Thus, it is assumed that the duration on death row is less than one year. Similarly, in this specification, the sentencing rate is calculated as the ratio of death sentences in a year to murder arrests *in that same year*, assuming that the time length from arrest-to-trial-to-sentencing is also less than one year. Consequently, measuring

[6] Mocan and Gittings (2003) employed risk variables that take the average duration on death row as six years (denominator SENT lagged six years) in models for executions and commutations. Because the time between sentencing and REMOVE from death row is about 5 years, they employed SENT lagged five years in the denominator when the model included removals. Dohonue III and Wolfers (2006), on the other hand, use zero lags of SENT in the denominator.

Table 2.1
Determinants of the Murder Rate
The First Measure of Execution, Commutation, and Removal

Duration on death row: 5 years

$(EXEC_t/SENT_{t-5})_{-1}$	-0.0056** (0.0027)			-0.0058** (0.0028)	-0.0066** (0.0029)
$(COMM_t / SENT_{t-5})_{-1}$		0.0065 (0.0047)		0.0070 (0.0046)	
$(REMOVE_t/SENT_{t-5})_{-1}$			0.0024*** (0.0008)		0.0027*** (0.0009)
N	734	743	691	733	688

Duration on death row: 4 years

$(EXEC_t/SENT_{t-4})_{-1}$	-0.0054** (0.0022)			-0.0055** (0.0022)	-0.0047** (0.0021)
$(COMM_t/SENT_{t-4})_{-1}$		0.0036* (0.0021)		0.0038** (0.0019)	
$(REMOVE_t/SENT_{t-4})_{-1}$			0.0004 (0.0007)		0.0005 (0.0007)
N	785	790	744	781	741

Duration on Death Row: 0 Years; Time between Arrest and Death Sentence: 0 Years

$(EXEC_t/SENT_t)_{-1}$	0.0003 (0.0014)			0.0001 (0.0013)	0.0001 (0.0014)
$(COMM_t/SENT_t)_{-1}$		0.0041*** (0.0013)		0.0041*** (0.0013)	
$(REMOVE_t/SENT_t)_{-1}$			0.0002 (0.0003)		0.0002 (0.0003)
N	986	984	921	977	918

Note: See Section II for the explanation of the measurement of variables. Robust and clustered standard errors are in parentheses. * indicates statistical significance between 10 and 5 percent, ** statistical significance between 5 and 1 percent. *** statistical significance at the 1 percent level or better. [aa] Specification estimated by Donohue and Wolfers (2006).

the risk variables this way allows the execution result disappear, but the mis-specification cannot eliminate the impact of commutations on the murder rate.

Table 2.2 reports results obtained from models where the executions, commutations and removals are measured using the second set of variables that allocates events by the quarter in which they occur as described in Section II above. In other words, the only difference between results reported in Table 2.1 and Table 2.2 is the measurement of the numerator of the execution, commutation, and removal rates. Once again, the impact of the execution rates does not disappear, unless one estimates the specification promoted by Donohue and Wolfers (2006). And, even in that case, similar to Table 2.1, the impact of the commutation rate on the murder rate remains positive and statistically significant.

2.3.1 All Executions are in Texas!

It can be argued that California and Texas are interesting states which contain potentially useful information for establishing the deterrent effect of the death penalty, and it could be that the deterrence results in the literature may be sensitive to exclusion of Texas and California from the analysis. Of course, dropping an observation arbitrarily from the analysis is not very sensible, especially when the observation is known to contain information and when it is not an outlier. Nevertheless, dropping these two states and re-estimating the models provided results that are reported in Table 2.3. As the table demonstrates, the impact of executions and commutations or removals are still significant when Texas and California are omitted from the analysis.[7]

[7] We also omitted Texas and California individually. In neither case could we make the results disappear. See Mocan and Gitting (2006) pp. 38-9.

Table 2.2
Determinants of the Murder Rate
The Second Measure of Execution, Commutation, and Removal

Duration on death row: 5 years

$(EXEC_t/SENT_{t-5})_{-1}$	-0.0058*** (0.0020)			-0.0062*** (0.0022)	-0.0073*** (0.0022)
$(COMM_t\ /\ SENT_{t-5})_{-1}$		0.0044 (0.0047)		0.0056 (0.0040)	
$(REMOVE_t/SENT_{t-5})_{-1}$			0.0018*** (0.0007)		0.0021*** (0.0007)
N	737	743	712	736	709

Duration on death row: 4 years

$(EXEC_t/SENT_{t-4})_{-1}$	-0.0069* (0.0035)			-0.0070** (0.0035)	-0.0063* (0.0033)
$(COMM_t/SENT_{t-4})_{-1}$		0.0034* (0.0019)		0.0036** (0.0016)	
$(REMOVE_t/SENT_{t-4})_{-1}$			0.0002 (0.0008)		0.0005 (0.0007)
N	785	792	761	783	758

Duration on Death Row: 0 Years; Time between Arrest and Death Sentence: 0 Years

$(EXEC_t/SENT_t)_{-1}$	-0.0002 (0.0020)			-0.0001 (0.0019)	-0.00004 (0.0019)
$(COMM_t/SENT_t)_{-1}$		0.0039*** (0.0010)		0.0039*** (0.0001)	
$(REMOVE_t/SENT_t)_{-1}$			-0.0002 (0.0006)		-0.0002 (0.0006)
N	989	990	952	984	949

Note: See Section II for the explanation of the measurement of variables. Robust and clustered standard errors are in parentheses. * indicates statistical significance between 10 and 5 percent, ** statistical significance between 5 and 1 percent. *** statistical significance at the 1 percent level or better. [aa] Specification estimated by Donohue and Wolfers (2006).

Table 2.3
Determinants of the Murder Rate (Excluding Texas and California)

The First Measure of Executions, Commutations, and Removals Duration on Death Row: 5 years					
$(EXEC_t / SENT_{t-5})_{-1}$	-0.0029 (0.0019)			-0.0030** (0.0020)	-0.0041* (0.0023)
$(COMM_t / SENT_{t-5})_{-1}$		0.0048 (0.0043)		0.0051 (0.0042)	
$(REMOVE_t / SENT_{t-5})_{-1}$			0.0024*** (0.0008)		0.0026*** (0.0009)
n	704	713	662	703	659
Duration on Death Row: 4 years					
$(EXEC_t / SENT_{t-4})_{-1}$	-0.0041** (0.0019)			-0.0042** (0.0019)	-0.0036* (0.0018)
$(COMM_t / SENT_{t-4})_{-1}$		0.0042*** (0.0011)		0.0043** (0.0019)	
$(REMOVE_t / SENT_{t-4})_{-1}$			0.0007 (0.0007)		0.0008 (0.0007)
n	753	758	713	749	710
The Second Measure of Executions, Commutations, and Removals Duration on Death Row: 5 years					
$(EXEC2_t / SENT_{t-4})_{-1}$	-0.0039** (0.0016)			-0.0042** (0.0017)	-0.0054*** (0.0019)
$(COMM2_t / SENT_{t-4})_{-1}$		0.0037 (0.0040)		0.0046 (0.0034)	
$(REMOVE2_t / SENT_{t-4})_{-1}$			0.0018*** (0.0006)		0.0020*** (0.0007)
n	707	713	682	706	679
Duration on Death Row: 4 years					
$(EXEC2_t / SENT_{t-4})_{-1}$	-0.0055 (0.0034)			-0.0056* (0.0033)	-0.0051 (0.0031)
$(COMM2_t / SENT_{t-4})_{-1}$		0.0039*** (0.0011)		0.0040*** (0.0010)	
$(REMOVE2_t / SENT_{t-4})_{-1}$			0.0004 (0.0008)		0.0006 (0.0007)
n	753	760	730	751	727

Note: Robust and clustered standard errors are in parentheses. * indicates statistical significance between 10 and 5 percent, ** statistical significance between 5 and 1 percent. *** statistical significance at the 1 percent level or better.

2.3.2 *The Importance of the Denominator Once Again*

Why is it the case that omitting Texas does not make the results disappear despite the fact that Texas executes a disproportionately large number of death row inmates? One explanation is that it is incorrect to focus on execution counts when the correct measure is not the *number* of executions, but the *risk of the execution.* Despite the fact that a particular state has a large number of executions, the execution risk may not be high if the cohort of inmates that was sentenced to death is also large. Put differently, the number of executions needs to be adjusted by the appropriate denominator to obtain an actual measure of risk.

Table 2.4 summarizes the number of executions, commutation, and removals from death row between 1977 and 1997 for selected states; it also presents the average execution risk in each state during that period. The first measure is the number of executions in year *t* divided by number of death sentences 4 years earlier. The second measure deflates the number of executions by death sentences 5 years prior. The third and fourth measures displayed in the table are additional measures of risk used in the literature: the number of executions divided by prison population ($EXEC_t/PRISON_t$), and the number of executions deflated by the number of inmates on death row in the same year ($EXEC_t/ROW_t$), respectively. While Texas executes a large number of inmates annually, it is not the highest ranked state by any of these measures of execution risk. It is ranked 4th or 5th, depending on the risk measure, behind Virginia, Arkansas, and Louisiana. Missouri is generally ranked as the 5th. Therefore attempts to make the deterrence results disappear might be more productive if one were to omit high risk states rather than states with large absolute counts of executions.

Tables 2.5-2.7 present the results obtained from models when Virginia, Arkansas or Louisiana are dropped, respectively. In each case, dropping these states does not influence the results. That is, even when we remove the high-risk states from

Table 2.4
Execution Risk by State

	Exits from Death Row			Execution Risk				Execution Risk Ranking			
State	EXEC	COMM	REMV	$\frac{EXEC_t}{SENT_{t-4}}$	$\frac{EXEC_t}{SENT_{t-5}}$	$\frac{EXEC_t}{PRISON_t}$	$\frac{EXEC_t}{ROW_t}$	$\frac{EXEC_t}{SENT_{t-4}}$	$\frac{EXEC_t}{SENT_{t-5}}$	$\frac{EXEC_t}{PRISON_t}$	$\frac{EXEC_t}{ROW_t}$
AL	16	1	130	0.116	0.099	0.072	0.009	8	8	7	8
AR	16	1	46	0.49	0.327	0.157	0.031	2	2	3	3
GA	22	6	150	0.136	0.127	0.057	0.012	7	7	8	7
LA	24	2	78	0.345	0.315	0.226	0.056	3	3	1	2
MO	29	1	30	0.301	0.245	0.114	0.023	5	6	5	5
NV	6	3	32	0.079	0.08	0.072	0.007	10	9	6	9
OK	9	1	95	0.082	0.074	0.053	0.005	9	10	9	12
SC	13	3	49	0.28	0.31	0.05	0.014	6	4	10	6
TX	144	44	166	0.307	0.304	0.135	0.026	4	5	4	4
VA	46	5	15	0.612	0.652	0.162	0.059	1	1	2	1

Note: PRISON is the total number of prisoners in the state. ROW is the number of death row inmates. The numbers in the execution risk columns are average annual values for the states.

Table 2.5
Determinants of the Murder Rate (Excluding Virginia)

The First Measure of Execution, Commutation, and Removal					
Duration on Death Row: 5 years					
$(EXEC_t / SENT_{t-5})_{-1}$	-0.0066* (0.0035)			-0.0068* (0.0037)	-0.0084** (0.0036)
$(COMM_t / SENT_{t-5})_{-1}$)		0.0087** (0.0038)		0.0091** (0.0039)	
$(REMOVE_t / SENT_{t-5})_{-1}$			0.0025*** (0.0008)		0.0029*** (0.0010)
n	719	728	676	718	673
Duration on Death Row: 4 years					
$(EXEC_t / SENT_{t-4})_{-1}$	-0.0052** (0.0025)			-0.0052** (0.0025)	-0.0045* (0.0024)
$(COMM_t / SENT_{t-4})_{-1}$		0.0044*** (0.0016)		0.0045*** (0.0015)	
$(REMOVE_t / SENT_{t-4})_{-1}$			0.0004 (0.0007)		0.0005 (0.0007)
n	769	774	728	765	725
The Second Measure of Execution, Commutation, and Removal					
Duration on Death Row: 5 years					
$(EXEC2_t / SENT_{t-5})_{-1}$	-0.0063** (0.0026)			-0.0061** (0.0026)	- 0.0083*** (0.0024)
$(COMM2_t / SENT_{t-5})_{-1}$		0.0083*** (0.0030)		0.0083*** (0.0031)	
$(REMOVE2_t / SENT_{t-5})_{-1}$			0.0019*** (0.0007)		0.0023*** (0.0008)
n	722	728	697	721	694
Duration on Death Row: 4 years					
$(EXEC2_t / SENT_{t-4})_{-1}$	-0.0066 (0.0040)			-0.0067 (0.0040)	-0.0060 (0.0037)
$(COMM2_t / SENT_{t-4})_{-1}$		0.0043*** (0.0013)		0.0044*** (0.0012)	
$(REMOVE2_t / SENT_{t-4})_{-1}$			0.0003 (0.0008)		0.0005 (0.0007)
n	769	776	745	767	742

Note: Robust and clustered standard errors are in parentheses. * indicates statistical significance between 10 and 5 percent, ** statistical significance between 5 and 1 percent. *** statistical significance at the 1 percent level or better.

Table 2.6
Determinants of the Murder Rate (Excluding Arkansas)

The First Measure of Execution, Commutation, and Removal					
Duration on Death Row: 5 years					
$(EXEC_t / SENT_{t-5})_{-1}$	-0.0055* (0.0028)			-0.0056* (0.0029)	-0.0065** (0.0031)
$(COMM_t / SENT_{t-5})_{-1}$		0.0065 (0.0047)		0.0068 (0.0046)	
$(REMOVE_t / SENT_{t-5})_{-1}$			0.0023*** (0.0008)		0.0026*** (0.0009)
n	719	728	676	718	673
Duration on Death Row: 4 years					
$(EXEC_t / SENT_{t-4})_{-1}$	-0.0053 (0.0035)			-0.0054 (0.0035)	-0.0043 (0.0033)
$(COMM_t / SENT_{t-4})_{-1}$		0.0036* (0.0021)		0.0038** (0.0019)	
$(REMOVE_t / SENT_{t-4})_{-1}$			0.0004 (0.0008)		0.0005 (0.0008)
n	769	774	728	765	725
The Second Measure of Execution, Commutation, and Removal					
Duration on Death Row: 5 years					
$(EXEC2_t / SENT_{t-5})_{-1}$	-0.0058*** (0.0021)			-0.0063*** (0.0023)	-0.0074*** (0.0024)
$(COMM2_t / SENT_{t-5})_{-1}$		0.0044 (0.0047)		0.0057 (0.0040)	
$(REMOVE2_t / SENT_{t-5})_{-1}$			0.0017** (0.0007)		0.0021*** (0.0007)
n	722	728	697	721	694
Duration on Death Row: 4 years					
$(EXEC2_t / SENT_{t-4})_{-1}$	-0.0064 (0.0039)			-0.0065 (0.0039)	-0.0057 (0.0036)
$(COMM2_t / SENT_{t-4})_{-1}$		0.0034* (0.0019)		0.0036** (0.0016)	
$(REMOVE2_t / SENT_{t-4})_{-1}$			0.0002 (0.0008)		0.0005 (0.0008)
n	769	776	745	767	742

Note: Robust and clustered standard errors are in parentheses. * indicates statistical significance between 10 and 5 percent, ** statistical significance between 5 and 1 percent. *** statistical significance at the 1 percent level or better.

Table 2.7
Determinants of the Murder Rate (Excluding Louisiana)

The First Measure of Execution, Commutation, and Removal Duration on Death Row: 5 years					
$(EXEC_t / SENT_{t-5})_{-1}$	-0.0059 (0.0035)			-0.0063* (0.0036)	-0.0071* (0.0038)
$(COMM_t / SENT_{t-5})_{-1}$		0.0080* (0.0041)		0.0086** (0.0041)	
$(REMOVE_t / SENT_{t-5})_{-1}$			0.0022** (0.0009)		0.0024** (0.0009)
n	720	728	678	719	675
Duration on Death Row: 4 years					
$(EXEC_t / SENT_{t-4})_{-1}$	-0.0041* (0.0023)			-0.0042* (0.0023)	-0.0041* (0.0024)
$(COMM_t / SENT_{t-4})_{-1}$		0.0041*** (0.0015)		0.0042*** (0.0014)	
$(REMOVE_t / SENT_{t-4})_{-1}$			0.0006 (0.0007)		0.0008 (0.0007)
n	770	774	730	766	727
The Second Measure of Execution, Commutation, and Removal Duration on Death Row: 5 years					
$(EXEC2_t / SENT_{t-5})_{-1}$	-0.0060** (0.0026)			-0.0067** (0.0026)	-0.0063** (0.0026)
$(COMM2_t / SENT_{t-5})_{-1}$		0.0058 (0.0043)		0.0072** (0.0034)	
$(REMOVE2_t / SENT_{t-5})_{-1}$			0.0017** (0.0008)		0.0018** (0.0008)
n	722	728	699	721	696
Duration on Death Row: 4 years					
$(EXEC2_t / SENT_{t-4})_{-1}$	-0.0039 (0.0027)			-0.0040 (0.0027)	-0.0039 (0.0028)
$(COMM2_t / SENT_{t-4})_{-1}$		0.0039*** (0.0014)		0.0040*** (0.0013)	
$(REMOVE2_t / SENT_{t-4})_{-1}$			0.0005 (0.0007)		0.0006 (0.0007)
n	770	776	747	768	744

Note: Robust and clustered standard errors are in parentheses. * indicates statistical significance between 10 and 5 percent, ** statistical significance between 5 and 1 percent. *** statistical significance at the 1 percent level or better.

the analysis, the results are still robust. This may not be all that surprising, as the coefficients are estimated through within state variation when including state fixed effects.

This analysis shows that attempts to make the deterrence results disappear are ineffective. Even if one estimates an unusual specification that takes the numerator and denominator of the risk variables contemporaneously (in the bottom panels of Table 2.1 and Table 2.2) the estimated impact of executions becomes statistically insignificant, but the positive impact of commutations on the murder rate does not disappear.

2.4 The Impact of Death Penalty Laws

Donohue III and Wolfers (2006) argued that the murder rates were higher in Kansas and New Hampshire after these states adopted the death penalty; lower in New York and New Jersey after their adoption of the death penalty; and that murder rates declined in Massachusetts and Rhode Island after these states abolished the death penalty. We estimated various models in an effort to substantiate this statement. Because they indicate the impact of the death penalty laws are estimated separately for each of the mentioned states while controlling for the same variables as in the main specification, we estimated models separately for Massachusetts, Rhode Island, Kansas, New Hampshire, New York and New Jersey.

For each state a dummy variable is created that takes the value of one if the death penalty is legal, and zero otherwise. Kansas legalized the death penalty in 1994. New Hampshire legalized it in 1991. Legalization took place in 1982 and 1995 for New Jersey and New York, respectively. Massachusetts and Rhode Island abolished the death penalty in 1984.[1] Because the sample runs from 1977 to 1997, estimating regressions for each state separately is complicated by a degrees-of-freedom problem. The results are summarized in Table 2.8. The reported coefficients pertain to a lagged dummy variable indicating the legality of the death penalty.[2]

TABLE 2.8
The Impact of the Death Penalty on the Murder Rate

The Coefficient (std err) of *Death Penalty Legal (t-1)*					
	(1)	(2)	(3)	(4)	(5)
Kansas	-0.0214 (0.0220)	-0.0044 (0.0183)	-0.0007 (0.0061)	-0.008* (0.0040)	-0.0011 (0.0033)
New Hampshire	-0.0226 (0.0119)	-0.0253** (0.0099)	-0.0125 (0.0105)	-0.0206** (0.0080)	-0.0213** (0.0078)
Massachusetts	-0.0055 (0.0060)	-0.0059 (0.0048)	-0.0082* (0.0045)	-0.0075 (0.0065)	-0.0066 (0.0051)
Rhode Island	-0.0087 (0.0046)	-0.0051 (0.0096)	-0.0043 (0.0070)	-0.0034 (0.0076)	-0.0063 (0.0067)
New York	0.0087 (0.0183)	0.0165 (0.0122)	0.0113 (0.0125)	0.0119 (0.0108)	0.0145 (0.0194)
New Jersey	-0.0101 (0.0140)	-0.009* (0.0037)	-0.0085*** (0.0017)	-0.0132** (0.0036)	-0.0132** (0.0030)

Note: Each cell reports the coefficient (standard error) of Death Penalty Legal (t-1) variable in the murder rate regressions. Robust and clustered standard errors in parentheses. * statistical significance between 10 and 5 percent, ** statistical significance between 5 and 1 percent. *** statistical significance at the 1 percent level or better.

As the table shows, inclusion or exclusion of control variables has no substantial impact on the estimated coefficients of legal death penalty indicator. In these regressions, the coefficient of the death penalty indicator is not statistically

[1] Massachusetts abolished the death penalty in October 1984. Thus, 1985 is the first year with no death penalty in Massachusetts in the data since abolishment took place. Similarly, 1985 is the first full year where the death penalty is illegal in Rhode Island.

[2] Complete set of results can be found in Mocan and Gittings (2006). The number of control variables differs between the specifications to investigate the sensitivity. The sentencing rate could only be included in the regressions for New Jersey, because there is no variation in the number of death sentences in the five other states. Similarly, the drinking age cannot be included in the models.

different from zero in Rhode Island and New York. It is negative and significant in New Hampshire and New Jersey. In Kansas and Massachusetts, the coefficients are always negative, and significant in one specification for each state.

2.4.1 Evidence from Panel Data

In this section, we investigate whether the existence of the death penalty in a state has a separate impact on the murder rate in addition to the risks associated with being on the death row. To that end, we estimated the same models as those presented in Tables 2.1 and 2.2, but we added a dichotomous indicator if death penalty is legal in a given state in a particular year. Furthermore, we interacted this dummy variable with the execution rate, commutation rate and removal rate variables.

The results are displayed in Tables 2.9 and 2.10, where the two alternative measures of execution, commutation and removal risks are employed. In each case, models are estimated with 4 and 5-lags of the death sentences in the denominator of the risk variables as before. The results demonstrate that the existence of the death penalty in a state has a negative and statistically significant impact on the murder rate. In addition, the execution rate has a negative impact on the murder rate, and commutations and removals have a positive impact, although not always statistically significant.

2.5 The Denominator of the Risk Variables Again

Individuals do not exit the death row in the same year as they received the death sentence. To make the point more visible, the average duration on death row is calculated each year for those inmates who are removed that year, and plotted in Figure 2.1 by the reason of exit. As can be inferred, individuals who were commuted, executed or otherwise removed from death row had spent an average of about six

Table 2.9
Determinants of the Murder Rate

The First Measure of Execution, Commutation, and Removal					
	(1)	(2) $SENT_{t-4}$	(3) $SENT_{t-4}$	(4) $SENT_{t-5}$	(5) $SENT_{t-5}$
Death Penalty Legal (-1).	-0.0152**	-0.0148**	-0.0123**	-0.0135**	-0.0116**
	(0.0063)	(0.0060)	(0.0056)	(0.0064)	(0.0056)
Murder Arrest Rate (-1)	-0.0009	-0.0019	-0.0020	-0.0028	-0.0021
	(0.0032)	(0.0026)	(0.0024)	(0.0026)	(0.0026)
Sentencing Rate (-1)	-0.0026	0.0093	0.0112	-0.0105	-0.0171
	(0.0216)	(0.0222)	(0.0236)	(0.0198)	(0.0198)
Prisoners per Violent Crime (-1)	-0.0401***	-0.0397***	-0.0378***	-0.0391***	-0.0375***
	(0.0087)	(0.0083)	(0.0085)	(0.0086)	(0.0087)
Death Penalty Legal (-1) x		-0.0056**	-0.0050**	-0.0061**	-0.0069**
Execution Rate (-1)		(0.0022)	(0.0020)	(0.0028)	(0.0029)
Death Penalty Legal (-1) x		0.0038**		0.0067	
Commutation Rate(-1)		(0.0019)		(0.0046)	
Death Penalty Legal (-1) x			0.0005		0.0028***
Removal Rate (-1)			(0.0007)		(0.0009)
n	894	781	741	733	688

Note: The column headings $SENT_{t-4}$ and $SENT_{t-5}$ mean that execution, commutation and removal rates are calculated by deflating $EXEC_t$, $COMM_t$ and $REMOVE_t$ by $SENT_{t-4}$ or $SENT_{t-5.}$ Robust and clustered standard errors are in parentheses. * indicates statistical significance between 10 and 5 percent, ** statistical significance between 5 and 1 percent. *** statistical significance at the 1 percent level or better.

Table 2.10

Determinants of the Murder Rate

The Second Measure of Execution, Commutation, and Removal					
	(1)	(2) $SENT_{t-4}$	(3) $SENT_{t-4}$	(4) $SENT_{t-5}$	(5) $SENT_{t-5}$
Death Penalty Legal (-1).	-0.0152**	-0.0147**	-0.0126**	-0.0136**	-0.0131**
	(0.0063)	(0.0060)	(0.0056)	(0.0064)	(0.0057)
Murder Arrest Rate (-1)	-0.0009	-0.0018	-0.0028	-0.0029	-0.0028
	(0.0032)	(0.0026)	(0.0028)	(0.0026)	(0.0028)
Sentencing Rate (-1)	-0.0026	0.0092	0.0121	-0.0069	-0.0105
	(0.0216)	(0.0222)	(0.0237)	(0.0209)	(0.0199)
Prisoners per Violent Crime (-1)	-0.0401***	-0.0398***	-0.0387***	-0.0399***	-0.0388***
	(0.0087)	(0.0082)	(0.0082)	(0.0085)	(0.0085)
Death Penalty Legal (-1) x		-0.0070**	-0.0064*	-0.0064***	-0.0075***
Execution Rate (-1)		(0.0035)	(0.0032)	(0.0021)	(0.0022)
Death Penalty Legal (-1) x		0.0036**		0.0054	
Commutation Rate(-1)		(0.0016)		(0.0040)	
Death Penalty Legal (-1) x			0.0005		0.0022***
Removal Rate (-1)			(0.0007)		(0.0007)
n	894	783	758	736	709

Note: The column headings $SENT_{t-4}$ and $SENT_{t-5}$ mean that execution, commutation and removal rates are calculated by deflating $EXEC_t$, $COMM_t$ and $REMOVE_t$ by $SENT_{t-4}$ or $SENT_{t-5}$. Robust and clustered standard errors are in parentheses. * indicates statistical significance between 10 and 5 percent, ** statistical significance between 5 and 1 percent. *** statistical significance at the 1 percent level or better.

years on death row. On the other hand, those who were executed or commuted in 1997 had completed about 11 years on death row. Given this picture, one can use time-varying durations on death row to calculate the risks of execution, commutation or removals. For example, the execution risk in year 1981 can be calculated as the number of executions in 1981 divided by the number of death sentences in 1980 (because the duration on death row was one year in 1981). On the other hand, the risk of execution in 1990 can be measured as the number of executions in 1990 divided by the number of death sentences in 1982 (because the average duration on death row for those who were executed in 1990 was 8 years. See Figure 10). More generally, the execution, commutation and removal rates are calculated as ($EXEC_t$ / $SENT_{t-i}$), ($COMM_t$ / $SENT_{t-j}$), and ($REMOVE_t$ / $SENT_{t-k}$), where i, j and k are average durations on death row for spells ending in year t for executions, commutations and removals, respectively. Calculating the risks this way produced the results displayed in Table 2.11. Once again, we are unsuccessful in eliminating the impact of the execution risk on the murder rate.[1]

Some researchers calculated the execution risk as the number of executions in a year divided by the number of prisoners in that state in that year (e.g. Katz et al. 2003). This calculation assumes that every prisoner in state correctional facilities is at risk of being executed. This assumption has little validity as about 99.7 percent of the inmates in state prisons are incarcerated for non-capital offenses, and therefore they are not at risk of being executed. The difference is not simply a matter of scaling. The number of total prisoners to the number of death row inmates is not a constant

[1] Another extreme is to uniformly increase the lag length of the denominator. For example, when lag-length seven is imposed the same results are obtained, but not surprisingly, the statistically significance is lowered.

Table 2.11
Determinants of the Murder Rate
With Time Varying Durations on Death Row

The First Measure of Execution, Commutation, and Removal					
$(EXEC_t / SENT_{t-i})_{-1}$	-0.0058*			-0.0058*	-0.0055*
	(0.0031)			(0.0034)	(0.0029)
$(COMM_t / SENT_{t-j})_{-1}$		0.0014		0.0009	
		(0.0064)		(0.0067)	
$(REMOVE_t/SENT_{t-k})_{-1}$			0.0003		0.0001
			(0.0008)		(0.0008)
N	830	642	784	629	773
The Second Measure of Execution, Commutation, and Removal					
$(EXEC2_t / SENT_{t-i})_{-1}$	-0.0049*			-0.0050	-0.0049*
	(0.0026)			(0.0032)	(0.0027)
$(COMM2_t / SENT_{t-j})_{-1}$		0.0009		0.0004	
		(0.0054)		(0.0059)	
$(REMOVE2_t/SENT_{t-k})_1$			0.0007		0.0006
			(0.0007)		(0.0007)
n	833	643	806	632	797

Note: . Robust and clustered standard errors are in parentheses. * indicates statistical significance between 10 and 5 percent, ** statistical significance between 5 and 1 percent. *** statistical significance at the 1 percent level or better. i, j and k are average durations on death row for spells ending in year t for executions, commutations and removals, respectively.

proportion over time or across states.[2] Nevertheless, the results when introducing the total number of prisoners as the denominator is provided in Table 2.12. Although this inaccurate measure makes the impact of commutations disappear, it cannot make the impact of executions go away.

A more appropriate way of calculating the risk of execution would be to use the ratio of executions to the number of inmates on death row rather than deflating by the prison population in the state, although this measure is still inappropriate since a particular death row inmate is not at risk of execution if he just entered death row. Nevertheless, deflating by the stock of death row inmates is much more reasonable

[2] For example, in 1997 there were a total of 1,127,686 inmates in state prisons, and there were 3,328 death row inmates. The number of total prisoners was 1,316,302 in 2004 and the number of people on death row was 3,314 in the same year.

Table 2.12
Determinants of the Murder Rate

The First Measure of Execution, Commutation, and Removal deflated Total Prisoners/1,000					
$(EXEC_t / PRIS_t)_{-1}$	-0.0258** (0.0101)			-0.0255** (0.0102)	-0.0257 ** (0.0101)
$(COMM_t / PRIS_t)_{-1}$		0.0085 (0.0077)		0.0075 (0.0083)	
$(REMOVE_t / PRIS_t)$			0.0007 (0.0008)		0.0006 (0.0008)
n	894	894	894	894	894
The Second Measure of Execution, Commutation, and Removal deflated by Total Prisoners/1,000					
$(EXEC2_t / PRIS_t)_{-1}$	-0.0208** (0.0083)			-0.0206** (0.0083)	-0.0208** (0.0083)
$(COMM2_t / PRIS_t)_{-1}$		0.0065 (0.0067)		0.0056 (0.0073)	
$(REMOVE2_t / PRIS_t)$			0.0003 (0.0007)		0.0028 (0.0007)
n	894	894	894	894	894

Note: PRIS stands for the number of total prisoners. Each model includes the variables in the footnote to Table 2.7.

than deflating by total prisoners. Results obtained from this exercise are reported in Table 2.13. Once again, executions have a negative impact on the murder rate in the state and commutations are positively related to murder.

Two other denominators are promoted as deflators to the number of executions. For example, Donohue III and Wolfers (2006, p. 815) write "A very simple alternative that avoids this scaling issue is measuring executions per 100,000 residents." They also write: "Another alternative scaling –and perhaps the one most directly suggested by the economic model of crime—is to analyze the ratio of the number of executions to the (lagged) homicide rate." (p. 815). Although it is evident that these suggested measures are poor indicators of the relevant risks, we estimated the models with these denominators as well. The first panel of Table 2.14 displays the results when the annual count of executions, commutations and removals are deflated

Table 2.13
Determinants of the Murder Rate

The First Measure of Execution, Commutation, and Removal deflated by Death Row Inmates					
$(EXEC_t / ROW_t)_{-1}$	-0.0465* (0.0277)			-0.0463* (0.0276)	-0.0466 (0.0284)
$(COMM_t / ROW_t)_{-1}$		0.0098*** (0.0014)		0.0097*** (0.0015)	
$(REMOVE_t / ROW_t)$			-0.0026 (0.0062)		-0.0021 (0.0062)
n	894	894	890	894	890
The Second Measure of Execution, Commutation, and Removal deflated by Death Row Inmates					
$(EXEC2_t / ROW_t)_{-1}$	-0.0501* (0.0287)			-0.0500* (0.0285)	-0.0485 (0.0298)
$(COMM2_t / ROW_t)_{-1}$		0.0084*** (0.0017)		0.0083*** (0.0017)	
$(REMOVE2_t / ROW_t)$			-0.0043 (0.0051)		-0.0039 (0.0052)
n	894	894	893	894	893

Note: ROW stands for the number of death raw inmates. Each model includes the variables listed in the footnote to Table 2.13

by state population, and the second panel presents the results when they are deflated by lagged homicide rate.

Note that the dependent variable for the analysis is the murder rate, which is measured as murders deflated by population; thus, deflating executions by the state population means that population enters into the denominator of both the dependent and independent variables, inducing a positive bias in the estimated coefficient of the execution rate. Nevertheless, the coefficient of the execution rate remains negative and significant. Because the dependent variable of the analysis is the murder rate, to use the murder rate as the deflator of executions is not meaningful either.[3] However, as the

[3] Donohue III and Wolfers seem to recognize this, and write that in their analysis they employ the lagged homicide rate as the deflator (Donohue and Wolfers 2006, ft. 63). However, if the homicide rate has any path-dependence, such as a simple AR(1) model, using the lagged-dependent variable in the denominator of the independent variable does not avoid a bias.

second panel of Table 2.15 demonstrates, using the lagged murder rate as the denominator did not make the results disappear.

Table 2.14
Determinants of the Murder Rate

The Raw Count of Executions, Commutations, and Removals Deflated by Population/100,000					
$(\#EX_t / POP_t)_{-1}$	-0.055*			-0.0055*	-0.0051*
	(0.0281)			(0.028)	(0.0028)
$(\#C_t / POP_t)_{-1}$		0.0099		0.0011	
		(0.0212)		(0.020)	
$(\#R_t / POP_t)_{-1}$			0.0037		0.0037
			(0.0061)		(0.0063)
n	894	894	894	894	894
The Raw Count of Executions, Commutations, and Removals Deflated by Lagged Murder Rate x 1000					
$(\#EX_t / MURDER_{t-1})_1$	-0.0543**			-0.0542**	-0.0543**
	(0.0251)			(0.0022)	(0.0021)
$(\#C_t / MURDER_{t-1})_{-1}$		-0.0120		-0.0098	
		(0.0254)		(0.0252)	
$(\#R_t / MURDER_{t-1})_{-1}$			-0.0004		0.0001
			(0.0122)		(0.0127)
n	894	894	894	894	894

Note: $\#EX_t$ denotes the raw counts of executions. $\#C_t$ denotes the raw counts of commutations, and $\#R_t$ stands for the raw counts of death row removals. POP is the population in the state. MURDER is the murder rate. Robust and clustered standard errors are in parentheses. * indicates statistical significance between 10 and 5 percent, ** statistical significance between 5 and 1 percent. *** statistical significance at the 1 percent level or better.

2.6 Further Attempts to Make the Results Disappear

The risk measures employed in this paper are calculated such that if there is an execution in a given state in a given year, but if it so happens that no individual received a capital sentence five years prior, then the risk ($EXEC_t/SENT_{t-5}$) is set to missing because the denominator is zero. On the other hand, in cases where nobody was sentenced and nobody was executed, the execution risk was taken as zero.

One can adopt an algorithm where observations are dropped from the data when the corresponding executions and death sentences are both zero. This algorithm assumes that the risks cannot be calculated in situations when they should be zero, such as the cases where there is no legal death penalty. Even so, and despite the fact that this algorithm eliminates about half of the legitimate observations, the impact of the death penalty on the murder rate remains as shown in Tables 2.15 and 2.16.

What happens to the results if we go to the extreme where the count of executions, commutations and removals are considered as appropriate signals to individuals, rather than the rates at which they occur (as defined by the correct denominator)? While we do not agree that this is the correct specification, estimation of this model showed that even this modification does not eliminate the impact of prices on human behavior. Although the coefficients of commutations and removals become statistically insignificant, the coefficient of execution remains significant even in this model.[4]

It may be possible that the deterrent impact of the death penalty which exists in states with large populations such as New York and New Jersey exerts disproportionate influence in a population-weighted regression and overwhelms the no-deterrence result that would have been obtained in regressions with no weighting. To investigate if the results are driven by this hypothesis, we estimated the models presented in Tables 2.1 and 2.2 without population weights.[5] In models where the duration of death row is taken as 5 years, the results are actually stronger with the coefficients of the commutation rate being statistically significant. In the models where the duration of death row is taken as 4 years, the execution rate is insignificant, but the removal rate becomes significant when it was insignificant in the weighted regression displayed in Tables 2.1 and 2.2. Finally, the results of the regression

[4] The results are reported in Mocan and Gittings (2006) p.59.
[5] The results, which are not reported in the interest of space, can be found in Mocan and Gittings (2006), pp. 60-1.

Table 2.15
Determinants of the Murder Rate
Dropping Observations Where Risk is Not Well Defined.
The First Measure of Execution, Commutation, and Removal

Duration on death row: 5 years					
$(EXEC_t / SENT_{t-5})_{-1}$	-0.0043			-0.0045‡	-0.0061**
	(0.0027)			(0.0029)	(0.0026)
$(COMM_t / SENT_{t-5})_{-1}$		0.0057		0.0061	
		(0.0050)		(0.0050)	
$(REMOVE_t/SENT_{t-5})_1$			0.0022**		0.0025***
			(0.0008)		(0.0009)
n	398	398	398	398	398
Duration on death row: 4 years					
$(EXEC_t / SENT_{t-4})_{-1}$	-0.0053**			-0.0053*	-0.0054**
	(0.0022)			(0.0022)	(0.0021)
$(COMM_t / SENT_{t-4})_{-1}$		0.0018		0.0019	
		(0.0025)		(0.0023)	
$(REMOVE_t/SENT_{t-4})_1$			0.0002		0.0003
			(0.0006)		(0.0006)
n	426	426	426	426	426
Donohue III & Wolfers Specification Duration on Death Row: 0 Years; Time between Arrest and Death Sentence: 0 Years					
$(EXEC_t / SENT_t)_{-1}$	0.0000			-0.0000	-0.0000
	(0.0012)			(0.0012)	(0.0013)
$(COMM_t / SENT_t)_{-1}$		0.0034*		0.0034*	
		(0.0019)		(0.0013)	
$(REMOVE_t / SENT_t)_-$			0.0004		0.0004
			(0.0003)		(0.0003)
n	543	543	543	543	543

Note: Observation are dropped when risk=0 and denominator=0. Robust and clustered standard errors are in parentheses. * indicates statistical significance between 10 and 5 percent, ** statistical significance between 5 and 1 percent. *** statistical significance at the 1 percent level or better.

Table 2.16
Determinants of the Murder Rate
Dropping Observations Where Risk is Not Well Defined.
The Second Measure of Execution, Commutation, and Removal

Duration on death row: 5 years					
$(EXEC_t / SENT_{t-5})_{-1}$	-0.0052** (0.0022)			-0.0058** (0.0023)	-0.0068*** (0.0024)
$(COMM_t / SENT_{t-5})_{-1}$		0.0041 (0.0045)		0.0054 (0.0037)	
$(REMOVE_t / SENT_{t-5})_{-1}$			0.0017** (0.0006)		0.0020*** (0.0007)
n	398	398	398	398	398
Duration on death row: 4 years					
$(EXEC_t / SENT_{t-4})_{-1}$	-0.0069* (0.0035)			-0.0069* (0.0036)	-0.0071** (0.0034)
$(COMM_t / SENT_{t-4})_{-1}$		0.0019 (0.0021)		0.0021 (0.0019)	
$(REMOVE_t / SENT_{t-4})_{1}$			0.00002 (0.0007)		0.0003 (0.0006)
n	426	426	426	426	426
Donohue III & Wolfers Specification Duration on Death Row: 0 Years; Time between Arrest and Death Sentence: 0 Years					
$(EXEC_t / SENT_t)_{-1}$	-0.0006 (0.0020)			-0.0007 (0.0020)	-0.00005 (0.0019)
$(COMM_t / SENT_t)_{-1}$		0.0034** (0.0013)		0.0034** (0.0013)	
$(REMOVE_t / SENT_t)_{-}$			-0.0005 (0.0005)		-0.0005 (0.0005)
n	543	543	543	543	543

Note: Observation are dropped when risk=0 and denominator=0. Robust and clustered standard errors are in parentheses. * indicates statistical significance between 10 and 5 percent, ** statistical significance between 5 and 1 percent. *** statistical significance at the 1 percent level or better.

estimated by Donohue and Wolfers (2006) using contemporaneous numerators and denominators remain unchanged whether the regressions are weighted or not.

In Table 2.17 we present the results obtained from the models that exclude New York and New Jersey, and estimate the models without weighting. As can be seen, the impact of leaving the death row on the murder rate cannot be eliminated by dropping New York and New Jersey from the analysis and running the regressions with no weighting. The same conclusion is obtained, when we ran the models displayed in Tables 2.2-2.6 with no weights. Thus, the results are not an artifact of weighting.[6]

2.7 Ph.D. Economists versus Criminals

In his Nobel lecture, Gary Becker described his inspiration for modeling economic behavior of crime as follows.

> "I began to think about crime in the 1960s after driving to Columbia University for an oral examination of a student in economic theory. I was late and had to decide quickly whether to put the car in a parking lot or risk getting a ticket for parking illegally on the street. I calculated the likelihood of getting a ticket, the size of the penalty, and the cost of putting the car in a lot. I decided it paid to take the risk and park on the street. (I did not get a ticket.)
>
> As I walked the few blocks to the examination room, it occurred to me that the city authorities had probably gone through a similar analysis. The frequency of their inspection of parked vehicles and the size of the penalty imposed on violators should depend on their estimates of the type of calculations potential violators like me would make." (Becker 1992, p.42).

[6] Dezhbakhsh and Rubin (2007) conduct extensive analyses on similar issues as well as others to investigate the sensitiv

Table 2.17
Determinants of the Murder Rate (Excluding New York and New Jersey)
Unweighted Regressions

The First Measure of Execution, Commutation, and Removal Duration on Death Row: 5 years					
$(EXEC_t / SENT_{t-5})_{-1}$	-0.0043**			-0.0044**	-0.0056**
	(0.0022)			(0.0021)	(0.0025)
$(COMM_t / SENT_{t-5})_{-1}$		0.0077**		0.0079***	
		* (0.0022)		(0.0021)	
$(REMOVE_t / SENT_{t-5})_{-1}$			0.0027***		0.0030***
			(0.0008)		(0.0009)
n	704	713	665	703	662
Duration on Death Row: 4 years					
$(EXEC_t / SENT_{t-4})_{-1}$	-0.0038			-0.0036	-0.0033*
	(0.0023)			(0.0023)	(0.0022)
$(COMM_t / SENT_{t-4})_{-1}$		0.0050**		0.0049***	
		* (0.0007)		(0.0007)	
$(REMOVE_t / SENT_{t-4})_{-1}$			0.0017**		0.0018**
			(0.0008)		(0.0008)
n	753	758	716	749	713
The Second Measure of Execution, Commutation, and Removal Duration on Death Row: 5 years					
$(EXEC_t / SENT_{t-5})_{-1}$	-0.0044**			-0.0046**	-0.0054**
	(0.0022)			(0.0022)	(0.0027)
$(COMM_t / SENT_{t-5})_{-1}$		0.0064**		0.0068***	
		(0.0026)		(0.0022)	
$(REMOVE_t / SENT_{t-5})_{-1}$			0.0019***		0.0021***
			(0.0006)		(0.0007)
n	707	713	685	706	682
Duration on Death Row: 4 years					
$(EXEC_t / SENT_{t-4})_{-1}$	-0.0048			-0.0048	-0.0049
	(0.0038)			(0.0038)	(0.0036)
$(COMM_t / SENT_{t-4})_{-1}$		0.0046**		0.0045***	
		* (0.0008)		(0.0009)	
$(REMOVE_t / SENT_{t-4})_{-1}$			0.0013*		0.0015**
			(0.0007)		(0.0007)
n	753	760	732	751	729

Note: Robust and clustered standard errors are in parentheses. * indicates statistical significance between 10 and 5 percent, ** statistical significance between 5 and 1 percent. *** statistical significance at the 1 percent level or better.

One standard objection to economic analysis of crime is whether potential criminals are as astute as Ph.D. economists to evaluate these probabilities accurately. This objection is invalid so long as the researcher believes that empirical research should be conceptually consistent with the underlying theory. If one assumes a priori that individuals are incapable of calculating the risks as they are defined by theory, then there is no room to conduct proper empirical research. For example, if one rejects the theoretically-proper measure of the execution risk as executions within a cohort of death row inmates in a given year divided by death sentenced handed out to that cohort in some earlier year (because one believes that potential criminals do not observe either the executions or the death sentences), then one ought to claim that they cannot observe and evaluate other variables either, including the arrest rates, the size of the police force or police spending. Thus, there would be no need to conduct research investigating whether people react to deterrence, under the belief that people could not evaluate variations in deterrence risks to begin with.

Furthermore, attempts to justify the use of inappropriate variables based on the claim that individuals cannot observe, measure, or determine the values of decision parameters will produce peculiar analyses that cannot be defended theoretically. For example, if the theory indicates that the real wages should matter in a particular context, it would be silly to suggest the use of nominal wages in a regression (instead of real wages) on the grounds that people cannot observe and predict accurately the level of the consumer price index. If the theory indicates that the accident risk in a state is best measured by the number of accidents per vehicle miles traveled, it would be incorrect to promote deflating accidents by other measures such as the square miles of the state or the number of car dealerships, on the grounds that vehicle miles traveled is difficult to observe.

It should be noted, however, that in our context, the results are robust even to the use of measures that are inconsistent with theory. A summary of the findings is provided in Table 2.18, which displays the results obtained from estimating various versions of Equation (1) along with the description of the measurement of the execution, commutation and removal rates in each specification. The table displays results that are obtained from specifications where the key variables (execution, commutation and removal risks) are measured as dictated by theory. The table also presents results from the models where they are measured incorrectly. Examples are the specifications where executions, commutations and removals are deflated by lagged murder rate, by population; where the raw count of executions, commutations and removals are used; or the specifications promoted by Donohue and Wolfers (2006) (reported in rows 5 and 6 of Table 2.18). As the table demonstrates, the results are remarkably stable even across models that substantially deviate from theory.

2.8 Conclusion and Discussion

Do people respond to incentives? An economist's answer to this question is a resounding "yes," not only because economic theory indicates that incentives matter, but because an enormous empirical literature shows that they do. An especially confusing dimension for non-economists is the behavior of individuals in such domains as the consumption of addictive substances, sexual activity and criminal behavior. In the case of criminal behavior, non-economists frequently express the belief that human beings are not rational enough to make calculated decisions about the costs and benefits of engaging in crime, and that criminal activity cannot be altered by incentives. Of course personal beliefs should not determine the answers to scientific questions. Rather, answers should be provided by careful and objective scientific inquiry.

Table 2.18
Summary of the Results

A	B	(A/B)	Exec	Comm	Rem
First Measures of Executions, Commutations, Removals	Death Sentences handed out 5 years prior (duration on death row=5 years)	$(EXEC_t/SENT_{t-5})$, $(COMM_t/SENT_{t-5})$, $(REMOVE_t/SENT_{t-5})$	--*	+	+*
First Measures of Executions, Commutations, Removals	Death Sentences handed out 4 years prior (duration on death row=4 years)	$(EXEC_t/SENT_{t-4})$, $(COMM_t/SENT_{t-4})$, $(REMOVE_t/SENT_{t-4})$	--*	+*	+
Second Measures of Executions, Commutations, Removals	Death Sentences handed out 5 years prior (duration on death row=5 years)	$(EXEC2_t/SENT_{t-5})$, $(COMM2_t/SENT_{t-5})$, $(REMOVE2_t/SENT_{t-5})$	--*	+	+*
Second Measures of Executions, Commutations, Removals	Death Sentences handed out 4 years prior (duration on death row=4 years)	$(EXEC2_t/SENT_{t-4})$, $(COMM2_t/SENT_{t-4})$, $(REMOVE2_t/SENT_{t-4})$	--*	+*	+
First Measures of Executions, Commutations, Removals (D-III&W Specification)	Death Sentences handed out the same year (duration on death row=0 years)	$(EXEC_t/SENT_t)$, $(COMM_t/SENT_t)$, $(REMOVE_t/SENT_t)$	--	+*	+
Second Measures of Executions, Commutations, Removals (D-III&W Specification)	Death Sentences handed out the same year (duration on death row=0 years)	$(EXEC2_t/SENT_t)$, $(COMM2_t/SENT_t)$, $(REMOVE2_t/SENT_t)$	--	+*	+
First or Second Measures of Executions, Commutations, Removals	Death Sentences handed out *i*, *j*, or *k* years prior for spells ending in year *t* (duration on death row=changes by year)	$(EXEC_t/SENT_{t-i})$, $(COMM_t/SENT_{t-j})$, $(REMOVE_t/SENT_{t-k})$	--*	+	+
First Measures of Executions, Commutations, Removals	Death Row Inmates (ROW)	$(EXEC_t/ROW_t)$, $(COMM_t/ROW_t)$, $(REMOVE_t/ROW_t)$	--*	+*	+
Second Measures of Executions, Commutations, Removals	Death Row Inmates (ROW)	$(EXEC2_t/ROW_t)$, $(COMM2_t/ROW_t)$, $(REMOVE2_t/ROW_t)$	--*	+*	+

Table 2.18 (Continued)

First Measures of Executions, Commutations, Removals	Total Prisoners (PRIS)	($EXEC_t/PRIS_t$), ($COMM_t/PRIS_t$), ($REMOVE_t/PRIS_t$)	--*	+	+
Second Measures of Executions, Commutations, Removals	Total Prisoners (PRIS)	($EXEC2_t/PRIS_t$), ($COMM2_t/PRIS_t$), ($REMOVE2_t/PRIS_t$)	--*	+	+
The Raw Count of Executions (#EX), Commutations (#C), Removals (#R)	Population (POP)	($\#EX_t/POP_t$), ($\#C_t/POP_t$), ($\#R_t/POP_t$)	--*	+	+
The Raw Count of Executions (#EX), Commutations (#C), Removals (#R)	Lagged Murder Rate (MURDER)	($\#EX_t/MURDER_t$), ($\#C_t/MURDER_t$), ($\#R_t/MURDER_t$)	--*	--	+
The Raw Count of Executions (#EX), Commutations (#C), Removals (#R)		($\#EX_t$), ($\#C_t$), ($\#R_t$)	--*	--	+
First Measures of Executions, Commutations, Removals (Unweighted Regression)	Death Sentences handed out 5 years prior (duration on death row=5 years)	($EXEC_t/SENT_{t-5}$), ($COMM_t/SENT_{t-5}$), ($REMOVE_t/SENT_{t-5}$)	--*	+*	+*
First Measures of Executions, Commutations, Removals (Unweighted Regression)	Death Sentences handed out 4 years prior (duration on death row=4 years)	($EXEC_t/SENT_{t-4}$), ($COMM_t/SENT_{t-4}$), ($REMOVE_t/SENT_{t-4}$)	--	+*	+*
Second Measures of Executions, Commutations, Removals (Unweighted Regression)	Death Sentences handed out 5 years prior (duration on death row=5 years)	($EXEC2_t/SENT_{t-5}$), ($COMM2_t/SENT_{t-5}$), ($REMOVE2_t/SENT_{t-5}$)	--*	+*	+*
Second Measures of Executions, Commutations, Removals (Unweighted Regression)	Death Sentences handed out 4 years prior (duration on death row=4 years)	($EXEC2_t/SENT_{t-4}$), ($COMM2_t/SENT_{t-4}$), ($REMOVE2_t/SENT_{t-4}$)	--	+*	+

Note: A + (--) indicates that the coefficient is positive (negative) in at least 2 of the 3 regressions pertinent to that specification. A * indicates that the coefficient is statistically significant in at least 2 of the 3 specifications. The details are reported in various tables in the paper.

In the economic approach to crime, decades of empirical research has demonstrated that potential criminals indeed respond to incentives. It has been documented that improved labor market conditions reduce the extent of criminal activity (recent examples include Grogger 1998, Freeman and Rodgers 2000, Gould et al. 2002), and criminal activity reacts to deterrence (e.g. Ehrlich 1975, Levitt 1998b, Kessler and Levitt 1999, Corman and Mocan 2000, Mustard 2003, Corman and Mocan 2005). For example, Levitt (1998b) showed that deterrence is empirically more important than incapacitation in explaining crime, and that increases in arrest rates deter criminal activity. Kessler and Levitt (1999) show that Proposition 8 in California, which introduced sentence enhancements for certain crimes, reduced eligible crimes by 4 percent in the year following its passage and 8 percent 3 years after the passage, providing strong evidence that crime rates react to the severity of punishment. In an analysis of the relationship between crime and punishment for juveniles, Levitt (1998a) finds that changes in relative punishment between juveniles and adults explain 60 percent of the differential growth rates in juvenile and adult crime, and that abrupt changes in criminal involvement with the transition from juvenile to adult courts indicate that individuals do respond to the expected punishment (as economic theory suggests). Corman and Mocan (2005, 2000) show that criminal activity responds to variations in arrests and the size of the police force.

As discussed in the introduction, the signal provided by leaving death row is no different from any other change in expected punishment. That is, an execution is a signal of an increase in expected punishment, and a commutation represents a decrease in expected punishment. However, it is sometimes claimed that because executions are infrequent events, they cannot possibly be a strong enough signals to alter the behavior of people. Yet, the same analysts have no difficulty in believing that a prospective criminal observes correctly and accurately the extent of the increase in the number of arrests, and coupled with the information about the level of crime, he calculates the enhanced risk of getting caught, and changes his behavior. Similarly, the suggestion that if the local authority hires 20 new police officers, the associated increase in the risk of getting caught by this move is properly evaluated by potential criminals does not raise objections. Even prison deaths are believed to provide signals to people who are not in prison. Katz, Levitt and Shustorovich (2003) find that the death rate in prisons constitutes deterrence, and an increase in prison deaths has a negative impact on crime rates. It is very difficult to argue that an increase in prison deaths would be a signal of deterrence, but an increase in the executions would not.

Clearly, analysts' personal beliefs regarding what should and should not constitute a strong signal are irrelevant. Whether or not police, arrests, prison deaths, executions, or commutations provide signals to people about the extent of expected punishment is an empirical question. In this paper we estimate a large number of models in an effort to make the relationship between murder rates and death penalty related outcomes (executions, commutations and removals) disappear. We change the measurement of the risk variables by altering the numerator and the denominator of the variables in a variety of ways (see Table 2.18 for a summary); we also investigate how the results change when we exclude various states from the analysis. The basic results are insensitive to these and a variety of other specification tests performed in the paper.

It is understandable that the death penalty may evoke strong feelings which could be due to political, ideological, religious, or other personal beliefs. It could also be because of the fear that a scientific paper which identifies a deterrent effect could be taken as an endorsement or justification of the death penalty. This should not be the case for any scientific research. This point is highlighted by Mocan and Gittings (2003) and Katz,Levitt and Shustorovich (2003). For example, Katz, Levitt and

Shustorovich (2003) find that the death rate among prisoners (a proxy for prison conditions) deters crime. This finding obviously does not suggest that the society should increase the death rate of the prisoners by worsening the prison conditions to reduce the crime rate. Nevertheless, the authors feel the need to state the obvious, and write that:

> "We cannot stress enough that evidence of a deterrent effect of poor prison conditions is neither a necessary nor a sufficient condition for arguing that current prison conditions are either overly benign or unjustifiably inhumane. Efficiency arguments related to deterrence are only one small aspect of an issue that is inextricably associated with basic human rights, constitutionality, and equity considerations. Our research is descriptive, not proscriptive." (p.322)

Similarly, Mocan and Gittings (Mocan and Gittings 2003, p. 474) write that the fact that there exists a deterrent effect of capital punishment, should not imply a position on death penalty. There are a number of significant issues surrounding the death penalty, ranging from potential racial discrimination in the imposition of the death penalty (Baldus et al., 1998) to discrimination regarding who is executed and who is commuted once the death penalty is received (Argys and Mocan 2004).

Given these concerns, it is critically important to preserve objectivity in scientific research on a subject matter in which opinions may have been formed without, or sometimes despite the evidence. This unfortunate phenomenon is described succinctly by Sunstein and Vermeule (2006), where they write in their reply to Donohue and Wolfers (2006):

> "We cannot help but add that as new entrants into the death penalty debate, we are struck by the intensity of people's beliefs on the empirical issues, and the extent to which their empirical judgments seem to be driven by their moral commitments. Those who oppose the death penalty on moral grounds often seem entirely unwilling to

> consider apparent evidence of deterrence and are happy to dismiss such evidence whenever even modest questions are raised about it. Those who accept the death penalty on moral grounds often seem to accept the claim of deterrence whether or not good evidence has been provided on its behalf."

In summary, the detailed analysis in this paper demonstrates the deterrent effect of capital punishment. Yet, this finding does not imply that capital punishment is good or bad, nor does it provide any judgment about whether capital punishment should be implemented or abolished. It is just a scientific finding which demonstrates that people react to incentives.

CHAPTER 3

COMBINING SYNTHETIC DATA AND NOISE INFUSION FOR CONFIDENTIALITY PROTECTION OF THE QUARTERLY WORKFORCE INDICATORS

3.1 Introduction

Since 2003, the U.S. Census Bureau has published a new and novel statistical series: the Quarterly Workforce Indicators (QWI). The underlying data infrastructure was designed by the Longitudinal Employer-Household Dynamics Program (Abowd et al.; 2004) and is described in detail elsewhere (Abowd et al.; 2005). At its core, the QWI system uses administrative records data collected by a large number of states for both jobs and employers. These administrative databases are integrated and enhanced with other data from Census Bureau censuses and surveys. Consequently, the QWI offer unprecedented demographic and economic detail on the local dynamics of labor markets.

Due to the fine detail offered by the published statistics and the confidential nature of the micro-data used to compile the statistics, confidentiality protection is a critical and integral part of the design of the QWI system. Application of a state-of-the-art dynamically-consistent noise infusion protection system allows the Census Bureau to publish these statistics. Even so, at very detailed levels of industry and geography, there are still many suppressions, which hinder the effective use of the released data. To eliminate these suppressions, we have developed a synthetic data model that replaces the suppressions with draws from an appropriate posterior predictive distribution. In this article, we summarize the layers of confidentiality protection in the QWI system and discuss the experimental enhancements. We show that confidentiality protection provided by the experimental system (which has no suppressions at all) is comparable to the protection afforded by the suppressions, but the analytic validity of the experimental system is better because the synthetic data are better than the best inference an external user can make regarding the suppressions.

The remainder of this article is structured as follows. Section 3.2 briefly describes the QWI variables and the relationships between them. Section 3.3 briefly discusses the current protection methods. Section 4 presents the details of the synthesizer. Section 3.5 details the algorithm used to create the synthetic data. Section 3.6 presents the existing disclosure avoidance protocol and the new layer introduced by the synthetic data. Section 3.7 discusses the results and Section 3.8 concludes.

3.2 QWI Notation and Definitions

In order to present an integrated description of the combination of noise infusion and synthetic data to protect the Quarterly Workforce Indicators (QWI), we develop some notation for the establishment-level micro data. The QWI are presented for categories of employees classified according a=1,...,8 (age groups) and s=1,2 (sex). These micro-data are built up from job-level records relating an individual i to his or her employer establishment j. In the notation below, use of the a,s subscripts implies summation over all persons i satisfying the conditions for a particular employment category (B,E,etc.) at establishment j in quarter t.

The basic employment variables subject to micro-data protection are:

- B_{asjt}, beginning-of-period employment
- E_{asjt}, end-of-period employment
- H_{asjt}, new hires
- R_{asjt}, recalls
- A_{asjt}, accessions

- S_{asjt}, separations
- M_{asjt}, flow employment
- $JF_{asjt} = E_{asjt}\text{-}B_{asjt}$, net job flows
- $JC_{asjt} = \max(0, JF_{asjt})$, job creations
- $JD_{asjt} = \max(0, -JF_{asjt})$, job destructions

The following identities connect the employment and job flow concepts:

$$A_{asjt} = H_{asjt} + R_{asjt} \tag{3.1}$$

$$S_{asjt} = A_{asjt} - (JC_{asjt} - JD_{asjt}) = A_{asjt} - JF_{asjt} \tag{3.2}$$

$$E_{asjt} = B_{asjt} + A_{asjt} - S_{asjt} \tag{3.3}$$

$$M_{asjt} = A_{asjt} + B_{asjt} \tag{3.4}$$

Thus, there are four basic employment flow quantities, B_{asjt}, E_{asjt}, H_{asjt}, R_{asjt}, only three of which may vary independently. When time-varying demographic factors are not applied, the following intertemporal identity always holds:

$$B_{\bullet sjt} = E_{\bullet sjt-1} \tag{3.5}$$

The • subscript indicates summation over that dimension. However, when age-specific employment variation is measured, this identity need not hold because individuals change relevant age groups on the first day of the new period (the reference date for B_{asjt}). For a complete description of how the establishment-level micro data are computed from the integrated longitudinal employer-employee data, see Abowd et al. (forthcoming).

The QWI also consist of full-quarter employment measures that are linked to the variables above through inequality constraints.

- F_{asjt}, full-quarter employment
- FA_{asjt}, accessions to full-quarter employment
- FS_{asjt}, separations with full-quarter employment
- $H3_{asjt}$, new hires to full-quarter employment
- $FJF_{asjt} = F_{asjt} - F_{asjt-1}$, net job flows
- $FJC_{asjt} = \max(0, FJF_{asjt})$, job creations
- $FJD_{asjt} = \max(0, -FJF_{asjt})$, job destructions

These variables are related to each other and those above through the following identities and constraints:

$$FS_{asjt} = FA_{asjt} - (F_{asjt} - F_{asjt-1}) = FA_{asjt} - FJF_{asjt} \tag{3.6}$$

$$H3_{asjt} \leq FA_{asjt} \leq A_{asjt} \tag{3.7}$$

$$H3_{asjt} \leq H_{asjt} \tag{3.8}$$

$$FS_{asjt} \leq S_{asjt} \tag{3.9}$$

$$F_{asjt} \leq \min[E_{asjt}, B_{asjt}] \tag{3.10}$$

Of the three full-quarter variables, F_{asjt}, FS_{asjt}, FA_{asjt}, only two may vary independently while $H3_{asjt}$ is constrained only by the inequalities. Note that the inequalities contain quantities from the both the full-quarter and basic employment variables. This joint determination of the variables adds an additional layer of complexity to the synthesis.

3.3 Overview of Current Protection Methods

Currently, confidentiality of the underlying micro-data is ensured through several layers of protection: multiplicative noise infusion, weighting and cell suppression. The first two layers (noise and weighting) sufficiently protect the vast majority of cells. Distribution preserving noise is infused to all workplace estimates at the establishment level. The noise factor is constructed as having the appropriate distributional properties so that cross-sectional and time-series attributes of the data are maintained after aggregation, except for the increase in the cross-sectional variance arising from the noise infusion.

Draw noise factor δ_j from the appropriate distribution. Denote X_{asjt} as the complete set of QWI employment count variables and define $X^*_{asjt}=\delta_j X_{asjt}$. One can verify that all of the identities still hold. See Abowd, Stephens and Vilhuber (2005) for a complete description of how the noise infusion is constructed. The data are then aggregated from the workplace to higher levels of sub-state geography and industry using a time-varying establishment-level weight designed so that beginning quarter employment (B) of the QWI matches the first monthemployment of the Quarterly Census of Employment and Wages (QCEW). This second layer of protection produces estimates that differ even further from the underlying confidential micro-data. However, the extraordinary detail of the QWIs still produces some sparsely populated aggregate cells. Cell suppression is then used when those cell counts are determined to be too small. The problem is then that users of the data must model the missing data based on publication counts. The synthetic data methods we develop here will replace the suppressed values in the publication tables, using the methods we develop here but with confidential parameters for the actual implementation.

3.4 Synthetic Data Model

To synthesize the core set of variables and maintain dynamic consistency we need a model that satisfies all of the definitions and identities. However, the process is complicated by the desire to use the synthetic data only when the noise infusion does not adequately protect an estimated employment count. Since all of the released data will be based on either noise infusion or synthesis, preserving dynamic consistency requires that the noise-infused values and the synthetic values be consistent. This is done by choosing an appropriate conditioning set of variables and sampling from the posterior to ensure the identities hold (see below for details).

Our synthetic data model is based on a multinomial likelihood with a Dirichlet on the priors. Specifically, denote Y_{asjt} as the set of QWI variables to be jointly synthesized (e.g. $Y_{asjt} = (B_{asjt}, H_{asjt}, R_{asjt})$) and ${Y_{asjt}}^r$ as the resulting set of synthetic values. Each element of Y_{asjt} takes on the values of 0,1,2,3 or 4^+. We denote the conditioning set as Ω_{asjt} that contains Y_{asjt-1}, Y_{asjt+1} as well as job flows, JF^*_{asjt}, which has a feasible range of -4 to 4. Letting θ be the vector of multinomial probabilities and $\alpha_{1|asjt}, ..., \alpha_{L|asjt}$ be the shape parameters of the Dirichlet for the L possible outcomes. The likelihood and Dirichlet prior can be summarized by the following two equations:

$$\rho\left(n_{asjt} \middle| \Omega_{asjt}, \theta_{asjt}\right) \propto \Pi_{l=1}^{L} [\theta_{l|asjt}]^{n_{asjt}} \tag{3.11}$$

$$\theta_{asjt} \sim \text{Dirichlet}(\alpha_{1|asjt}, \ldots, \alpha_{L|asjt})_{sjt} \tag{3.12}$$

where n_{asjt} are the counts of Y_{asjt} for characteristics a,s in establishment j in quarter t. The prior shape is given by α. The resulting posterior can then be written as:

$$\theta_{asjt}^{pos} \sim \text{Dirichlet}(\alpha_{1|asjt} + n_{1|asjt}, \ldots, \alpha_{L|asjt} + n_{L|asjt}) \tag{3.13}$$

$$\rho\left(n_{asjt}^{pos} \middle| \Omega_{asjt}, \theta_{asjt}^{pos}\right) \propto \Pi_{l=1}^{L} [\theta_{asjt}^{pos}]^{n_{asjt}} \tag{3.14}$$

Filling the suppressions then requires first sampling from the posterior on the probabilities then using that draw to sample an outcome Y^r_{asjt} for establishment j.[1] With the resulting outcome, it remains to compute the remaining QWI variables using the identities.

3.4.1 Estimation of the Likelihood Component

To illustrate the construction of the likelihood, consider the synthesizer that draws Y^r_{asjt} conditional on Ω_{asjt}. For each age group and sex (a,s) with data configuration Ω_{asjt} we estimate the likelihood contribution separately for each quarter t as follows. In quarter t, select only those establishments for which the values of Y_{asjt} lie in the allowable outcome space. Stratify these establishments according to the observed combinations of Y_{asjt-1},Y_{asjt+1} and JF^*_{asjt}. Let $\eta_{l|asmt}$ be the establishment count for each possible combination l in the feasible outcome space of Y_{asjt}, where m designates each unique combination of Y_{asjt-1},Y_{asjt+1} and JF^*_{asjt}. Then, $\eta_{\bullet|asmt}$ is the total number of establishments with configuration m and $\theta_{l|asmt} = ((\eta_{l|asmt}) / (\eta_{\bullet|asmt}))$ are the maximum likelihood estimates of the feasible outcome probabilities.

3.4.2 Specification of the Dirichlet Prior

We want to use a data-based informative prior for the probabilities. To do this we aggregate by first dropping the conditioning variables Y_{asjt-1} and Y_{asjt+1} so that the conditioning set consists of age/sex groups and job flow counts, JF^*_{asjt}. The data are then pooled across the current quarter being synthesized along with three additional seasonally-consistent quarters-- historical if available, future otherwise--designated by the set of quarters Q_t. To ensure that the posterior receives positive weight on all

[1] Empirically computing the Dirichlet posterior is relatively simple. An algorithm for doing so can be found in the text "Bayesian Data Analysis" by Gelman, Carlin and Stern (2003).

feasible outcomes, we blend the data-based prior with a uniform prior denoted as u. The Dirichlet prior shape parameters are estimated by

$$\rho_{l|asmt}=0.99\times((\textstyle\sum_{t\in Q_t}\eta_{l|asmt})/(\sum_{t\in Q_t}\eta_{\cdot|asmt})+0.01\times u \qquad (3.15)$$

The specification of the Dirichlet is completed by assigning a prior sample size. The results shown below are for a prior sample size of 1; however, the exact prior sample size used in production will be confidential. Denoting the prior sample size by α_0, the Dirichlet prior can be completely specified by $\alpha_{l|asmt}=\alpha_0\rho_{l|asmt}$, where l ranges over all values in the feasible set that have positive prior probability.

3.4.3 Sampling from the Posterior

For each observed value of (a,s,m,t), the probabilities $\theta^{pos}{}_{l|asmt}$ are Dirichlet with parameters $\alpha_{1|asmt} + n_{1|asmt},\ldots,\alpha_{L|asmt} + n_{L|asmt}$, where events with zero posterior probability have been removed from the feasible outcome space. For each (a,s,m,t), sample $\theta_{l|asmt}$ from the Dirichlet and then for each establishment j, sample $Y^r{}_{asjt}$ from these probabilities. Compute the remaining QWI variables from the identities and definitions above. If the computed values from the identities are infeasible (i.e., are negative), reject and draw again. The synthetic data sampling can be performed multiple times; however the results reported below are for single synthetic data samples.

3.5 Algorithm

Since the QWIs are linked through a series of identities and inequality constraints, the quantities must be partitioned into a subset that is synthesized and a subset that is evaluated using the identities. Furthermore, this allows for the creation of multiple sets of synthetic data depending on which subsets are synthesized and calculated. A decision rule on which set to use minimizes the amount of synthesis necessary for protection.

The synthesis is conducted in two stages with the full-quarter variables being synthesized last. Let (B_{asjt},H_{asjt},R_{asjt}) be group 1 variables to synthesize and (E_{asjt},A_{asjt},S_{asjt}) be group 2 variables to synthesize. When group 1 is synthesized, E_{asjt},A_{asjt},S_{asjt},M_{asjt} are evaluated using the identities and likewise, and when group 2 is synthesized, B_{asjt},H_{asjt},R_{asjt},M_{asjt} are evaluated. The second stage consists of synthesizing the values (F_{asjt},FA_{asjt},$H3_{asjt}$) and evaluating FS_{asjt}. Note that the second stage draws the full-quarter synthetic values conditional on the synthetic set obtained from the first stage.

Let $Y^{1r}{}_{asjt}$ denote the synthetic value of variables B_{asjt},E_{asjt},H_{asjt},R_{asjt},JF_{asjt}, JC_{asjt}, JD_{asjt},A_{asjt},S_{asjt},M_{asjt} resulting from group 1 synthesis, and let $Y^{2r}{}_{asjt}$ denote the values from group 2 synthesis. Let $Z^{1r}{}_{asjt}$ denote the synthetic values of full-quarter variables F_{asjt},FA_{asjt},FS_{asjt},$H3_{asjt}$,FJF_{asjt}, FJC_{asjt}, FJD_{asjt} that result from conditioning on $Y^{1r}{}_{asjt}$ in the second stage, and $Z^{2r}{}_{asjt}$ the synthetic values that result from conditioning on $Y^{2r}{}_{asjt}$ in the second stage. This results in two full sets of synthetic data we label $X^{1r}{}_{asjt} = [Y^{1r}{}_{asjt}\ Z^{1r}{}_{asjt}]$ and $X^{2r}{}_{asjt} = [Y^{2r}{}_{asjt}\ Z^{2r}{}_{asjt}]$.

The overall synthesizing algorithm, which is based on partially synthetic data (Reiter, 2004 and Raghanathan, Reiter, and Rubin, 2003) can be summarized by the following.

In stage 1, draw ($B^r{}_{asjt}$, $H^r{}_{asjt}$, $R^r{}_{asjt}$) conditional on (B_{asjt-1}, H_{asjt-1}, R_{asjt-1}), (B_{asjt+1},H_{asjt+1},R_{asjt+1}), $JF^*{}_{asjt}$. Also draw ($E^r{}_{asjt}$, $A^r{}_{asjt}$, $S^r{}_{asjt}$) conditional on (E_{asjt-1},A_{asjt-1},S_{asjt-1}), (E_{asjt+1}, A_{asjt+1},S_{asjt+1}), $JF^*{}_{asjt}$. For each synthesized group, evaluate the remaining variables using the identities (note that use of the definitions insures that $JC^r{}_{asjt} = JC^*{}_{asjt}$ and $JD^r{}_{asjt} = JD^*{}_{asjt}$).

Next check that $Y^{1r}_{asjt}, Y^{2r}_{asjt} \geq 0$ for all variables. If not, redraw the appropriate synthesis group and reevaluate the identities. For $Y^{r}_{asjt}=JF_{asjt},JC_{asjt},JD_{asjt}$ calculate $Y^{r}_{\bullet sjt}=Y^{*}_{\bullet sjt}, Y^{r}_{a\bullet jt}=Y^{*}_{a\bullet jt}$, and $Y^{r}_{\bullet\bullet jt}=Y^{*}_{\bullet\bullet jt}$. For the remaining variables in Y^{r}_{asjt}, calculate $Y^{r}_{\bullet sjt}$, $Y^{r}_{a\bullet jt}$, and $Y^{r}_{\bullet\bullet jt}$.

In stage 2, draw $(F^{r}_{asjt}, FA^{r}_{asjt}, H3^{r}_{asjt})$ given $(F_{asjt-1},FA_{asjt-1},H3_{asjt-1})$, $(F_{asjt+1},FA_{asjt+1},H3_{asjt+1})$ and note that conditioning on FJF^{*}_{asjt} here would fully constrain F^{r}_{asjt}). Evaluate FS^{r}_{asjt}, FJC^{r}_{asjt}, FJD^{r}_{asjt} , and FS^{r}_{asjt}. Check that Z^{1r}_{asjt}, $Z^{2r}_{asjt} \geq 0$ for all variables. If not, redraw the appropriate synthesis group and reevaluate the identities. Finally, for the remaining variables in Z^{r}_{asjt}, calculate $Z^{r}_{\bullet sjt}$, $Z^{r}_{a\bullet jt}$, and $Z^{r}_{\bullet\bullet jt}$.

At the end of the synthesizing algorithm, each establishment has a complete set of X_{asjt}, X^{*}_{asjt}, X^{1r}_{asjt}, and X^{2r}_{asjt} and all dynamic identities hold. Furthermore, the dynamic identities and the intraestablishment marginal employment counts are all consistent between the noise-infused and synthetic data.

3.6 Forming the Quarterly Workforce Indicators

The QWI are created by aggregating the micro-data for establishments j into ownership × geography × industry categories using the establishment weight w_{jt}. Call a cell of a particular aggregation k. Thus, the subscript k replacing the establishment subscript indicates that all the establishments meeting a particular set of ownership, geography, and industry criteria have been summed. For each k, consider the QWI X_{askt}, X^{*}_{askt}, and X^{r}_{askt}.

At each level of aggregation, the QWIs are evaluated for confidentiality protection. Cells are suppressed that are composed too few individuals or too few establishments and are given a status flag of 5. Cells are flagged as 9 if the protection mechanisms have distorted the data past a particular threshold, and the remaining cells are released with a status flag of 1.[2]

Given the experiments with synthetic data described in this paper, the experimental rules are the following.[3] If $X_{askt} \in \{1,2\}$ then release the appropriate X^r_{askt} and set status to 9. Else if abs $((X_{askt}\text{-}X_{askt}) / (X_{askt})) \geq \beta$ then release X_{askt}^* and set status to 9. Else release X_{askt}^* and set status to 1.

Under the experimental rules with a synthetic data protection component, there are no data with status 5 (i.e., there are no suppressions); however, significant distortion can arise from either noise infusion or synthesis. Hence, the revised release rules meet either the magnitude inference distortion or the probability inference distortion conditions set forth in the introduction.

3.7 Results for Experimental Protection Rules

These results summarize the effects of the various layers of the protection system. The data item to be protected is the value in the unweighted, undistorted micro-data, which corresponds to a particular variable in X_{askt} for aggregations k and individuals in demographic category (a,s). The aggregations presented here are for county-level geography and NAICS industry group (4-digit), although the tables have been computed for the other industry classifications as well. The 4-digit NAICS classification is chosen because it naturally has the largest number of small cells among the QWI publication tables and therefore more suppressions in the released data. The first set of results are cross-tabulations that show how the values of the

[2] The threshholds that determine whether cells are flagged with a 5 or 9 are confidential.
[3] Evans, Zayatz, and Slanta (1998) proposed a noise infusion protection system using the Research and Development survey. In doing so, they did not reveal the actual rules for any particular release of data, but they did detail the parameters of their experiments and rules they used to evaluate their methods.

unprotected microdata are perturbed by each of the protection layers. The second set of results illustrate the how the time series properties the data are affected.

3.7.1 Cross-Tabulations

The following tables have been computed for each of the eleven synthesized variables B_{asjt},E_{asjt},H_{asjt},R_{asjt},A_{asjt},S_{asjt},M_{asjt},F_{asjt},FA_{asjt},FS_{asjt}, and $H3_{asjt}$ for the states of Maryland and Illinois but only the results for B_{asjt} are shown here for each state. The remaining tables have the same conclusions.

Table 3.1 to 3.3 show how the unweighted, undistorted data are affected by distortion, distortion plus weighting and then synthesis, respectively, for the variable B_{asjt} in the Maryland data. Tables 3.4 to 3.6 show the same data for Illinois. The data are rounded to the nearest unit in the column, where 5^+ contains data values of 5 or more. The main elements of interest in the tables are the percentages along the diagonal, which shows how often the value of the confidential micro data (the rows of the table) is unchanged by the particular protection method (the columns of the table). If the value on the diagonal is too high, the data are insufficiently protected.

By reviewing the rows of Table 3.1, it is clear that the noise infusion does not adequately protect single individuals in an age/sex category and, by extension, beginning-of-period employment in the establishment of 1. It is also clear that values of 2 are adequately protected if the required inference error rate is set at 10% or more. Of course, one cannot suppress only values of 1 which is why more than one cell must be suppressed in the current protocols. Table 3.2 shows the effects of combining noise infusion with weighting. Again, the percentage on the diagonal for values of 1 is still high, but Table 3.3 shows that introducing our synthetic data methods now adequately protects the small values and suppression is no longer needed. The remaining tables

Table 3.1
Variable: B
Unweighted/Undistorted vs. Unweighted/Distorted

	0	1	2	3	4	5+
0	99.61	0.39	0.00	0.00	0.00	0.00
1	0.00	98.57	1.43	0.00	0.00	0.00
2	0.00	1.04	96.10	2.85	0.00	0.00
3	0.00	0.00	2.19	93.21	4.60	0.00
4	0.00	0.00	0.00	7.30	82.52	10.18
5+	0.00	0.00	0.00	0.00	1.60	98.40

Note: The data represent county data for Maryland/NAICS Industry Group. Cells represent row percentages and sum to 100.

Table 3.2
Variable: B
Unweighted/Undistorted vs. Weighted/Distorted

	0	1	2	3	4	5+
0	99.19	0.81	0.00	0.00	0.00	0.00
1	0.14	89.29	10.56	0.01	0.00	0.00
2	0.04	1.39	67.45	30.70	0.42	0.00
3	0.03	0.04	2.19	50.99	42.76	3.99
4	0.03	0.02	0.03	3.07	41.04	55.81
5+	0.01	0.00	0.00	0.02	0.33	99.64

Note: The data represent county data for Maryland/NAICS Industry Group. Cells represent row percentages and sum to 100.

Table 3.3
Variable: B
Unweighted/Undistorted vs. Synthesized

	0	1	2	3	4	5+
0	99.17	0.82	0.01	0.00	0.00	0.00
1	7.85	84.74	6.62	0.78	0.01	0.00
2	0.51	11.93	61.06	24.14	2.24	0.12
3	0.06	0.76	7.53	47.50	39.13	5.02
4	0.03	0.11	0.93	7.40	38.84	52.69
5+	0.01	0.01	0.01	0.11	0.71	99.16

Note: The data represent county data for Maryland/NAICS Industry Group. Cells represent row percentages and sum to 100.

Table 3.4
Variable: B
Unweighted/Undistorted vs. Unweighted/Distorted

	0	1	2	3	4	5+
0	99.64	0.36	0.00	0.00	0.00	0.00
1	0.00	98.71	1.29	0.00	0.00	0.00
2	0.00	1.28	95.98	2.74	0.00	0.00
3	0.00	0.00	2.63	92.89	4.48	0.00
4	0.00	0.00	0.00	8.07	81.81	10.12
5+	0.00	0.00	0.00	0.00	2.13	97.87

Note: The data represent county data for Illinois/NAICS Industry Group. Cells represent row percentages and sum to 100.

Table 3.5
Variable: B
Unweighted/Undistorted vs. Weighted/Distorted

	0	1	2	3	4	5+
0	99.47	0.53	0.00	0.00	0.00	0.00
1	0.16	94.86	4.97	0.00	0.00	0.00
2	0.00	2.81	80.74	16.30	0.14	0.00
3	0.00	0.00	5.88	67.79	24.73	1.59
4	0.00	0.00	0.13	8.51	55.64	35.73
5+	0.00	0.00	0.00	0.06	1.20	98.74

Note: The data represent county data for Illinois /NAICS Industry Group. Cells represent row percentages and sum to 100.

Table 3.6
Variable: B
Unweighted/Undistorted vs. Synthesized

	0	1	2	3	4	5+
0	99.45	0.54	0.00	0.00	0.00	0.00
1	7.36	87.45	4.70	0.48	0.00	0.00
2	0.40	12.99	70.81	14.52	1.23	0.06
3	0.02	0.67	10.62	63.33	22.97	2.39
4	0.00	0.07	1.11	12.68	52.13	34.00
5+	0.00	0.00	0.01	0.20	1.69	98.09

Note: The data represent county data for Illinois /NAICS Industry Group. Cells represent row percentages and sum to 100.

display the same patterns and the results for the variables not presented here are also similar.

3.7.2 Analytical Validity of Time-Series Properties

The conclusion that the synthesizer sufficiently protects the QWI micro data is positive, but it is also of interest how the statistical properties of the data hold up. The current use of suppression is problematic for users of the data because they are forced to model the missing data themselves based on the released data or ignore it. Here we show that replacing the suppressions with synthetic data not only retains the statistical properties of the underlying data but also yields an improvement over modeling the missing data externally.

Our test of the time-series properties computes the first-order autocorrelation coefficient using maximum likelihood for each variable for age/sex groups at the county/NACIS Industry Group (4-digit) level of aggregation. To judge the analytical validity we look at the distribution of the difference between the autocorrelation coefficient using the unweighted/undistorted data and the estimate produced when using one of the layers of protection. A difference of zero between these two estimates would indicate no bias and preservation of the time-series properties. Tables 3.7 to 3.9 show the distribution of this difference for each variable under each protection scheme for Maryland. Tables 3.10 to 3.12 show the results for Illinois. Table 3.7 shows the difference between the autocorrelation coefficient estimated from the unweighted/undistorted data versus the unweighted/distorted data. Table 3.8 compares the underlying micro data with the weighted/distorted data that suppresses the appropriate small values, and Table 3.9 displays the results when the suppressions are replaced with synthetic data.

Table 3.7
Distribution of the Difference Between Autocorrelation Coefficients
Unweighted/Undistorted vs. Unweighted/Distorted

Percentile	B	H	R	E	A	S	M	F	FA	FS	H3
99	0.067	0.058	0.042	0.066	0.061	0.061	0.079	0.059	0.056	0.054	0.056
95	0.036	0.035	0.025	0.036	0.036	0.036	0.042	0.032	0.034	0.033	0.035
90	0.025	0.025	0.015	0.024	0.025	0.025	0.028	0.021	0.024	0.023	0.024
75	0.009	0.009	0.005	0.009	0.010	0.010	0.011	0.008	0.009	0.008	0.008
50	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
25	-0.008	-0.011	-0.005	-0.009	-0.010	-0.010	-0.011	-0.008	-0.009	-0.008	-0.009
10	-0.023	-0.028	-0.016	-0.023	-0.026	-0.026	-0.027	-0.022	-0.026	-0.024	-0.026
5	-0.035	-0.038	-0.025	-0.037	-0.036	-0.038	-0.041	-0.033	-0.037	-0.035	-0.038
1	-0.061	-0.063	-0.045	-0.065	-0.059	-0.065	-0.074	-0.061	-0.061	-0.059	-0.062

Note: The data represent county data for Maryland/NAICS Industry Group. Cells represent the difference between the autocorrelation coefficients the percentile designated by the rows.

Table 3.8
Distribution of the Difference Between Autocorrelation Coefficients
Unweighted/Undistorted vs. Weighted/Distorted w/ Suppressions

Percentile	B	H	R	E	A	S	M	F	FA	FS	H3
99	0.318	0.592	0.538	0.325	0.602	0.652	0.330	0.366	0.606	0.656	0.648
95	0.153	0.326	0.221	0.158	0.294	0.304	0.134	0.181	0.317	0.352	0.359
90	0.086	0.196	0.136	0.084	0.173	0.181	0.063	0.108	0.196	0.218	0.229
75	0.021	0.060	0.049	0.019	0.053	0.053	0.017	0.025	0.068	0.070	0.087
50	-0.002	-0.006	0.000	-0.003	-0.006	-0.008	-0.006	-0.001	-0.004	-0.004	-0.001
25	-0.029	-0.075	-0.063	-0.031	-0.071	-0.078	-0.037	-0.026	-0.070	-0.084	-0.082
10	-0.085	-0.188	-0.162	-0.091	-0.170	-0.190	-0.099	-0.077	-0.169	-0.200	-0.190
5	-0.150	-0.289	-0.242	-0.154	-0.258	-0.288	-0.176	-0.141	-0.262	-0.300	-0.286
1	-0.393	-0.551	-0.474	-0.426	-0.505	-0.531	-0.489	-0.419	-0.499	-0.538	-0.527

Note: The data represent county data for Maryland/NAICS Industry Group. Cells represent the difference between the autocorrelation coefficients the percentile designated by the rows.

Table 3.9
Distribution of the Difference Between Autocorrelation Coefficients
Unweighted/Undistorted vs. Weighted/Distorted w/ Synthetic Replacements

Percentile	B	H	R	E	A	S	M	F	FA	FS	H3
99	0.107	0.246	0.223	0.096	0.238	0.263	0.210	0.227	0.260	0.157	0.294
95	0.056	0.144	0.126	0.051	0.134	0.166	0.123	0.112	0.145	0.076	0.185
90	0.036	0.104	0.092	0.033	0.099	0.120	0.084	0.075	0.108	0.049	0.133
75	0.011	0.050	0.043	0.010	0.047	0.058	0.037	0.031	0.051	0.018	0.066
50	-0.004	0.003	0.007	-0.005	0.005	0.007	0.006	0.002	0.007	-0.003	0.009
25	-0.030	-0.043	-0.013	-0.030	-0.032	-0.032	-0.017	-0.018	-0.030	-0.034	-0.037
10	-0.065	-0.109	-0.067	-0.064	-0.089	-0.095	-0.049	-0.056	-0.095	-0.082	-0.128
5	-0.092	-0.167	-0.120	-0.093	-0.142	-0.145	-0.081	-0.088	-0.144	-0.116	-0.194
1	-0.166	-0.295	-0.257	-0.176	-0.257	-0.268	-0.168	-0.187	-0.277	-0.233	-0.343

Note: The data represent county data for Maryland/NAICS Industry Group. Cells represent the difference between the autocorrelation coefficients the percentile designated by the rows.

Table 3.10
Distribution of the Difference Between Autocorrelation Coefficients
Unweighted/Undistorted vs. Unweighted/Distorted

Percentile	B	H	R	E	A	S	M	F	FA	FS	H3
99	0.070	0.060	0.040	0.070	0.060	0.060	0.080	0.070	0.060	0.050	0.060
95	0.040	0.030	0.020	0.040	0.040	0.040	0.040	0.030	0.030	0.030	0.030
90	0.020	0.020	0.010	0.030	0.020	0.020	0.030	0.020	0.020	0.020	0.020
75	0.010	0.010	0.000	0.010	0.010	0.010	0.010	0.010	0.010	0.010	0.010
50	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
25	-0.010	-0.010	0.000	-0.010	-0.010	-0.010	-0.010	-0.010	-0.010	-0.010	-0.010
10	-0.020	-0.020	-0.010	-0.020	-0.020	-0.020	-0.020	-0.020	-0.020	-0.020	-0.020
5	-0.030	-0.040	-0.020	-0.040	-0.030	-0.040	-0.040	-0.030	-0.030	-0.030	-0.030
1	-0.070	-0.060	-0.050	-0.070	-0.060	-0.060	-0.070	-0.060	-0.060	-0.050	-0.060

Note: The data represent county data for Illinois/NAICS Industry Group. Cells represent the difference between the autocorrelation coefficients the percentile designated by the rows.

Table 3.11
Distribution of the Difference Between Autocorrelation Coefficients
Unweighted/Undistorted vs. Weighted/Distorted w/ Suppressions

Percentile	B	H	R	E	A	S	M	F	FA	FS	H3
99	0.350	0.700	0.570	0.390	0.650	0.700	0.340	0.420	0.660	0.670	0.670
95	0.190	0.390	0.260	0.190	0.360	0.380	0.150	0.230	0.380	0.390	0.400
90	0.110	0.260	0.150	0.110	0.230	0.230	0.080	0.140	0.240	0.230	0.270
75	0.030	0.090	0.050	0.030	0.070	0.070	0.020	0.030	0.090	0.080	0.110
50	0.000	0.000	0.000	0.000	-0.010	-0.010	0.000	0.000	0.000	0.000	0.000
25	-0.030	-0.080	-0.070	-0.040	-0.090	-0.090	-0.040	-0.030	-0.080	-0.090	-0.080
10	-0.120	-0.200	-0.180	-0.130	-0.200	-0.220	-0.140	-0.110	-0.200	-0.220	-0.220
5	-0.240	-0.290	-0.270	-0.230	-0.290	-0.330	-0.250	-0.220	-0.320	-0.330	-0.330
1	-0.690	-0.570	-0.490	-0.710	-0.540	-0.610	-0.700	-0.650	-0.550	-0.600	-0.600

Note: The data represent county data for Illinois/NAICS Industry Group. Cells represent the difference between the autocorrelation coefficients the percentile designated by the rows.

Table 3.12
Distribution of the Difference Between Autocorrelation Coefficients
Unweighted/Undistorted vs. Weighted/Distorted w/ Synthetic Replacements

Percentile	B	H	R	E	A	S	M	F	FA	FS	H3
99	0.140	0.250	0.260	0.120	0.250	0.270	0.250	0.250	0.260	0.180	0.300
95	0.070	0.150	0.150	0.060	0.150	0.170	0.140	0.120	0.160	0.090	0.190
90	0.040	0.110	0.110	0.040	0.110	0.120	0.100	0.080	0.120	0.060	0.130
75	0.010	0.050	0.050	0.010	0.050	0.060	0.050	0.030	0.060	0.020	0.060
50	0.000	0.000	0.010	-0.010	0.010	0.010	0.010	0.000	0.010	0.000	0.010
25	-0.030	-0.040	-0.010	-0.040	-0.040	-0.040	-0.020	-0.020	-0.030	-0.030	-0.040
10	-0.070	-0.120	-0.070	-0.080	-0.110	-0.110	-0.060	-0.070	-0.110	-0.080	-0.130
5	-0.110	-0.170	-0.140	-0.110	-0.160	-0.170	-0.090	-0.110	-0.180	-0.130	-0.200
1	-0.210	-0.310	-0.270	-0.220	-0.290	-0.300	-0.210	-0.240	-0.340	-0.260	-0.390

Note: The data represent county data for Illinois/NAICS Industry Group. Cells represent the difference between the autocorrelation coefficients the percentile designated by the rows.

It is no surprise that there is almost no bias when only distortion is used since it was designed to preserve these properties. However, the results comparing the synthetic data (Table 3.9) are clearly superior those with suppressions (Table 3.8). Using the synthetic data introduce very little bias compared to Table 3.7 with almost all of the bias in the tails whereas the difference is largely zero for much of the distribution. Across the board this bias is less than that shown in Table 3.8 and the results clearly demonstrate that the time-series properties of the data are preserved with the synthesizer.

3.8 Conclusion

This paper describes a data synthesizer developed to replace sensitive values that are suppressed in the Quarterly Workforce Indicators. The sensitive values are necessarily suppressed in the QWI because the current methods (estimation, noise infusion, and weighting) may not provide adequate confidentiality protection for certain small cells. This paper shows that the synthetic data that replace the suppressed cells offer sufficient protection and the synthetic data also preserves the time-series properties of the underlying confidential data. In particular, noise infusion and weighting alone often do not yield a high enough error rate for small cells but the synthesizer clearly masks those values sufficiently. Furthermore, not only are the data protected but the time-series properties are retained in the synthetic data and yield a substantial improvement over the current published data with suppressions.

APPENDIX

Table APP1
Mobility Definitions

Label	Definition	Notation: $(\Delta RL, \Delta UNIT, \Delta JC)$
Promotion	A rise in reporting level closer to the CEO that also entails a change in job title.	$(\Delta RL^{+}, \Delta UNIT^{+}, \Delta JC^{1})$ $(\Delta RL^{+}, \Delta UNIT^{-}, \Delta JC^{1})$ $(\Delta RL^{+}, \Delta UNIT^{0}, \Delta JC^{1})$
Demotion	A fall in reporting level away from the CEO that also entails a change in job title.	$(\Delta RL^{-}, \Delta UNIT^{+}, \Delta JC^{1})$ $(\Delta RL^{-}, \Delta UNIT^{-}, \Delta JC^{1})$ $(\Delta RL^{-}, \Delta UNIT^{0}, \Delta JC^{1})$
Lateral - Strict	A transition within the same reporting level, the same organizational unit level and also entails a change in job title.	$(\Delta RL^{0}, \Delta UNIT^{0}, \Delta JC^{1})$
Lateral - Up	A transition within the same reporting level, a rise in organizational unit level closer to the CEO and also entails a change in job title.	$(\Delta RL^{0}, \Delta UNIT^{+}, \Delta JC^{1})$
Lateral - Down	A transition within the same reporting level, a fall in organizational unit level away from the CEO and also entails a change in job title.	$(\Delta RL^{0}, \Delta UNIT^{-}, \Delta JC^{1})$
Lateral - Same Job	A transition within the same reporting level, but *does not* entail a change in job title.	$(\Delta RL^{0}, \Delta UNIT^{+}, \Delta JC^{0})$ $(\Delta RL^{0}, \Delta UNIT^{-}, \Delta JC^{0})$
Level Up - Same Job	A rise in reporting level closer to the CEO, but *does not* entail a change in job title.	$(\Delta RL^{+}, \Delta UNIT^{+}, \Delta JC^{0})$ $(\Delta RL^{+}, \Delta UNIT^{-}, \Delta JC^{0})$ $(\Delta RL^{+}, \Delta UNIT^{0}, \Delta JC^{0})$
Level Dn - Same Job	A fall in reporting level away from the CEO, but *does not* entail a change in job title.	$(\Delta RL^{-}, \Delta UNIT^{+}, \Delta JC^{0})$ $(\Delta RL^{-}, \Delta UNIT^{-}, \Delta JC^{0})$ $(\Delta RL^{-}, \Delta UNIT^{0}, \Delta JC^{0})$
Stayer	No change in reporting level, no change in organizational unit level and *does not* entail a change in job title.	$(\Delta RL^{0}, \Delta UNIT^{0}, \Delta JC^{0})$

Definitions of Fundamental LEHD concepts

We briefly explain some of the basic concepts underlying QWI processing, and indeed, much of the LEHD Infrastructure. A more exhaustive list of definitions can be found on theLEHD website at http://lehd.dsd.census.gov.

Fundamental Concepts

Dates

The QWI is a quarterly data system with calendar year timing. We use the notation YYYY:Q to refer to a year and quarter combination. For example, 1999:4 refers to the fourth quarter of 1999, which includes the months October, November, and December.

Employer

An employer in the QWI system consists of a single Unemployment Insurance (UI) account in a given state's UI wage reporting system. For statistical purposes the QWI system creates an employer identifier called an State Employer Identification Number (SEIN) from the UI-account number and information about the state (FIPS code). Thus, within the QWI system, the SEIN is a unique identifier within and across states but the entity to which it refers is a UI account.

Establishment

For a given employer in the QWI system, an SEIN, each physical location within the state is assigned a unit number, called the SEINUNIT. This SEINUNIT is based on the reporting unit in the ES-202 files supplied by the states. All QWI statistics are produced by aggregating statistics calculated at the establishment level. Single-unit SEINs are UI accounts associated with a single reporting unit in the state.

Thus, single-unit SEINs have only one associated SEINUNIT in every quarter. Multi-unit SEINs have two or more SEINUNITS associated for some quarters. Since the UI wage records are not coded down to the SEINUNIT, SEINUNITs are multiply imputed as described in Abowd et al. (2005). A feature of this imputation system is that it does not permit SEINUNIT to SEINUNIT movements within the same SEIN. Thus, for multi-unit SEINs, the definitions below produce the same flow estimates at the SEIN level whether the definition is applied to the SEIN or the SEINUNIT.

Employee

Individual employees are identified by their Social Security Numbers (SSN) on the UI wage records that provide the input to the QWI. To protect the privacy of the SSN and the individual's name, a different branch of the Census Bureau removes the name and replaces the SSN with an internal Census identifier called a Protected Identity Key (PIK).

Job

The QWI system definition of a job is the association of an individual (PIK) with an establishment (SEINUNIT) in a given year and quarter. The QWI system stores the entire history of every job that an individual holds. Estimates are based on the definitions presented below, which formalize how the QWI system estimates the start of a job (accession), employment status (beginning- and end-of-quarter employment), continuous employment (full-quarter employment), the end of a job (separation), and average earnings for different groups.

Unemployment Insurance wage records (the QWI system universe)

The Quarterly Workforce Indicators are built upon concepts that begin with the report of an individual's UI-covered earnings by an employing entity (SEIN). An individual's UI wage record enters the QWI system if at least one employer reports earnings of at least one dollar for that individual (PIK) during the quarter. Thus, the job must produce at least one dollar of UI-covered earnings during a given quarter to count in the QWI system. The presence of this valid UI wage record in the QWI system triggers the beginning of calculations that estimate whether that individual was employed at the beginning of the quarter, at the end of the quarter, and continuously throughout the quarter. These designations are discussed below. Once these point-in-time employment measures have been estimated for the individual, further analysis of the individual's wage records results in estimates of full-quarter employment, accessions, separations (point-in-time and full-quarter), job creations and destructions, and a variety of full-quarter average earnings measures.

Employment at a point in time

Employment is estimated at two points in time during the quarter, corresponding to the first and last calendar days. An individual is defined as employed at the beginning of the quarter when that individual has valid UI wage records for the current quarter and the preceding quarter. Both records must apply to the same employer (SEIN). An individual is defined as employed at the end of the quarter when that individual has valid UI wage records for the current quarter and the subsequent quarter. Again, both records must show the same employer. The QWI system uses beginning and end of quarter employment as the basis for constructing worker and job flows. In addition, these measures are used to check the external consistency of the data, since a variety of employment estimates are available as point-in-time measures.

Many federal statistics are based upon estimates of employment as of the 12th day of particular months. The Census Bureau uses March 12 as the reference date for employment measures contained in its Business Register and on the Economic Censuses and Surveys. The BLS Quarterly Census of Employment and Wages (QCEW)3 series, which is based on the ES-202 data, use the 12th of each month as the reference date for employment. The QWI system cannot use exactly the same reference date as these other systems because UI wage reports do not specify additional detail regarding the timing of the wage payments. QWI research has shown that the point-in-time definitions used to estimate beginning and end of quarter employment track the QCEW month one employment estimates well at thelevel of an employer (SEIN). For single-unit SEINs, there is no difference between an employerbased definition and an establishment-based definition of point-in-time employment. For multiunit SEINs, the unit-to-worker imputation model assumes that unit-to-unit transitions within the same SEIN cannot occur. So, point in time employment defined at either the SEIN or SEINUNIT level produces the same result.

Employment for a full quarter

The concept of full quarter employment estimates individuals who are likely to have been continuously employed throughout the quarter at a given employer. An individual is defined as fullquarter- employed if that individual has valid UI-wage records in the current quarter, the preceding quarter, and the subsequent quarter at the same employer (SEIN). That is, in terms of the point-intime definitions, if the individual is employed at the same employer at both the beginning and end of the quarter, then the individual is considered full-quarter employed in the QWI system.

Consider the following example. Suppose that an individual has valid UI wage records at employer A in 1999:2, 1999:3, and 1999:4. This individual does not have a valid UI wage record at employer A in 1999:1 or 2000:1. Then, according to the definitions above, the individual is employed at the end of 1999:2, the beginning and end of 1999:3, and the beginning of 1999:4 at employer A. The QWI system treats this individual as a full-quarter employee in 1999:3 but not in 1999:2 or 1999:4. Full-quarter status is not defined for either the first or last quarter of available data.

Point-in-time estimates of accession and separation

An accession occurs in the QWI system when it encounters the first valid UI wage record for a job (an individual (PIK)-employer (SEINUNIT) pair). Accessions are not defined for the first quarter of available data from a given state. The QWI definition of an accession can be interpreted as an estimate of the number of new employees added to the payroll of the establishment (SEINUNIT) during the quarter. The individuals who acceded to a particular employer were not employed by that employer during the previous quarter but received at least one dollar of UI-covered earnings during the quarter of accession.

A separation occurs in the current quarter of the QWI system when it encounters no valid UI wage record for an individual-employer pair in the subsequent quarter. This definition of separation can be interpreted as an estimate of the number of employees who left the employer during the current quarter. These individuals received UI-covered earnings during the current quarter but did not receive any UI-covered earnings in the next quarter from this employer. Separations are not defined for the last quarter of available data.

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