THREE ESSAYS ON WAGE INEQUALITY AND
HEALTH INSURANCE COVERAGE

BY

CHICHUN FANG

DISSERTATION

Submitted in partial fulfillment of the requirements
for the degree of Doctor of Philosophy in Human Resources and Industrial Relations
in the Graduate College of the
University of Illinois at Urbana-Champaign, 2012

Urbana, Illinois

Doctoral Committee:

LER Alumni Professor Craig A. Olson, Chair
Associate Professor Darren H. Lubotsky
Associate Professor John C. Dencker
William G. Karnes Professor Jeffrey R. Brown

ABSTRACT

Two of the important issues concerning the general well-beings of the working population– wage inequality and wage health insurance– are studied. This thesis consists three chapters. In the first chapter, we (co-authored) examine the effects of corporate restructuring on wage patterns and inequality in a Fortune 500 firm. We show that the restructuring, including reductions in force and transformation in compensation systems, shifts the firm's employment system away from the traditional internal labor market model; hence the wage structure is penetrated by external market forces, leading to lower starting salaries for new hires, lower returns to seniority, and a more polarized wage distribution within the firm. The second chapter addresses the causal relationship between dynamics of health insurance coverage and employment. I find that at most 60% of the male non-elderly population is consistently insured during a 12-year span while only 2% consistently uninsured. Estimating the causal relationship between coverage and employment is challenging because of their correlation and state dependence. A model without controlling for the correlation between coverage and employment suggests that unemployment has a negative impact on the likelihood of being insured that lasts for three years. However, after controlling the correlation between coverage and employment, the effect becomes much smaller. In the final chapter I examine the racial gap in employer-sponsored health insurance coverage as well as how the recent hike in insurance premium affects employer offering as well as employee enrollment in the health insurance plan. The black-white insurance coverage gap is trivial after controlling for individual and job characteristics, but the Hispanic-white gap remains significant. Around one-third of the racial gap can be explained by the racial discrepancy in education and another one-third by the discrepancy in job characteristics. The minorities, nevertheless, are especially vulnerable to the loss of employer offering and hence health insurance coverage when the insurance premium cost increases.

à mes parents, pour leur amour et leur soutien.

ACKNOWLEDGMENTS

This has been a great journey.

First and foremost, I want to dedicate my work to my family. My parents kindly provide me their unconditional love and support to pursue my career in a foreign country. Ava, my fiancée, helped me to cruise through the challenge of adopting myself in an unfamiliar environment during my early time on campus and has been the most important part of my life.

I can never overemphasize how grateful I am to my thesis committee members. Professor Craig Olson has given me more freedom than I could have ever imagined to explore my own research interest and yet is always patient and helpful whenever I need his assistance. What I learned from him is much more than just doing research– his consistent passion to advance the knowledge of the field is equally inspirational. Working with Professor John Dencker is also a great experience, and our co-authored paper eventually becomes one chapter in this thesis. Professor Jeff Brown sparks my interest in employee benefits issues, and Professor Darren Lubotsky has been kind and generous with his precious time and expertise since the early stage of my study.

Throughout my time in the Ph.D. program, I have benefited a lot from some other faculty members and received support from the department staff and my student colleagues. Professor Todd Elder equipped me with a solid fundamental of quantitive skills. Dean Joel Cutcher-Gershenfeld is always encouraging me to bridge my study on human resource specifics and the broader topics in the workplace. Professor Tom Kochan and Professor Diane Burton provide me the luxurious opportunity to network with students from other campuses through the distance learning consortium among doctoral students in human resources and industrial relations. The supporting staff at the School of Labor Employment Relations, Becky Barker, Lynne Hoveln, and Kathy Kuntz have been tremendously helpful in administrative issues.

A special thank you goes to Eun Kyung Lee, my student colleague and officemate. She has kindly been the first reader of several of my early drafts, and the discussion about ideas and research methods with her substantially improves me as a researcher in human resource studies.

Finally, I would like to thank Professor Kevin Hallock, who brought me into the field of labor relations and human resource management in his class in the spring of 2004. I would not have been here without his encouragement.

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CHAPTER 1

CORPORATE RESTRUCTURING AND WAGE INEQUALITY

1.1 Introduction

The increase of wage inequality in U.S. labor market since the 1980s is well documented in the social science literature. The literature shows that the wage structure has polarized (Morris, Bernhardt and Handcock 1994), as the upper-tail (the difference between the 90^{th} and 50^{th} percentiles) inequality increased steadily and the lower tail (the difference between the 50^{th} and 10^{th} percentiles) inequality rose sharply in the early 1980s but stopped increasing and even retracted thereafter (Autor, Katz and Kearney 2008, Lemieux 2008). Also, the within-group dimension of inequality has increased significantly since the late 1970s (Juhn, Murphy and Pierce 1993), and occupation is playing increasingly important roles in wage determination and inequality (Kim and Sakamoto 2008, Mouw and Kalleberg 2010).

Research on the increase in wage inequality has generated a wealth of information about this phenomenon, yet it has also raised important questions regarding the ultimate determinants of this increase. Scholars in different disciplines have sought to answer these questions by examining changes in demand for certain types of skills, as well as changes in the occupational structure. While the increase of wage inequality in the 1980s is studied

[0]This chapter is co-authored with Professor John Dencker in School of Labor and Employment Relations at the University of Illinois.

early and a lot in labor economics (Autor et al. 2008, Autor, Katz and Krueger 1998, Borjas and Ramey 1995, Card and DiNardo 2002, Chay and Lee 2000, DiNardo, Fortin and Lemieux 1996, Katz and Autor 1999, Katz and Murphy 1992, Lemieux 2006, Lemieux 2008, Murphy and Welch 1992), the roles of the firm and its human resource policies in contributing to such increase is surprisingly not widely studied. A growing number of scholars maintain that answers to these questions can be obtained by assessing how corporate restructuring– a widespread and ongoing process that substantially transformed the way firms reward employees (Cappelli, Bassi, Katz, Knoke, Osterman and Useem 1997)– affected wage dynamics (Kim and Sakamoto 2008, Morris and Western 1999). However, aside from important insights on how organizational practices modify effects of restructuring on wages (Fernandez 2001) and on the nature of work in restructured firms (Cornfield, Campbell and McCammon 2001), it is not only unclear whether and how restructuring influenced wage inequality, but also why– given that multiple forms of restructuring influenced employee groups in similar ways (Baumol, Blinder and Wolff 2003)– the rate of growth in inequality varied within and across these groups (Kim and Sakamoto 2008, Mouw and Kalleberg 2010).

Extant research provides some important insights into whether and how restructuring influenced inequality, suggesting that there are two main ways in which it did so: through corporate reductions in force (RIF) and through substantial transformations in compensation systems within firms (Cappelli et al. 1997). RIF– a large scale separation process affecting most if not all employee groups– transformed employment relationships from closed systems, wherein employees held long term employment contracts with firms, to open systems, wherein firms could terminate employees at will (Sørensen 2000).

RIF thus reflected a shift in power from employees to firms' owners, allowing firms to rely more extensively on external market forces to govern the allocation of rewards (Cappelli 1992, Sørensen 2000). For example, through RIF, firms could arguably replace employees with equally productive but lower-paid individuals from the external labor market, thereby generating strong downward pressures on starting salaries for these vulnerable employees.

By ending guarantees of life-time employment, RIF also made past implicit employment contracts– in which pay was a strong function of continued employment with a firm– much less feasible. Nevertheless, because the employment relationship was not fully open– in that it did not resemble a spot market contract (Goldthorpe 2000)– firms had to find new ways of motivating and rewarding employees. They typically did so by relying on variable pay systems that reduced the degree to which wages were dependent on seniority, and increased the degree to which they were dependent on performance (Cappelli et al. 1997, Dencker 2009). The shift to variable-pay systems has had an influence on wage inequality as, for instance, the number of jobs wherein rewards depend solely on employee performance increased considerably (Lemieux, MacLeod and Parent 2009). Yet, such types of restructuring likely affected wage inequality in other ways as it also increased the degree to which all types of jobs were less dependent on seniority (and more dependent on performance).

Although RIF and transformations in compensation systems have affected employment relationships of most if not all employee groups in similar ways, we argue that their effect on wage inequality will vary within and across employee groups due to variations in path dependencies in pay and employment systems for each group. In particular, in order to understand the effect of the two main restructuring forms on inequality, we maintain that it is important to consider the nature of pre-restructuring job and wage structures for each employee group– as well as variation in how and the extent to which these group-specific structures were altered by the restructuring process.

Prior to the onset of restructuring, production workers derived their power in the employment relationship through labor unions and collective bargaining. Unions secured above market wages for these workers, and limited within-group wage inequality through a reliance on simple job structures and on seniority for allocating pay increases. As we will argue, restructuring increased inequality for production workers not only due to a shift to a two-tiered job structure (Cappelli and Sherer 1990)– wherein starting salaries of incoming workers were lower than they were for previous cohorts– but also to a decreased reliance on seniority (and increased reliance on performance) to govern salaries.

By contrast, managers' power in the pre-restructuring employment relationship was a function of their firm-specific human capital, which led to the formation of closed (long-term) employment relationships (Sørensen 2000). In such relationships, firms implemented incentive systems that resembled deferred compensation contracts. In particular, managers received a starting salary that was lower than they could receive in the external market, with the implicit promise that their salaries would raise above the external market later in their careers (Lazear 1979). As such, seniority played an important role in salary increases, although performance was a key factor as well. Because corporate restructuring limited firms' ability to use implicitly bonded contracts, they were unable to rely on seniority-based pay systems to the extent they previously did. Instead, firms relied on market forces to govern managerial pay– thus arguably leading to an increase in starting salaries of managers, albeit with some limits imposed by the staying power of this group's job and wage structures. Moreover, firms altered the way they rewarded managers by increasing the link between pay and productivity. However, since firms had previously relied in part on performance to adjust salaries over time, the magnitude of the decrease in returns to seniority would be lower for managers than for production workers.

Finally, the power of clerical workers in the employment relationship was generally lower than that of managers and production workers, yet their wages were in theory higher than what they could receive in the external market due to efficiency wage and fairness considerations. For example, firms paying above market wages to some employees would pay above market wages to all employees. Yet, clerical workers generally were paid at a lower wage rate than were production workers and managers. Thus, although restructuring should lead to downward pressures on starting salaries of clerical workers, their starting salaries should not fall as much as those of production workers. Similarly, the magnitude of the decreased reliance on seniority to allocate salary increases following restructuring should be lower for clerical workers relative to production workers– as clerical workers' rewards in the past were in part a function of performance, not simply seniority (Spilerman 1986).

In short, the two forms of corporate restructuring– RIF and the transformation in compensation systems– should not only increase the degree to which firms relied on market forces to govern the allocation of rewards, but also decrease the degree to which institutional features of internal labor markets (ILMs), such as seniority-based pay systems, affected pay adjustments. Moreover, pre-restructuring variation across employee groups in the nature of job and wage structures, and the institutional rules governing the allocation in rewards– combined with variation in stability of these factors over time– should lead to post-restructuring differences in the rate of increase in wage inequality within and across these groups, even though the forms of restructuring had similar effects on all groups.

In this article, we develop a framework to explain the effects of restructuring on three wage patterns and outcomes: starting salaries; returns to seniority; and wage inequality– for three key types of employees, namely production workers, clerical workers, and managers. We analyze predictions from our framework using data from personnel files of a Fortune 500 energy sector firm for the period 1969 to 1993. Our study thus offers a unique insight into the effects of restructuring on wage inequality. It spans a period of stability and change and allows us to analyze at a more fine grained level the proximate mechanisms influencing wage dynamics within and between work groups.

The remainder of this article is structured as follows. We begin by discussing how the dependencies of a firm on different employee groups influenced pre-restructuring wage and job structures. We then develop a framework to explain how temporal variation in the inter-group stability of these structures moderated or magnified the effect of restructuring on wage determinants and outcomes. After providing information on our data and methods, we present and discuss our results.

1.2 Pre-Restructuring Wage Systems and Patterns

A key feature of the employment relationship prior to the onset of corporate restructuring was the internal labor market (ILM)– a set of rules and processes whereby employment and wage decisions were made within firms rather than through a reliance on the external market (Doeringer and Piore 1971). In ILMs, employees were buffered from market competition, with employment separation decisions largely being the right of the employee rather than a firm. Although ILMs were similar across employee types on several dimensions (e.g., entry into common portals, with promotions allocated to internal workers), their determinants– and hence the bargaining power of the employee– differed according to the group to which she belonged.

For production workers, unions were a critical source of bargaining power, as they gave their members a voice in the actions of firms, and provided protection against economic turbulence (Freeman and Medoff 1984). For example, layoffs for production workers were largely temporary, with more senior workers the last to be laid off, and the first to be recalled. Unions also helped production workers secure wages that were higher than what they would receive on the external market through collective bargaining (Kochan, Katz and McKersie 1994), with spill-over effects increasing the welfare of workers in non-union settings (Hirsch and Addison 1986) where firms provided high enough wages to deflect union organization drives (Kochan et al. 1994).

In effect, unionism created a highly formalized contract wherein a particular wage rate was attached to a job, with unions controlling income over a worker's career by setting up seniority rules that allocated job vacancies in a firm's hierarchy for internal promotion (Kochan et al. 1994). These seniority based systems also played a key role in minimizing wage inequality for similarly situated production workers because salary adjustments were determined primarily by time spent in a firm and a job, with unions resisting the use of performance-based pay.

Managerial ILMs, by contrast, arose largely from the joint investment between a firm and its managers in the managers' human capital, resulting in a specific asset unique to their relationship (Becker 1994). Given the long-term nature of these employment relationship– as both firms and managers wished to remain attached– firms relied on incentive systems to motivate managers (Sørensen 1994). These systems promised future rewards to ensure that managers remained attached to a firm and that employees put forth the effort sought by the firm. Thus, the employment relationship in an ILM resembled a deferred compensation contract: a new manager accepted a below-market wage when she joined a firm, with her wage growing at a faster rate over time than productivity (Lazear 1979).

Managers also experienced greater within-group wage inequality than production workers did due to the hierarchical job structures within which their careers unfolded. In these structures, pay was generally attached to a job level, with the wage increasing in increasing job level (Baker, Gibbs and Holmstrom 1994). These salary grade level systems often spanned myriad levels (Gerhart and Rynes 2003), thereby making wage inequality a structural feature of managerial ILMs. Thus, an employee's wage was a function of her ability to move up the organizational job ladder, and to earn salary increases within a given job level. Promotions and pay adjustments were dependent on both seniority (Medoff and Abraham 1980) and performance (Rosenbaum 1979).

ILMs also provided employment protection to clerical workers. Although these workers obtained a lower wage than other employees, reflective of their relatively lower bargaining power– in theory, their wages were higher than what they would receive in the external market due for instance to efficiency wage rationales (i.e., above market wages motivated workers). In addition, the fairness variant of efficiency wage theory predicts that a firm paying above market wages to some of its workers would pay above market wages to all of its workers, as scholars have uncovered in studies of inter-industry wage profiles (Krueger and Summers 1988).

Similar to managers, a key means of wage growth for clerical workers was a promotion to a higher ranked job, as well as salary growth within a given job level that was driven primarily by seniority. For example, promotion competitions were less common for clerical workers than for managers, as their upward moves were determined by seniority in a job, and some minimum level of performance (Spilerman 1986). In addition, career ladders of clerical workers were shorter than those of managers, leading to lower within-group wage inequality.

In sum, prior to the onset of corporate restructuring, the different determinants of ILMs and the job and wage structures within them lead to differences in the extent of inequality within work groups, as well as differences across these groups in the extent to which pay was determined by seniority. Within group inequality was greatest among managers and clerical workers relative to production workers. Average wages levels were highest for managers, and lowest for clerical workers, with production workers' average wages falling in the middle of these two distributions. Finally, seniority was an important determinant of salary increases for all employee groups, but it was more important for production workers relative to clerical workers, and for clerical workers relative to managers.

1.3 Corporate Restructuring and Wages

Corporate restructuring refers to a process affecting workers at all organizational levels (Frenkel 2003). It involves the positive language of reducing costs, increasing profits, improving product and service quality, increasing share price, and responding quickly to opportunities (Hirsch and

De Soucey 2006). Due to corporate restructuring, traditional career models such as long-term employment and job security are less common (Cappelli et al. 1997, Cornfield et al. 2001, Hallock 2009), as "new economy" employment models redefine career paths, risks, and networks (Hirsch and De Soucey 2006).

Corporate restructuring began in the early 1980s, and was driven by a number of factors (Baumol et al. 2003). For example, demand shifts stemming from increased foreign competition pressured firms to make extensive changes in the way they operated (Baumol et al. 2003). These pressures were exacerbated in the early 1980s by corporate raiders who engaged in hostile takeovers (Shleifer and Summers 1998). Although takeovers were restricted by the late 1980s (Jensen 1993), the pace of restructuring increased afterwards due to pressures from institutional investors (Useem 1996).

Corporate restructuring has been widespread, with most large firms restructuring multiple times (Cascio, Young and Morris 1997). The two most common types of restructuring are corporate RIF and transformations in compensation and performance management systems (Cappelli et al. 1997). Firms engaging in RIF ended guarantees of protection against layoff, making continued employment a function of market rather than non-market factorsas firms had greater flexibility in replacing an employee if they could find a more productive one at a given wage rate (or a less expensive one with the same level of productivity). Thus a firm would no longer promise long-term employment and job security to its workers; instead, employment outcomes varied dramatically by types of employees, amounts of human capital, and employee bargaining power relative to the employer (Cappelli et al. 1997, Kim and Sakamoto 2010).

In effect, RIF increased the degree to which market forces penetrated a firm, and decreased the degree to which a firm relied on internal arrangements to link productivity and wages (Sørensen 1994). In addition, firms that transformed their compensation and performance management systems dramatically altered the way they linked wages and productivity by making pay more variable (i.e., more dependent on performance and less dependent on seniority) (Cappelli et al. 1997). As we show below, both forms of restructuring have significant implications for key wage determinants and outcomes.

1.3.1 The Effect of Corporate Restructuring on Starting Salaries

Due to the onset of corporate restructuring and the shift to more "open" employment contracts (Sørensen 2000), market forces play an increasingly important role in wage determination within a firm. These forces should have an impact on wages of employees at different career stages, but– due to differential determinants of bargaining power prior to restructuring, and to variation in the strength of path dependencies in job and wage structures that reflected these prior power differences– they arguably will have the greatest impact at the main interface between the firm and the market, namely on starting salaries.

As noted, the relationship between the wages attached to entry level jobs and those in the external market differed by employee type, with production workers obtaining an above-market wage due to practices and policies of unions, managers receiving a below-market wage due to the nature of their human capital investment, and clerical workers receiving an above-market wage due to efficiency wage and fairness considerations. An implication is that a firm could obtain a less expensive production worker from the external market and/or simply lower the wage provided to new entrants. This notion is consistent with research on how firms decrease labor costs while preserving the wages and benefits of current workers, namely by adopting "two-tiered" wage systems (Cappelli and Sherer 1990, Martin and Peterson 1987, McFarlin and Frone 1990). As such, wages for entry level positions would move toward the market wage for such jobs– indicating that starting salaries of production workers should decline considerably following the onset of restructuring.

Due to the use of deferred compensation schemes, the starting salaries of managers prior to the onset of corporate restructuring in theory were lower than the spot-contract external market wage that they could otherwise receive. Thus, starting salaries of managers should increase following the onset of restructuring– as firms found it difficult to rely on deferred compensation arrangements, and hence would be forced to offer market salaries to new entrants. Nevertheless, the magnitude of this increase might be limited by pre-restructuring job and wage structures, which have been resilient to change (Gerhart and Rynes 2003). For example, the tight and extensive linkages between jobs and wages in managerial ILMs could limit the degree to which firms can raise or lower wages at different points in a manager's career. That is, adjusting a wage range for entry level positions might require a firm to modify wage ranges for higher level positions, thereby limiting the upward pressure of market forces on starting salaries of managers.

Finally, to the extent that wages of clerical workers prior to the onset of restructuring were adjusted upward due to efficiency wage considerations, their starting salaries should decline accordingly following the onset of corporate restructuring, as institutional factors that exerted upward pressures on wages are replaced by market forces. Moreover, the low bargaining power of clerical workers relative to a firm's owners would limit their ability to prevent firms from adjusting their wages to the market wage. However, because starting salaries of clerical workers prior to the onset of restructuring did not deviate from the market wages as much as the starting salaries of production workers did, the magnitude of the decrease will be lower for clerical workers than for production workers.

In sum, the variation in a firm's dependence on the three types of employee groups prior to and during corporate restructuring will lead to different patterns in starting salary dynamics for these groups. In particular, the starting salaries of production workers will decline the most following the onset of corporate restructuring– with starting salaries of clerical workers declining at a lower rate, and starting salaries of managers increasing in this time period.

1.3.2 The Effect of Corporate Restructuring on Returns to Seniority

Because RIF limited firms' ability to use deferred compensation contracts– and because employment relationships were not fully open following restructuring (Goldthorpe 2000)– firms needed to find new ways of motivating and rewarding employees. One of the main ways they did so was to implement variable pay systems with a goal of increasing the link between pay and productivity (Cappelli et al. 1997).

Prior to the onset of corporate restructuring, seniority played a key role in salary adjustments and promotions (Doeringer and Piore 1971)– particularly for production workers, as unions resisted the implantation of any type of incentive system. Yet, seniority also played a non-trivial role in career outcomes for managers and clerical workers (Spilerman 1986). Following the onset of corporate restructuring, the determinants of salary adjustments shifted from seniority to performance as firms implemented performance-based compensation systems in locations where they were previously restricted, and revamped existing systems. That is, restructuring firms reduced the extent to which wage increases were dependent on seniority in a job and/or firm, and increased the degree to which wage increases depended on performance.

The effect of restructuring on returns to seniority will affect all employee groups, albeit with the magnitude of the effect varying across groups due to differences in the degree to which each group's pay increases were previously dependent on seniority. That is, the extent to which restructuring affected wage determinants depends on the degree to which each group relied on seniority in allocating rewards prior to the onset of the transformation process. For production workers, implementing pay-for-performance systems– which had been resisted by unions– would arguably lead to significant reductions in returns to seniority. That is, because the baseline starting point for returns to seniority would be higher for production workers than for other employees due to the collective bargaining contract, the magnitude of the effect of restructuring on wage determinants would be greater for these workers than for managers and clerical workers. In addition, the magnitude of this effect should be higher for clerical workers than for managers, since clerical workers were rewarded for seniority to a greater extent than were managers (Spilerman 1986).

1.3.3 The Effect of Corporate Restructuring on Wage Inequality

Much of the research on wage inequality focuses on how broad forces such as occupations and changes in demands for certain skills influences wage distributions of different employees. We do not deny that these forces exist, but, like Fernandez (2001), we do argue that the way they affect wage patterns and outcomes will be influenced in non-trivial ways by the systems, practices, policies, and actions of firms.

Although extant research often focuses on one type of restructuring, it is important to consider the effect of multiple forms of organizational change in order to understand not only whether restructuring affected wage inequality, but also how. In particular, the two types of restructuring we study should both have a substantial impact on wage differences within employee groups, as well as between them, albeit with the rate of growth varying across groups due to different path dependencies.

Wage inequality for production workers will increase substantially as a result of restructuring initiatives, not only as the lower end of the wage distribution is thickened by two-tier wage systems, but also as returns to performance increase wage dispersion for these workers. Wage inequality for managers will be less changed due to market pressures on starting salaries, but within-group dispersion should increase as institutional determinants of wages are reduced and those for performance are increased. Wage distributions for clerical workers should increase slightly as market forces push their starting wages downward, and as seniority becomes a less important determinant of wage increases (and performance becomes more important in this regard). In short, given the predicted changes in wage patterns, we argue that wage inequality should increase for all types of worker groups due to corporate restructuring, albeit for different reasons and at differing rates within and across these groups.

1.4 Organizational Setting, Data, Measures, and Methods

1.4.1 Organizational Setting

We analyze data obtained from confidential personnel files of a Fortune 500 energy sector firm for the period 1969 to 1993. Like most other large firms in the same time period, the firm had an ILM composed of hierarchically-ranked salary grade levels (SGLs) (Gerhart and Rynes 2003, Spilerman and Petersen 1999). In the SGL system, jobs were evaluated and assigned to levels to which salary ranges were attached. Non-exempt employees (clerical, secretarial, administrative, and support staff) were in SGL 1 through 9, and exempt employees (managers and professionals) were in SGL 7 through 24. Roughly 25% of employees were paid on a salaried basis and were not a part of the SGL system. We use the terms "clerical workers," "managers," and "production workers" to denote these three broad categories (non-exempt, exempt, and salaried), respectively. Top managers, such as CEOs and vice presidents, were considered by the firm to be "above" the SGL system and are not included in our analyses. Table 2.1 provides descriptive statistics for employees in the three work groups.

Like other firms in the 1980s and 1990s, the firm we study restructured multiple times (Cascio et al. 1997). The firm undertook two RIF during the 1980s and early 1990s, with significant cutbacks during each wave, with the first RIF occurring in the early- to mid-1980s, and the second in the early 1990s. Soon after the first RIF, the firm transformed its reward system from one in which pay was determined by seniority in a job to one in which pay was contingent on an employee's performance relative to similar employees. This new system was similar to other firms' systems at the time: the firm sent senior managers to other firms to study the changes that these firms made, and hired consultants to help design and implement its own system. As part of the change, performance records were eliminated after pay decisions were made– according to the firm, it sought to minimize potential bias in future performance rankings, in that prior performance would be less likely to be taken into account in measuring current performance. Although the firm was not an industry leader in the implementation of the restructuring events, it tended to take these actions at roughly the same time its competitors did.

1.4.2 Data Set

The firm provided career records of a 25% random sample of U.S. employees between 1969 and 1993. Only employees hired after 1969 are included in our analyses in order to avoid potential bias caused by incomplete career information of employees hired before 1968 (Petersen 1995). In the original data set provided by the firm, a new record was added whenever there was a "career change" (such as salary change, promotion, demotion, etc.) for each employee. To transform the data into a yearly panel, we keep only the last record for each employee in each year. Hence, we have a "snapshot" of all employees in the firm in the end of each year. All career information, such as promotion, transfer, etc., is nevertheless preserved and merged into the snapshot of the year in which such an event occurred. Our final sample includes 22,187 employees: 6,773 production workers (34,808 employee-year records), 10,099 clerical workers (46,173 records), and 8,517 managers (67,276 records). An employee who experiences change in type of work (for example, being promoted from a clerical worker to a manager) is counted in all the types that he or she ever belongs to. Thus, the summed number of employees in each occupation type (6,773+10,099+8,517) is greater than total numbers of employees (22,187).

1.4.3 Dependent Variable

The salary of the employee is our main variable of interest. The data set provided nominal annual salaries, which we deflated to 2007 U.S. Dollars. In our analyses, we also use the logarithm transformation of the real (inflation-adjusted) salary when necessary.

1.4.4 Independent Variables

We assess effects of corporate restructuring on wage patterns, determinants and inequality by examining the relations between wages and two main independent variables: restructuring and seniority. The various methods we use require different operationalization of the restructuring variable, which we will address in further details when we discuss our methods and results. In our analysis we focus on how the first RIF and the change in compensation system that occurred in the 1980s changed wage outcomes (since our data end in 1993, they do not provide a long enough window for a precise assessment of the second RIF on wages).

Information on seniority can be constructed from date of entry into the firm, job level, and date of promotion or demotion. We calculate an employee's firm tenure (years elapsed between entry into the firm and time of observation) and job tenure (years elapsed since starting the current job) in each year. In robustness tests, we consider how an employee's performance influences his or her wage. Because the firm eliminated performance evaluations, we attempt to impute this information from the data by regressing wages on all individual characteristics observable in the data and estimating the regression residuals.[1] This performance proxy is time varying and is updated each year.[2]

Finally, instead of using separate dummy variables representing different broad occupations (production, managerial, and clerical) and including a full set of interaction between the dummy variables and all other independent and control variables, we estimate separate regressions for these three broad occupations.

1.4.5 Control Variables

We control for variables common in studies of employment outcomes in large firms (DiPrete 2005, Elvira 2001, Petersen and Saporta 2004, Spilerman and Petersen 1999): age, education (a set of dummy variables representing the highest level of education attained: high school dropouts, high school graduates, college graduates, and post-secondary), salary grade level (entered as a set of dummy variables rather than a linear term), race (non-white versus white), sex, division, starting salary, and starting salary grade level. We also include the union coverage for a given employee in each year as a dummy variable.

Additionally, we control for effects of the different divisions with five

[1] The wage can be expressed as an equation of individual and job characteristics (education, job level, seniority, etc.), performance, an idiosyncratic component, and random errors. By regressing wages on individual and job characteristics as well as individual fixed effects, we obtain residuals of performance plus the random component in the original wage equation. The coefficient on this imputed performance term, hence, should be interpreted as the true returns to performance plus returns to residuals.

[2] An alternative and arguably more direct assessment of performance is promotion frequency, as high performers are likely to be promoted more often than other employees. However, in our identification strategy detailed below, we include employee-job level fixed effects to correct for the positive correlation between tenure and individual heterogeneity. Since a promotion is by definition a move from a lower to a higher job level and hence initiates a new employee-job level spell, the rate of promotion will not capture performance when employee-job level fixed effects are included in the regression.

dummy variables: one for the main corporate office (the omitted case), a dummy for each of three main divisions, and a fifth dummy that contained several tertiary divisions. To correct for potential selection bias due to job matching or turnover, we further control for the likelihood of departure in a year whereby we first modeled a multinomial employment separation measure– coded one if an employee retired, two if an employee resigned or was laid off, discharged, or terminated, and zero otherwise– from which we generated a predicted likelihood of employment separation that was included as a control measure using various approaches.[3] Preliminary analysis shows our results are similar no matter which set of predicted likelihoods we use.

[3]We tried two different strategies to generate the predicted likelihood of separation: a two-step logistic regression model (Ahn and Powell 1993, Lee and Maddala 1985) and a competing risks Cox model (Castilla 2005, Phillips 2001). Both strategies have their own advantages and shortcomings. As an extension of the conventional Heckman two-step model to correct for sample selection bias, the method proposed in Ahn and Powell (1993) use a multinomial logit model in the first step to allow more than 2 outcomes in the selection equation. In our analysis, although an employee either left the firm or not during the data period, different reasons of separation (retirement, resignation, layoff, discharge, etc.) might be the results of very different wage patterns and career types, and it is more appropriate to distinguish various reasons of separation/turnover rather than counting them all together as a single type of separation. And hence, the two-step model in Ahn and Powell (1993) provides a satisfying way to control for likelihood of separation. However, as the hazard of separation could be different in different years for two otherwise equivalent employees (for example, early retirements, layoffs and discharges are more likely when the firm engaged in RIFs), so it is also necessary to include 24 dummy variables indicating 25 years in the first step to capture the effect of time and external shocks on separation. Doing so is essentially estimating a hazard model with more restriction– a multinomial logistic regression imposes linearity constraints in its parameters while a semi-parametric Cox hazard model does not. Therefore we also estimated competing risks Cox proportional hazard model, again counting different reasons of separation separately, to generate the likelihood of separation. The potential problem with applying Cox proportional hazard in our analysis, however, is that the "proportionality" assumption might be violated (Allison 1982, Allison 1984). For example, in our data it is observed that demotions had a negative effect on wages and were associated with layoff. In a Cox model, the proportionality assumption implies that both negative effects on wages and higher risks of being laid off only occur after a demotion is observed. This assumption is likely to be violated in our context, since it is a sequence of poor performance, rather than demotion, the observable outcome of poor performance in the data, that is associated layoff. Consequently, the effect of demotion on the likelihood of separation should take place earlier than the point when a demotion is observed, hence violates the proportionality assumption. Although neither of these two strategies is perfect, they do generate qualitatively the same predicted likelihood of separation.

We opted for the competing risks Cox proportional hazard model. Finally, we impose a linear time trend in the wage patterns in order to capture any macroeconomic impacts on the wage level.

1.4.6 Methods

We use various methods to analyze our data. Here, we provide an overview of these techniques and address additional details when we present the results. We first use graphic presentations to illustrate the change in wage patterns within the firm over time. We then analyze how the changes in the firm's compensation system affected returns to seniority using fixed effect estimations similar to the two-step strategy in Topel (1991). In other words, returns to tenure are estimated using a model in which employee-job level fixed effects are added, so we only identify the returns to job tenure within the same salary grade level, regardless of firm tenure. We adopt this strategy to eliminate potential bias in our estimation caused by other factors that can possibly attenuate the relationship between tenure and wages, such as the positive correlation between tenure and individual heterogeneity (Abraham and Farber 1987, Altonji and Shakotko 1987, Altonji and Williams 1993, Altonji and Williams 2005, Topel 1991). This set of fixed-effect models is identified based on a dummy variable indicating the onset of the changing compensation system and its interaction with seniority (i.e., we assess how the implementation of the performance-based compensation system affected wage levels and returns to seniority).

A semi-parametric variance decomposition method proposed in DiNardo, Fortin and Lemieux (1996) is used to assess how restructuring practices, including RIF and the implementation of performance-based compensation, changed the wage distribution– and hence wage inequality– within the firm. This method decomposes the change in the wage distribution over time into the "compositional effect" (caused by changes in the composition of individual and job characteristics) and the "wage structure effect" (caused by the changes in the way workers are compensated). Thus, we can observe the changes in the wage distribution net of changes in workers composition. Unlike the Oaxaca-Blinder decomposition (Blinder 1973, Oaxaca 1973), this method also allows different treatment effects across different segments of the wage distribution, enabling us to assess effects of firm restructuring at various points across the wage distribution.

1.5 Results

1.5.1 The Effect of Corporate Restructuring on Starting Salaries

In order to assess the effect of corporate restructuring on starting salaries, we provide graphical depictions of wage patterns in Figures 1.1 through 1.3 (which replicate Figure 2 in Baker, Gibbs and Holmstrom (1994)). The solid baseline in each graph connects the mean starting wage of the cohort entering the firm each year. The line deviating from the baseline every year indicates year-by-year mean wages of a specific cohort in a given year.

Consistent with the predictions of our framework, figure 1.1 shows a significant trend of decreasing starting salaries for production workers in the early 1980s (when the first RIF occurred), with newly hired employees in the mid-1980s receiving a salary roughly $20,000 lower than for newly hired employees in the late 1970s. A regression of starting salary on individual and entry job characteristics, as well as a time effect, shows that on average, starting salaries for production workers are 30% lower after than before the first RIF. Figures 1.2 and 1.3 also show that, as predicted, changes in starting salaries for managers and clerical workers are not as drastic as were those for production workers– exhibiting a slight upward trend in starting salaries over time for managers and a decrease for clerical workers. Regression results confirm the patterns in the graphs, as they indicate that managers and clerical workers experienced a 2% increase and a 4% decrease in starting salaries in post-RIF years, respectively.

To further explore the impact of restructuring on starting salary, we follow the literature on the effect of deregulation on wage outcomes (Card 1986, Crémieux 1996, Hendricks 1994, Peoples 1998) and use external wage data as a comparison to the wage patterns within the firm. We extract the wage data for people in the same industry as our firm from the Current Population Survey (CPS) as a comparison to the wage patterns within the firm that we study.[4] The CPS has around 100 to 300 people in this specific industry in each year, which allows us to calculate the mean salary at the industry level. We also use the CPS sampling weight to ensure the representativeness of the CPS data.

Figure 1.4 indicates that mean wage patterns for production workers, man-

[4] We are not aware of any single publicly available data set that has a large enough sample at the three-digit industry level as well as covers the same period of time as our firm data. We ultimately combine the wage information from two different data sets: the May supplement to the Current Population Survey between 1973 and 1978 as well as the CPS Merged Outgoing Rotation Group (MORG) data between 1979 and 1994. These combined data sets correspond to wage series between 1972 and 1993 since the CPS asks about the wage information in the previous year. To our best knowledge, there are no comparable public data sets that provide wage information between 1969 and 1972. The May CPS and the CPS-MORG use the same question to collect weekly earnings, which we multiply by 50 to obtain the annual earnings. However, the wage information in the May CPS and the CPS-MORG are both top-coded, that is, if an individual has earnings above the top-coding threshold, her earnings are coded as that certain threshold. Following the convention (Lemieux 2006), we multiply the top-coded wage records by a factor of 1.4 before calculating the mean wage for each year.

agers, and clerical workers in the firm that we study were pretty stable in the 1970s. However, production workers experienced a real wage decrease in the 1980s, while clerical workers and managers experienced real wage increases. The CPS trend rose slightly in 1970s but became essentially flat after 1980.[5] Overall, the wage series in our firm and the CPS followed a similar pattern in 1970s but decoupled in 1980s, when managers and clericals experienced wage increases whereas production workers experienced a wage decline. This period coincides with the onset of corporate restructuring in the firm we study, and the decoupling of the trends in these wage series is strong evidence that the firm was trying to both retain high-performance employees with higher salaries and compress the wage structure of the production workers. Furthermore, we decompose the CPS series in figure 1.4 by occupations comparable to those in the firm that we study. Figure 1.5 depicts the comparison between production workers in the CPS and in the firm. It is clear that the "firm wage premium" of production workers quickly disappeared in the 1980s. On the other hand, figures **??** and 1.6 suggest that the wage patterns in the firm follow very closely to those in the CPS. And hence, the comparison between the wage patterns in the CPS and our firm is a strong evidence that the wage patterns in the firm changed fundamentally in the 1980s, especially for the production workers.

Overall, figures 1.1 through 1.3 show that although the first wave of restructuring, in the form of RIF, had a non-trivial influence on starting salaries– particularly with respect to production workers– they also indicate that the market forces argument is insufficient to explain all of the growth in wage

[5]Although the CPS provides the three-digit occupation code, we are unable to extract groups that are similar to the production, clerical, and managerial employees because some of these detailed occupations span across these groups (primarily because our occupations span across the firm's hierarchy).

inequality within the different employee groups. For example, figure 1.1 indicates that although starting salaries of production workers declined considerably following the onset of restructuring, these recent cohorts nevertheless experienced more rapid wage growth with time on the job than did the older cohorts. To assess what factors explain these career changes, in the following section we assess how the later wave of restructuring, the implementation of the performance-based pay system, changed post-hiring wage outcomes.

1.5.2 The Effect of Corporate Restructuring on Returns to Seniority and Performance

We measure seniority using job tenure, which is the number of years that have elapsed since the start of the current job.[6] In table 1.2, we provide employee-job level fixed effect (Topel 1991) estimation results for production workers.[7] Model 1 is the baseline model. Model 2 adds a dummy variable

[6] Job tenure is not necessarily an integer because a promotion could have occurred in any month in a given year. Empirically, we first calculate months elapsed since promotion, and then divide this number by 12 (but do not round the result).

[7] Assessing the effect of changes in the firm's performance management system on returns to seniority is not straight forward. Altonji and Shakotko (1987) proposed a model where the returns to seniority is decomposed into returns to "true" seniority and returns to job matching. They argued that ordinary least square (OLS) estimate of returns to seniority without controlling for individual heterogeneity can be upward biased because good job matches tend to last longer than bad matches, and OLS estimates of returns to seniority hence include both returns to seniority and returns to matching. To correct for the bias, they proposed an instrumental variable approach to isolate the effect of seniority on wages from the effect of job matching. Topel (1991), in turn, claimed that Altonji and Shakotko (1987) might over-correct the bias and proposed a two-step first difference estimator. Altonji and Williams (2005) revisited the literature and concluded that the direction and magnitude of bias depend on the error structure of the data and is an empirical question. According to Altonji and Shakotko (1987), it is very likely that the returns to seniority in our data would be much higher than the population mean in the U.S. labor market. Due to the nature of our data, employees with longer tenure have more observations than those with short tenure. If people with longer tenure tend to have better job matches, the overall returns to job matching (which is not observable and will be attributed to returns to seniority in an OLS model) may be high. We hence adopt a similar strategy as the one used in Topel (1991) and include employee-job level fixed effect in our model. Consequently, we look at the returns to seniority within a job level to avoid any spurious correlations between wages and firm tenure across salary grade levels. Since we include employee-job level fixed effects, only the returns to job tenure are identifiable.

indicating records observed after the compensation policy change (which, as noted, occurred a few years after the first RIF). It indicates that salaries of production workers following the transformation in the performance management system were 2.7% lower than in prior years, even after accounting for the economy-wide wage trends in the 1980s, such as the rising returns to college education caused by the changes in relative demand for and supply of high-skilled workers.

Model 3 of table 1.2 adds interaction terms of the restructuring measure and the job tenure measure. As such, the coefficient on the interaction term indicates how returns to seniority changed due to the transformation in the firm's performance management system. The interpretations of these coefficients become complicated with multiple interaction terms, multiple squared terns, and the linear trends. Thus, in figure 1.8, we provide a graph that summarizes returns to seniority based on results in Model 3.

The top-left panel in figure 1.8 illustrates the (marginal) effect of seniority in both pre- and post-restructuring years based on the regression results. Returns (in terms of percent increase in salary) are on the vertical axis, and seniority (in terms of years of job tenure) is on the horizontal axis. This graph clearly shows that, for production workers, post-restructuring returns to seniority are only half as large as the pre-restructuring returns.

Model 4 adds the interaction between our imputed performance measure and the restructuring variable, and provides a robustness check for the seniority results in Model 3. Findings are consistent with our framework, as the positive coefficients on the newly added terms indicate that the return to performance increased after the implementation of the new compensation

To assess the change in the returns to seniority and other factors, we add the interaction terms between dummy variables indicating records observed after restructuring and the variables of interest (job tenure, squared term of job tenure, etc.).

system.

Table 1.3 provides fixed effects estimation results for managers, and table 1.4 provides these results for clerical workers. The graphs corresponding to Model 3 in each table are shown in top-right and bottom-left panels of figure 1.8, show that managers experienced lower returns to seniority in post-restructuring years than before, but clerical workers did not experience a visually substantial change in this regard. These results are thus not fully consistent with our predictions, as they suggest that the shift away from seniority-based systems was stronger for managers than clericals. However, they do indicate that, consistent with our predictions, the effect of the implementation of the performance management system had a stronger negative effect on returns to seniority for production workers than for managers and clerical workers. Moreover, the results also support the path dependence notions in our framework, in that salary ranges in the SGL system limited wage fluctuation in post-restructuring years.[8]

A potential problem in our estimates of returns to seniority is simultaneity– that employees naturally become older after the restructuring than before. Due to decreasing employee productivity and employer learning, returns to seniority may decrease for older workers (Gibbons and Waldman 2006). Hence, the decreasing returns to seniority that we observe can be a result of restructuring or of employees becoming older. A robustness test is to re-estimate the models in tables 1.2 through 1.4 using employees who were hired and quit before the onset of corporate restructuring. Since restructuring would have no impact on employees who quit before the restructuring

[8]Results from analyses of employees who were hired before the onset restructuring and were still employed after the compensation system change were largely consistent with what we conclude above. In addition, results for tenure and performance measures were robust to an analysis where we include dummy measures of job/task types (and their interaction with the restructuring measure).

even started, we would expect to find no difference in returns to seniority over time for this group. Empirically, this test requires us to re-run the models in tables 1.2 through 1.4 using employees who are not exposed to restructuring (those who quit the firm before 1980) and assume a hypothetical "restructuring" occurred in an arbitrary year in the 1970s. That is, in our robustness test, we used employees who were only with the firm in the 1970s, and we called the early 1970s period as "pre-restructuring" and the late 1970s as "post-restructuring." This hypothetical "restructuring" is thus essentially a placebo that should not have the effect on wage patterns, indicating that we would expect to find no difference in returns to seniority between the early- and late-1970s. We found indeed what we expected in the robustness test: returns to seniority were significant throughout the 1970s, but the magnitude of returns stayed constant. In other words, the robustness tests show the decreasing returns to seniority only occurred in the 1980s, suggesting what we find in tables 1.2 through 1.4 are the true effect of corporate restructuring on wage determinants, rather than an artificial result of decreasing returns to seniority over time as employees become older.

1.5.3 The Effect of Corporate Restructuring on Wage Inequality

We assess effects of corporate restructuring on wage inequality by first providing a graphical depiction of wage percentiles by employee group in figures 1.9 through 1.11. The 10^{th}, 30^{th}, 50^{th}, 70^{th}, and 90^{th} wages percentiles in each year are shown in each graph. Figure 1.9 indicates that, overall, lower tail inequality (the difference between the 50^{th} and 10^{th} percentiles) increased dramatically for production workers. In addition, figure 1.10 shows that upper tail inequality (the difference between the 90^{th} and 50^{th} percentiles) increased faster than lower tail inequality for managers, whereas figure 1.11 demonstrates that both upper tail and lower tail inequality increased at similar paces for clerical workers. Thus, for production workers, inequality increased more rapidly in the lower end of the wage distribution following restructuring, whereas for managers, inequality increased more rapidly in the upper end of the wage distribution.

Although figures 1.9 through 1.11 show how the wage dispersion changes over time, the wage distribution can change due to a "compositional effect" and a "wage structure effect" (Fortin, Lemieux and Firpo 2010), where the composition effect refers to the changes in the distribution of individual and job characteristics and the wage structure effect refers to the change in the way that people are compensated. In our context, the change in inequality caused by changes in the compensation system belongs to the wage structure effect, while the change in inequality due to layoffs and hiring freezes should be categorized as the compositional effect. In order to separate these two effects, we focus on the change in inequality caused by the wage structure effect and ask the question "what would the post-restructuring wage distribution have been had employee characteristics resembled their pre-restructuring levels."[9]

We use a semi-parametric approach proposed in DiNardo, Fortin, and Lemieux (1996), henceforth DFL, to construct a "counterfactual" pre-restructuring wage distribution in which each individual employee is weighted in a way that overall employee characteristics (e.g., age, gender, education, job level,

[9]Alternatively, the question can be asked as "what would the pre-restructuring wage distribution have been had employee characteristics remained at the post-restructuring level?" This, however, only influences which time period is used as the base group and which distribution is weighted. Although Fortin, Lemieux, and Firpo (Fortin et al. 2010) show that using different base groups may change the decomposition outcome, such is not the case for our study.

tenure, and union coverage) resemble post-restructuring levels. Such a "counterfactual" distribution is then compared to the actual post-restructuring wage distribution to obtain the pure effect of restructuring on wages. Hence, the discrepancy between the "counterfactual" and the actual distributions can be interpreted as the change in wage distribution due to factors other than employee characteristics.

Figures 1.12 through 1.14 show results of the DFL decomposition for pre- and post-restructuring periods. We use the 1981 wage distribution as the "pre-restructuring" distribution and the 1990 distribution as the "post-restructuring" one.[10] There are three panels in each graph: the dashed line in the top-left panel denotes the pre-restructuring real wage distribution, and the solid line in the same panel denotes a "weighted" real wage distribution (the distribution that would have prevailed if the employee characteristics prior to restructuring were the same as those following restructuring). The solid line in the top-right panel is the same as the solid line in top-left panel, and the dashed line is the actual real wage distribution following the onset of restructuring. The difference between the two lines in the top-right panel is shown in the bottom-left panel.

If there is no difference in the two lines in the top-right panel, a horizontal line at zero should be observed in the bottom-left panel– which would indicate that employees across the entire range of the wage distribution are paid in the same way prior to and following restructuring (conditional on their characteristics). However, we do not observe a straight line in the bottom-left panel in figure 1.12. Instead, we observe some positive difference when the logarithm of real wage is around ten (equivalent to $22,026 in 2007 Dol-

[10]Since the selection of timing is somewhat arbitrary, we assessed the decomposition using alternative pre-restructuring years (e.g., 1980 or 1982) and post-restructuring years (1989 or 1991). In all cases, patterns were similar.

lars) and eleven (equivalent to $59,874 in 2007 Dollars), and we observe some negative difference when logarithm of real wage is between ten and eleven. This pattern indicates that, compared to the pre-restructuring period, following the onset of restructuring fewer people were paid between $22,026 and $59,874 (in 2007 Dollars), and more people were paid around $22,026 and around $59,874 (conditional on worker characteristics). Briefly, the wage distribution after restructuring is more polarized than before, holding employee characteristics constant.

Similar inspections in the bottom-left panels of figures 1.13 and 1.14 suggest the wage distributions for managers and clericals are more skewed to the left following restructuring than before. Thus, following restructuring the firm is more likely to pay a higher wage than a lower wage to these workers when employee and job characteristics are held constant. Since wages are measured in logarithm form, a “shift” in the compensation scheme indicates increasing wage inequality. Moreover, the discrepancy between weighted pre-restructuring and post-restructuring distributions is larger for managers than for clerical workers, implying that compensation system change impacted managers more than clerical workers.[11] Similar to the robustness test of changing returns to seniority in the previous session, we also performed an alternative set of the DFL decomposition, in which we used 1971 as the pre-restructuring wage distribution and 1980 as the post-restructuring one. We did not find significant changes in wage distributions between 1970 and 1980, again confirming the effect of the corporate restructuring in the 1980s

[11] Additional tests indicate that the variance of log wages grew substantially for all employees following the onset of restructuring: increasing from 0.122 (pre-restructuring) to 0.161 (post-restructuring) for production worker, from 0.111 to 0.135 for managers, and from 0.083 to 0.098 for clericals. Thus, the changes in wage distributions shown in figures 1.12 through 1.14 correspond to a roughly 20% to 30% increase in the variance of log wages.

on wage inequality. Overall, the DFL results indicate that wage inequality increased for all types of employees in post-restructuring years.[12]

1.6 Discussion and Conclusion

Our findings indicate that corporate restructuring had a non-trivial influence on wage inequality due to two key firm-level changes during this widespread process: an increased reliance on market forces to set wages, and a decreased reliance on seniority in salary adjustments due to changes in compensation systems. These two factors influenced all employee groups in similar ways, yet due to differences in path dependencies for the three employee groups– the extent of the effect varied for these groups, leading to substantial within- and between-group variation in wage determinants and outcomes.

Production workers– whose pre-restructuring wages were affected by unions and by seniority-driven rules limiting wage differences– experienced the most dramatic change in wage inequality from corporate restructuring. In particular, the firm's reliance on external forces to set wage levels led to a two-tiered wage structure, where starting salaries of new entrants were reduced by roughly one-half relative to pre-restructuring years. Yet, while the transformation in starting salaries is necessary to explain the effect of restructuring on increased wage inequality for production workers, it is not sufficient. In particular, the firm's use of performance-based compensation systems further increased wage inequality within this work group in non-trivial ways. By contrast, although corporate restructuring increased the wage inequality

[12] In a set of tests, we applied the DFL decomposition to both restructuring practices separately, rather than assessing the total effect of restructuring as in figures 1.12 through 1.14. This alternative set of results supports our claims that (1) following the first RIF, production workers experienced downward wage pressures while managers and clerical workers faced upward pressures; and (2) within-group inequality increased following the implementation of the new performance-based pay system.

inherent in pre-restructuring job and wage structures for managers and clerical workers, the extent of this increase was limited by the staying power of these structures.

Our findings have a number of important implications for research on inequality, organizations, and labor markets. First, consistent with claims of a growing number of scholars (Kim and Sakamoto 2008, Morris and Western 1999), we show that corporate restructuring had a significant influence on wage inequality. Moreover, we shed light on mechanisms for why restructuring affected wages, as well as on how such changes affected employee groups differentially. Thus, our findings not only provide confirmation for the unanswered untested proposition that restructuring influenced inequality, but also provide key insights into the ways in which it did, and why similar restructuring mechanisms nonetheless had substantially different effects on wage determinants and outcomes within and across employee groups.

Second, our findings indicate that the erosion of labor market structures during restructuring did not result in fully "open" employment contracts (Sørensen 2000). That is, job and wage structures continued to have a strong influence on wage inequality throughout the restructuring period (Goldthorpe 2000, Western and Rosenfeld 2011), a finding consistent with Fernandez' (2001) study. For example, figure 1.1 shows that although starting salaries of production workers decreased following restructuring, they experienced substantial increases in wages over timesuggesting perhaps that fairness considerations were still evident in the firm (and/or that returns to performance were considerable). Nevertheless, the nature of the employment relationship is clearly less "closed" following restructuring than before– in that salaries are determined more by short terms factors such as performance in a given year, and less on long term factors such as deferred compensation arrangements.

That is, some of the increase in wage inequality from the 1980s onward was presumably due to the erosion of labor market structures such as ILMs– and to transformations in rules governing wage setting.

Our findings also highlight that structural factors reflecting past employment relationships (and thus the determinants of a given employee group's power relative to a firm's owners) played a key role in explaining why wage inequality differed within and between the employee groups. In doing so they raise questions regarding the reasons for the differential erosion in job and wage structures across groups over time. On one hand, the decline in union power left production workers with less ability to influence wages (Western and Rosenfeld 2011). On the other hand, the greater relative power of managers afforded them the ability to preserve job and wage structures during restructuring. Yet, there are some patterns that do not derive easily from a bargaining power perspective. For instance, although managers experienced real (inflation-adjusted) wage growth following restructuring, they also were laid off disproportionately during the restructuring period (Cappelli 1992). In addition, the preservation of clerical workers' job and wage structures does not coincide with the relative lack of power of this group– particularly since it was disproportionately female and minority, and thus arguably should have less protection against forces of restructuring (Reskin 2003).[13]

These patterns suggest that a promising avenue of future research would be to examine how diversity influences wage patterns and outcomes in relation to broader external forces, doing so in a way that draws on important research showing how contextual factors and organizational systems, practices, and policies influence outcomes in this regard (Fernandez-Mateo 2009, Gorman

[13] In the firm that we study, 60% of the clerical workers are females, compared to roughly 20% for both production workers and managers. Also, 24% of clerical workers are minorities, while 17% of the production workers and 10% of the managers are.

and Kmec 2009, Haveman, Broschak and Cohen 2009, Kalev 2009). Research could also be advanced by considering differences in bargaining power within groups, such as examining how performance differentials impact a variety of employment outcomes. For example, our findings suggest that a key source of post-restructuring power for many types of employees is merit.

Third, our results provide important evidence on how restructuring, in the form of variable-pay systems, affects returns to seniority and wage inequality– the nature and effect of which has not been widely explored in the extant literature. Our findings are consistent with available research indicating that pay-for-performance systems account for roughly 20% of the growth in variance in male wages from the late 1970s to the early 1990s (Lemieux et al. 2009). Yet, they also suggest that this study may underestimate the effect of performance on wages, since these scholars only considered whether there was an increase in the number of jobs whose wages were entirely dependent on performance (e.g., commission-based pay jobs). We show that the effects of performance matter for many types of work.

Finally, we provide an alternative institutional explanation to the skill biased technological change (SBTC) argument for the widening of wage dispersion and inequality in 1980s. For example, the change in wage patterns for managers and clerical workers under our semi-parametric decomposition is somewhat similar to SBTC mechanisms (Autor et al. 1998, Fernandez 2001). Yet even though SBTC arguably occurred at a time when employees who used computers saw their wages increase faster than other people, our results suggest that this outcome may not have much (at least directly) to do with the increasing returns to education and widening inequality.[14] Thus,

[14]For example, to the extent that wage distributions are influenced by increasing demands for education and other skills, managers may experience a stronger upward push on overall wages relative to other workers, yet we maintain these changes are modified

although our study in some ways can be seen as a complement to the literature focusing on economy-wide antecedents of increasing wage inequality (e.g., SBTC and occupations), it also highlights the importance of assessing the change in inequality from a firm-level point of view in order to identify the processes leading to the changing wage structure– a feature often ignored in the extant literature which treats firms as having little role in key outcomes.

Because the findings reported in this article stem from one large firm located in one economic sector, there may be questions regarding potential generalizability. Several factors reduce these concerns. First, many of our pre-restructuring results are similar to findings in other single-firm studies in the same time frame (Petersen and Saporta 2004). Second, like most large firms, the firm we studied restructured multiple times (Cascio et al. 1997), and relied on external advice from consultants on the design and implementation of restructuring initiatives, and from senior managers sent to other firms to examine best practices. Third, it is important to note that organizational theory has frequently been advanced and tested with single-firm studies (Fernandez 2001), an important factor given that many data sets used to analyze inequality are sorely lacking in organizational characteristics (Morris and Western 1999). Finally, our study is a response to the call to conduct firm-level studies to better understand organizational-level mechanisms in wage determination.

To summarize, our study helps to fill in the critical knowledge gaps in the literature on wage inequality by theorizing about and empirically exploring the previously understudied role of corporate restructuring in this regard. In particular, we assess the change in inequality from a firm-level point of view

by job and wage structures. In our data, we also observe a widening college-high school wage gap starting in 1980s, consistent with the literature in wage inequality using publicly available individual level data such as the CPS (Autor et al. 1998).

in order to identify the processes leading to the changing wage structure. We demonstrated not only whether and how restructuring affected wage inequality, but also why this critical outcome varied within and across employee groups– even though the restructuring mechanisms operated in similar ways for these groups. Our findings suggest that although firms still matter for wage inequality– particularly with respect to their job and wage structures– their ability to limit the effects of market forces on wages has been weakened by RIF, and the way they set wages internally has been transformed substantially by changes in compensation and performance management systems.

Figure 1.1: Salary Paths by Cohort, Production Workers

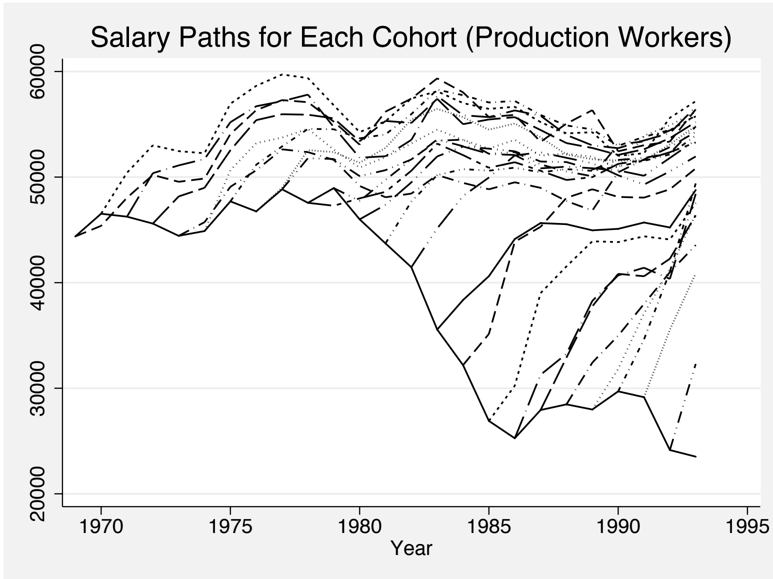

Figure 1.2: Salary Paths by Cohort, Managers

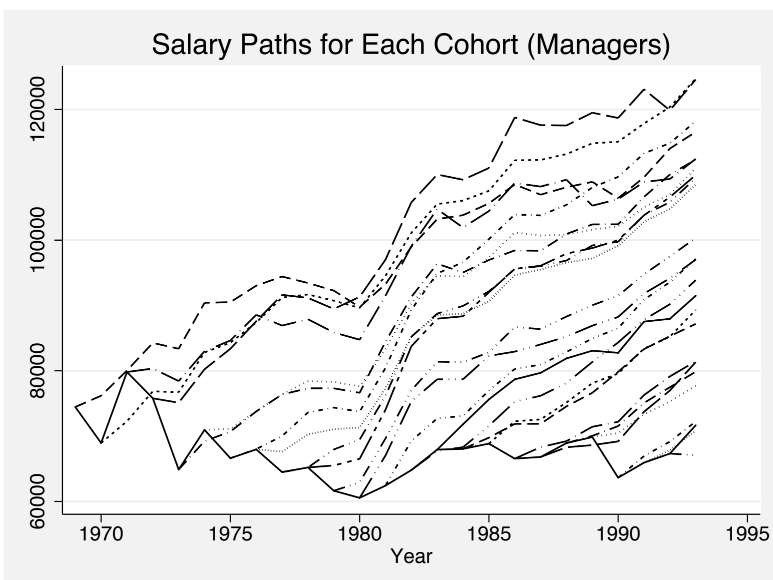

Figure 1.3: Salary Paths by Cohort, Clerical Workers

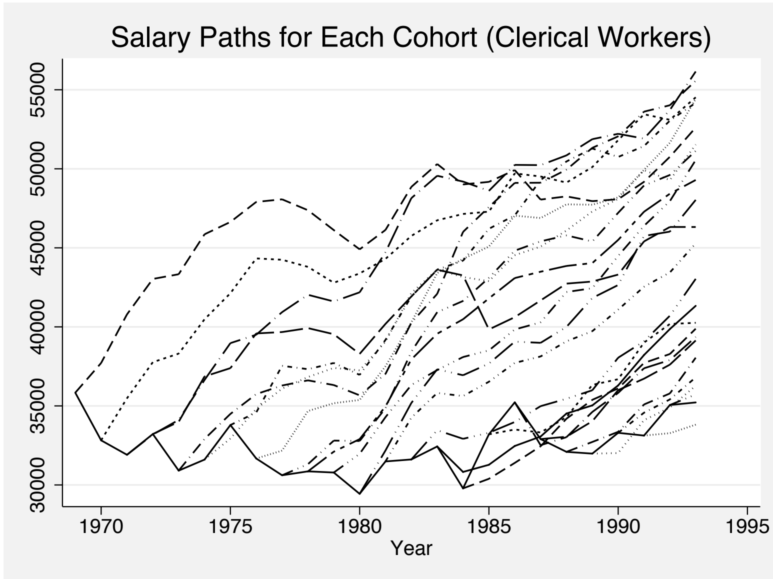

Figure 1.4: Industry vs Firm Wage Patterns

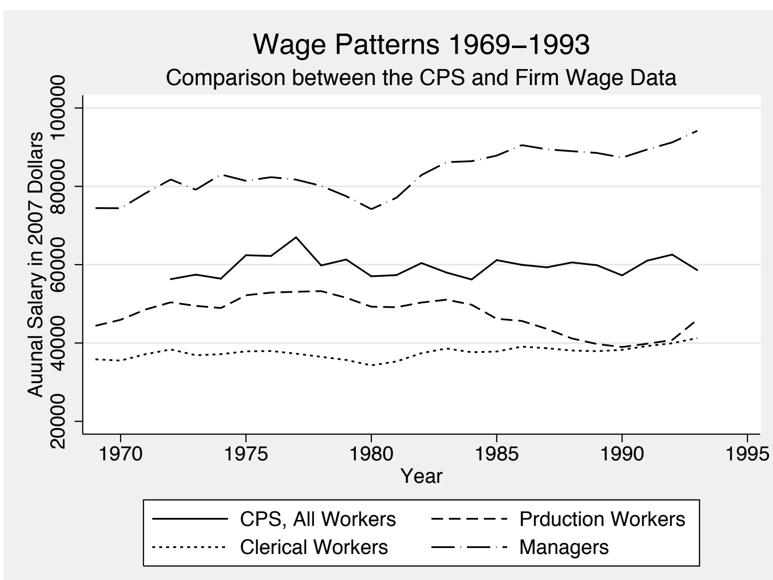

Figure 1.5: Industry vs Firm Wage Patterns, Production Workers

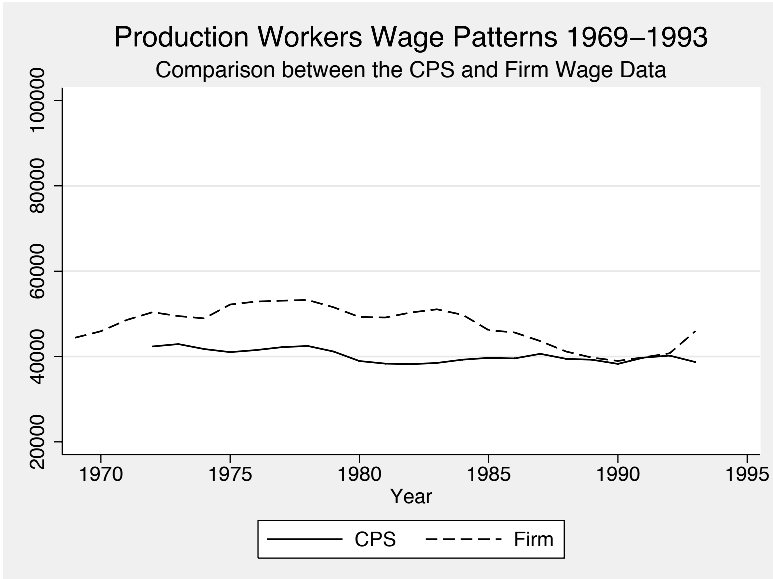

Figure 1.6: Industry vs Firm Wage Patterns, Clericals

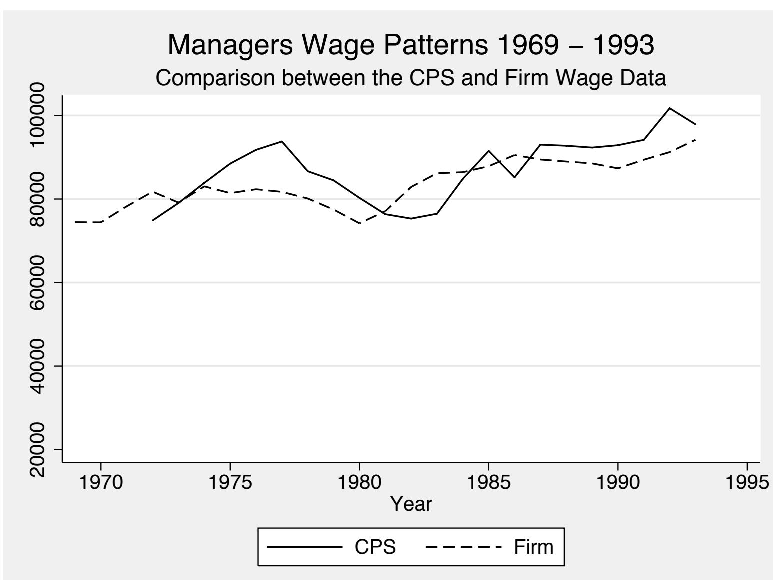

Figure 1.7: Industry vs Firm Wage Patterns, Starting Salaries

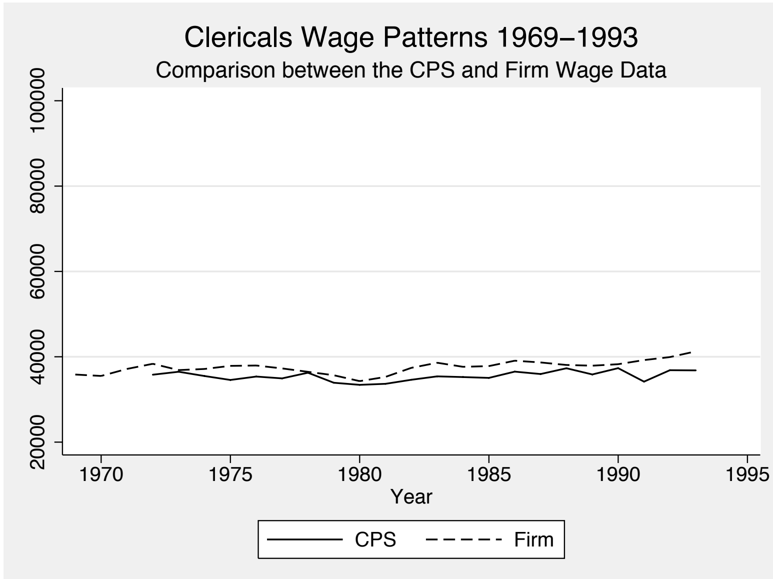

Figure 1.8: Changes in Returns to Seniority

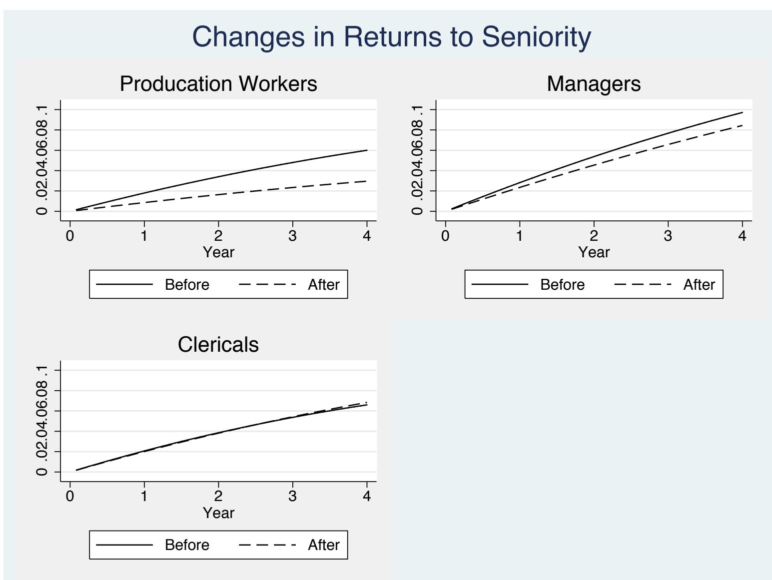

Figure 1.9: Wage Dispersion of Production Workers

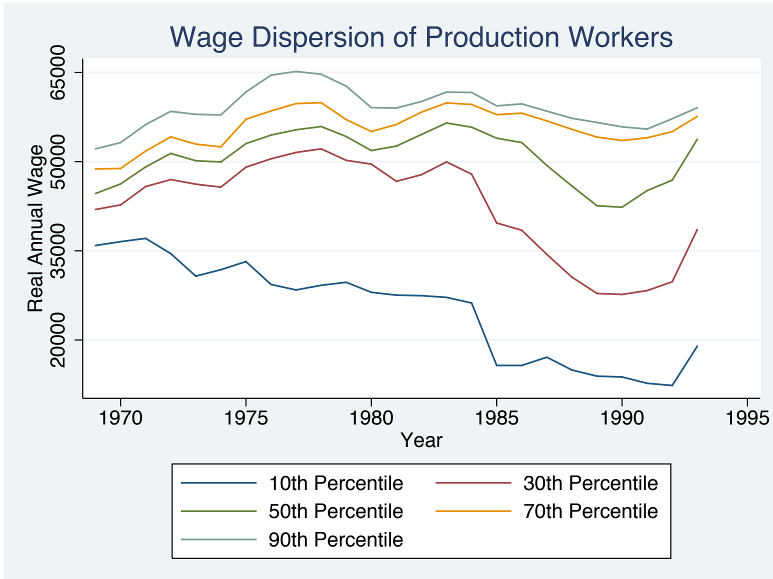

Figure 1.10: Wage Dispersion of Managers

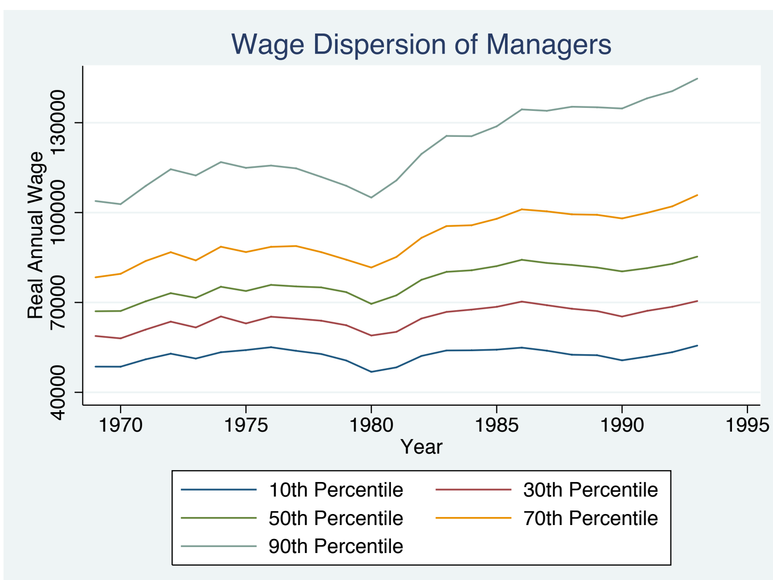

Figure 1.11: Wage Dispersion of Clerical Workers

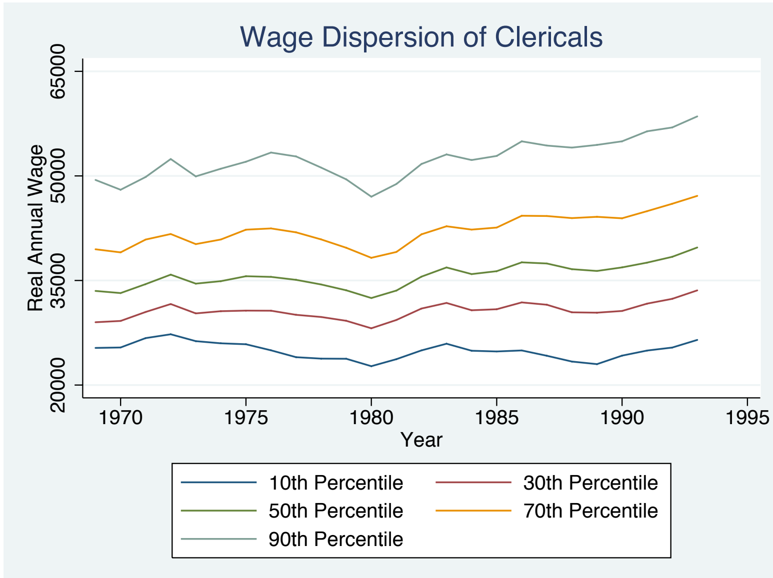

Figure 1.12: DFL Decomposition for Production Workers

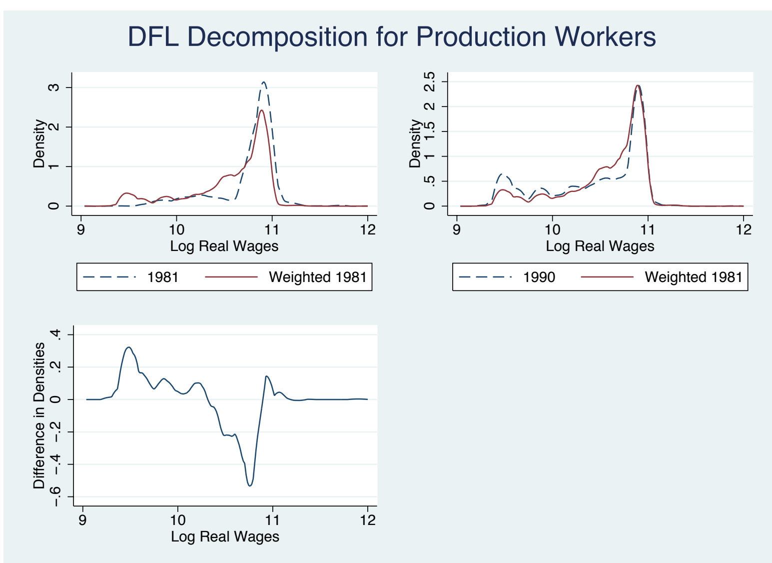

Figure 1.13: DFL Decomposition for Managers

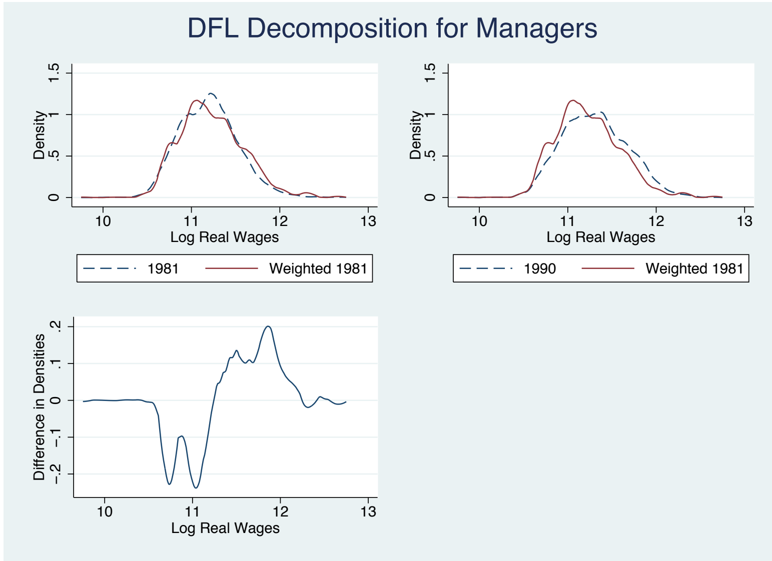

Figure 1.14: DFL Decomposition for Clerical Workers

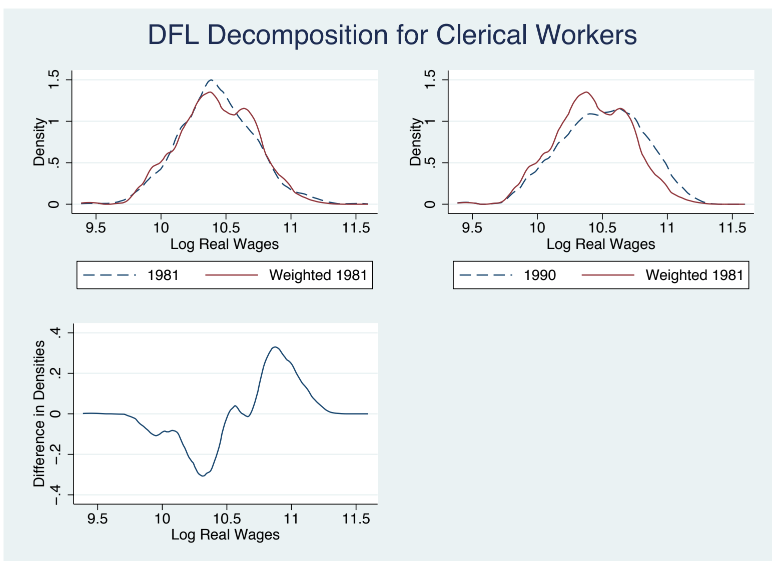

Table 1.1: Descriptive Statistics

	Production Workers			Managers			Clerical Workers		
Year	**Wage**	**Tenure**	**Age**	**Wage**	**Tenure**	**Age**	**Wage**	**Tenure**	**Age**
1969	$44,072	0.62	42	$35,426	0.60	31	$73,869	0.62	40
1970	$45,030	1.13	42	$35,177	1.16	31	$73,926	1.38	39
1971	$49,870	1.81	41	$38,045	1.81	32	$80,341	2.22	39
1972	$50,899	2.51	41	$38,551	2.43	32	$82,410	3.01	39
1973	$51,593	3.01	39	$38,121	2.60	31	$82,533	3.60	39
1974	$49,960	3.27	36	$37,681	2.70	31	$85,092	3.96	38
1975	$53,014	3.57	35	$38,302	3.28	31	$82,517	4.63	37
1976	$54,422	3.73	34	$38,825	3.56	31	$84,393	4.85	37
1977	$54,279	3.86	33	$37,856	3.78	31	$83,222	5.20	36
1978	$55,512	4.21	33	$37,690	3.92	31	$83,174	5.42	35
1979	$59,398	4.33	33	$36,807	4.01	31	$80,569	5.58	35
1980	$50,749	4.41	32	$34,915	3.91	31	$75,768	5.49	34
1981	$50,319	4.53	33	$35,607	3.86	31	$78,053	5.47	34
1982	$51,019	5.03	33	$37,660	4.43	32	$83,540	5.96	34
1983	$51,510	5.73	34	$38,589	5.05	32	$86,327	6.72	35
1984	$49,928	6.19	35	$37,648	5.52	33	$86,503	7.63	35
1985	$46,155	6.17	34	$37,691	5.85	34	$87,692	8.02	36
1986	$45,417	6.69	35	$38,739	6.68	34	$89,830	8.84	36
1987	$44,816	7.06	35	$38,551	7.05	35	$89,264	9.35	36
1988	$43,748	7.08	35	$38,212	6.55	35	$89,406	9.43	37
1989	$4 3,179	7.44	36	$37,822	6.49	35	$88,416	9.49	37
1990	$43,446	7.74	36	$38,620	6.71	35	$88,265	9.70	37
1991	$44,140	8.36	37	$39,557	6.95	36	$90,218	10.06	37
1992	$45,180	9.14	38	$40,189	7.70	37	$91,902	10.79	38
1993	$46,207	9.32	39	$40,894	7.83	37	$93,341	11.04	38

Only employees hired after 1968 are included.
Wages are in 2007 U.S. dollars.
Tenure are in years.

Table 1.2: Employee-Job Level Fixed Effect Estimations of Returns to Seniority, Production Workers

Dependent Variable: Logarithm of Salary in 2007 U.S. Dollars				
	Model 1	**Model 2**	**Model 3**	**Model 4**
Job Tenure	-0.0434***	-0.0442***	-0.0388***	-0.0512***
	(0.0033)	(0.0032)	(0.0032)	(0.0034)
Job Tenure Squared	-0.0007***	-0.0006***	-0.0010***	-0.0006***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Linear Time Trend	0.0577***	0.0589***	0.0578***	0.0630***
	(0.0030)	(0.0090)	(0.0028)	(0.0029)
Post Restructuring Dummy		-0.0276***	0.0058	0.0002
		(0.0052)	(0.0098)	(0.0091)
Post Restructuring Dummy X Job Tenure			-0.0100***	-0.0081***
			(0.0017)	(0.0016)
Post Restructuring Dummy X Job Tenure Squared			0.0006***	0.0004***
			(0.0001)	(0.0001)
Performance				0.0096***
				(0.0005)
Post Restructuring Dummy X Performance				0.0619***
				(0.0053)
Number of Employees	6,773	6,773	6,773	6,773
Number of Employee-Job Level Matches	6,773	6,773	6,773	6,773
Number of Records	34,808	34,808	34,808	34,808
Minimum Match Spell (Years)	1	1	1	1
Maximum Match Spell (Years)	25	25	25	25
Mean Match Spell (Years)	5.1	5.1	5.1	5.1

Additional control variables in all model: A set of dummy variables indicating divisions of the firm, a dummy variable indicating the occurrence of demotion, and the estimated likelihood of employment separation.
Standard errors are clustered by employee-job level matches and are shown in the parentheses.

*** : $p < 0.001$

** : $p < 0.01$

* : $p < 0.05$

Table 1.3: Employee-Job Level Fixed Effect Estimations of Returns to Seniority, Managers

Dependent Variable: Logarithm of Salary in 2007 U.S. Dollars				
	Model 1	**Model 2**	**Model 3**	**Model 4**
Job Tenure	-0.0165***	-0.0157***	-0.0102***	-0.0187***
	(0.0018)	(0.0019)	(0.0017)	(0.0017)
Job Tenure Squared	-0.0009***	-0.0009***	-0.0013***	-0.0014***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Linear Time Trend	0.0409***	0.0408***	0.0397***	0.0489***
	(0.0016)	(0.0016)	(0.0013)	(0.0015)
Post Restructuring Dummy		-0.0136***	-0.0145***	-0.0239***
		(0.0034)	(0.0023)	(0.0026)
Post Restructuring Dummy X Job Tenure			-0.0052**	-0.0037***
			(0.0009)	(0.0009)
Post Restructuring Dummy X Job Tenure Squared			0.0005***	0.0004***
			(0.0001)	(0.0000)
Performance				0.0094***
				(0.0007)
Post Restructuring Dummy X Performance				0.0158***
				(0.0021)
Number of Employees	8,517	8,517	8,517	8,517
Number of Employee-Job Level Matches	20,906	20,906	20,906	20,906
Number of Records	67,276	67,276	67,276	67,276
Minimum Match Spell (Years)	1	1	1	1
Maximum Match Spell (Years)	25	25	25	25
Mean Match Spell (Years)	3.2	3.2	3.2	3.2

Additional control variables in all model: A set of dummy variables indicating divisions of the firm, a dummy variable indicating the occurrence of demotion, and the estimated likelihood of employment separation.
Standard errors are clustered by employee-job level matches and are shown in the parentheses.
*** : $p < 0.001$
** : $p < 0.01$
* : $p < 0.05$

Table 1.4: Employee-Job Level Fixed Effect Estimations of Returns to Seniority, Clerical Workers

Dependent Variable: Logarithm of Salary in 2007 U.S. Dollars				
	Model 1	**Model 2**	**Model 3**	**Model 4**
Job Tenure	-0.0321***	-0.0323***	-0.0266***	-0.0371***
	(0.0021)	(0.0023)	(0.0021)	(0.0022)
Job Tenure Squared	-0.0009***	-0.0009***	-0.0014***	-0.0013***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Linear Time Trend	0.0509***	0.0509***	0.0487***	0.0588***
	(0.0016)	(0.0015)	(0.0014)	(0.0016)
Post Restructuring Dummy		0.0050	-0.0147***	-0.0260***
		(0.0046)	(0.0028)	(0.0035)
Post Restructuring Dummy X Job Tenure			-0.0010	0.0026
			(0.0013)	(0.0014)
Post Restructuring Dummy X Job Tenure Squared			0.0004***	0.0002*
			(0.0001)	(0.0001)
Performance				0.0294***
				(0.0036)
Post Restructuring Dummy X Performance				0.0267***
				(0.0067)
Number of Employees	10,099	10,099	10,099	10,099
Number of Employee-Job Level Matches	16,676	16,676	16,676	16,676
Number of Records	46,173	46,173	46,173	46,173
Minimum Match Spell (Years)	1	1	1	1
Maximum Match Spell (Years)	24	24	24	24
Mean Match Spell (Years)	2.8	2.8	2.8	2.8

Additional control variables in all model: A set of dummy variables indicating divisions of the firm, a dummy variable indicating the occurrence of demotion, and the estimated likelihood of employment separation.
Standard errors are clustered by employee-job level matches and are shown in the parentheses.
*** : $p < 0.001$
** : $p < 0.01$
* : $p < 0.05$

CHAPTER 2

UNEMPLOYMENT AND THE DYNAMICS OF HEALTH INSURANCE COVERAGE

2.1 Introduction

One of the most distinctive institutional arrangements in the U.S. labor market is that the health insurance coverage for the non-poor and non-elderly population is provided as a part of an individual's compensation package. Among this population, health insurance coverage for an individual is hence largely conditional on employment as well as the employer's offering of health insurance benefits. Such arrangement not only has significant impacts on employment dynamics (Buchmueller and Valletta 1996, Cooper and Monheit 1993, Gilleskie and Lutz 2002, Gruber and Madrian 1994, Gruber and Madrian 1997, Gruber and Madrian 2004, Madrian 1994b, Monheit and Cooper 1994) but also directly leads to the dynamics in health insurance coverage– the phenomenon that people move into and out of health insurance coverage over time (Fairlie and London 2009a, Fairlie and London 2009b, Short and Graefe 2003).

Bhandari and Mills (2003), for example, find that 16% of the full time workers in the U.S. experienced at least one month without health insurance each year. Fairlie and London (Fairlie and London 2009a, Fairlie and London 2009b) furthermore document the relationship between demographic variables and the probability to gain or lose health insurance coverage. Their results, however, do not account for the dynamics in employment. Due to the correlation between employment and the provision of health insurance coverage, the dynamics of health insurance coverage is also likely to be a function of employment dynamics, including moving from one employer to another and into and out of the labor force.

From the "job lock" literature (Gilleskie and Lutz 2002, Gruber 2000, Gruber and Madrian 1994, Gruber and Madrian 2004, Madrian 1994b), we know that the dynamics of employment is constrained by employer offering of health insurance benefits at the current job. Employees are less likely to switch to a new employer or retire if such move is accompanied by the loss of health insurance coverage. However, we know less about how people acquire health insurance coverage by moving from unemployment to employment or from a job that does not provide health insurance benefits to a job that does. We also do not know much about the long-term effect of an extended period of unemployment on health insurance coverage.

This paper intends to examine the dynamics of health insurance coverage– how an individual's health insurance coverage over lifetime– changes as a function of both past coverage as well as past and current employment status. In order to assess the causal relationship between health insurance coverage and employment status, it is crucial to incorporate the correlation between health insurance coverage and employment into the model. The health insurance coverage and employment status can be correlated either because they are causally related, or because they are correlated due to unobserved individual heterogeneity, or both. It poses empirical challenges to differentiate between these explanations for an observed correlation over time, and the Panel Study of Income Dynamics (PSID) has features that help to solve the problem.

This study uses 1999 through 2009 waves of the PSID, which provide information on both health insurance coverage and employment history. Although the PSID has become biennial since 1997, in each wave it surveys an individual's health insurance coverage in the past two years as well as the recent employment history for up to four jobs (including the current one). A twelve-year panel can be constructed accordingly, starting from 1997 to 2008. My results suggest that around 40% of the male population aged 22-64 has lost health insurance coverage for at least one month during the twelve-year period. Around two-thirds of this population experienced some spell of unemployment during the span. Those who are younger than 30, single, less educated, or having household earnings less than 200% of the Federal Poverty Level are also more likely to drop out of coverage. Most of the correlation between health insurance coverage and unemployment over time can be attributed to the correlation between them in each year and the state dependence of unemployment. After these correlations are accounted for, the causal relationship between unemployment and health insurance coverage becomes much smaller: unemployment spells of any length in two consecutive years reduces the likelihood of health insurance coverage by less than 2 percentage points.

The remaining of this paper is structured as follows: the next section presents the related literature and methods used in this paper. It is followed by an introduction of the PSID data, the descriptive statics, and a presentation of health insurance coverage dynamics. I then estimate the effect of unemployment on health insurance coverage before I conclude the paper.

2.2 Framework

2.2.1 Related Literature

Health insurance benefit is quickly becoming one of the most important components in employee compensation package in the United States. Since the New Deal era, most of the non-poor and non-elderly population in the U.S. has relied on employer-sponsored health insurance as the main source of coverage. The provision (or lack of) health benefits have been found to impact job choice and labor supply decisions. The dynamics of health insurance coverage, and hence, is highly correlated with the dynamics of employment.

The literature in job lock shows that the offering of health insurance at the current job may have an effect on labor supply decisions and limit job mobility. Assuming that both the workers' preference for health insurance and the employers' cost to provide health benefits are continuous, workers and firms would be sorted into an equilibrium in which workers who desire health insurance the most would work for employers who can offer health insurance in the least expensive manner. In a perfect competitive labor market, there should exist an universal compensation differential ΔW (Rosen 1986), and the workers who choose to enroll in employer-sponsored insurance plans are paid ΔW lower than their otherwise equivalent counterparts who do not enroll in health insurance plans. Nevertheless, to the extent that the availability of job options is limited, a worker who prefers to have health insurance coverage may choose not to move to a better job (in terms of job match, compensation, career opportunity, etc.) if that new job does not offer health insurance or the higher compensation accompanied does not offset the cost of a self-purchased health insurance plan. Alternatively, workers may be discouraged from quitting from the current job in order to find a better job match due to the lack of health insurance when unemployed. In other words, workers would be "locked" into their current jobs with health insurance benefits even when better job opportunities are available.

The negative impacts on job mobility caused by job lock is shown to be alleviated by the provision of continuation coverage such as COBRA. The Consolidated Omnibus Budget Reconciliation Act of 1985 (COBRA) is designed to ease the "job lock" problem by allowing outgoing employees to stay with their original employer-sponsored health insurance plans for up to 18 months upon job separation. Although it is estimated that only around 2% workers in a given year enroll in the COBRA (Madrian 1998)[1], it is well documented that COBRA effectively reduces the magnitude of job lock. For example, Gruber and Madrian (1997) find that continuation coverage increases the transition from employment to not in the labor force, the time spent not in the labor force, and reemployment earnings. Both Cooper and Monheit (1993) and Madrian (1994b) find that the lack of health insurance portability reduces turnovers; on the other hand, one year of continuation coverage increases job turnover by 10% (Gruber and Madrian 1994). Buchmueller and Valletta (1996) and Anderson (1997) further show that own employer-sponsored health insurance coverage reduces turnovers and mobility.

In addition to job lock, researchers have also focused on the effect of health insurance provision on retirement and female labor supply. Studies show that the availability of retiree health insurance (Gustman and Steinmeier 1994, Karoly and Rogowski 1994, Madrian 1994a) or continuation of coverage (Gruber and Madrian 1995, Gruber and Madrian 1996) leads to a

[1] On average, only about 10 percent of the workforce would experience a qualifying event that potentially triggers COBRA in a given year and some 20% among them would actually elect COBRA coverage (Madrian 1998).

higher probability of people retiring before they are eligible for Medicare. For those who are married, having spouses who have health benefits for the whole family increases the likelihood of turnover, decrease the hours worked, and increases the likelihood to take a prat-time job (Buchmueller and Valletta 1996, Olson 1998, Schone and Vistnes 2000). Overall, these results have two important implications: first, there is a close relationship between health insurance coverage and employment status; second, while coverage and employment are correlated, the literature mainly focuses on how the availability of health insurance coverage influences turnover or labor supply.

The other direction of the causal relationship between coverage and employment, namely, how workers without coverage move from "unemployed" to "employed and covered" or from "employed but not covered" to "employed and covered" to acquire health insurance coverage, is not widely examined. For example, how does an extended period unemployment impact the probably that an uninsured worker gains health insurance in the next period? How long does the impact last? For example, the literature in employer learning would suggest the experience of layoff is a negative signal to prospective employers and hence has adverse effect on future employment opportunity and wages (Altonji and Pierret 2001, Farber and Gibbons 1996, Podgursky and Swaim 1987, Schønberg 2007). Since health insurance coverage is a component of employee benefit, past unemployment likely also will decrease the likelihood of health insurance coverage.

Although it is an important question to distinguish the causal relationship between health insurance coverage and employment status, it is nevertheless empirically challenging. For instance, a worker can be uninsured because of a previous spell of unemployment, or because of her own preference in both demand for health insurance and labor supply. Due to the correlation between coverage and employment as well as the unobservable individual heterogeneity that simultaneously affects demand for health insurance and job choices, a model that does not account for the full correlations among these variables and their lagged terms are likely to yield inaccurate estimates.

Adding another layer of complexity to this empirically challenging question is the state dependence of unemployment (Arulampalam, Booth and Taylor 2000, Doiron and Gørgens 2008, Heckman and Borjas 1980, Stewart 2007). That is, even if there is no long-term causal relationship between unemployment and coverage, the correlation between them in each period as well as the state dependence of unemployment would lead to correlations between health insurance coverage and unemployment over time in the data. Additionally, the observed health insurance outcome is likely to be state dependent as well due to the unobserved individual heterogeneity that correlates with an individual's health insurance coverage. In an earlier version of this paper (Fang 2010), I show that the states of being insured or uninsured are strongly correlated over time: those who are insured in one period are more likely to stayed insured in the next period, while those who are not insured tend to stay uninsured for the very next period. Such state dependencies (serial-correlations) of unemployment and health insurance coverage make the identification of the causal effect between their dynamics over time more difficult.

This study utilizes the panel structure of the PSID to address the causal effect of unemployment on health insurance coverage. The information on both health insurance coverage and employment status in a twelve-year panel allows me to distinguish the causal relationship between the two. The following subsection introduces the econometric models.

2.2.2 The Causal Relationship between Unemployment and Health Insurance Coverage Dynamics

In another chapter in my thesis (Fang 2011), I show that an individual's health insurance coverage is largely conditional on being hired by an employer who offers health insurance benefits. If the coverage is determined only by the worker's demand, a model that can be used to estimate the likelihood of having health insurance coverage can be written as

$$Pr(HI_t = 1) = X^\top \beta + u_i + \varepsilon_{it}, \tag{2.1}$$

where X is a set of independent variables including individual and job characteristics observable to the researcher, u_i is the unobserved individual heterogeneity that also determines health insurance coverage, and ε_{it} is the residual term.

However, the dominance of employer-sponsored health insurance benefits suggests that health insurance coverage and employment are correlated. The interaction between health insurance coverage and employment can be expressed in a set of equations,

$$\begin{aligned} Pr(HI_t = 1) &= Pr(HI_t = 1, E_t = 1) + Pr(HI_t = 0, E_t = 1) \\ Pr(HI_t = 0) &= Pr(HI_t = 1, E_t = 0) + Pr(HI_t = 0, E_t = 0) \end{aligned}$$

which simply says that depending on the health insurance coverage and employment status, people can be categorized in four groups, with $(HI_t = 1, E_t = 1)$ representing people who are insured and employed, etc.[2] This means the health insurance coverage $Pr(HI = 1)$ cannot be estimated as a single equation because coverage also depends on employment status. For

[2]The term $Pr(HI_t = 0, E_t = 1)$ can be positive due to COBRA, spousal coverage, or the public health insurance programs.

example, in the one-equation setting, it is not possible to tell whether the lack of coverage is caused by unemployment ($HI_t = 0, E_t = 0$) or by having a job without health insurance benefits ($HI_t = 1, E_t = 0$). And hence, without controlling for the employment history and the correlation between coverage and employment, equation (2.1) is by no means structural.

The interaction between coverage and employment hence suggests a bivariate probit model,

$$\begin{aligned} Pr(HI_t = 1) &= X_{HI}^{\top}\beta_{HI} + u_i + \varepsilon_{it}^{HI} \\ Pr(E_t = 1) &= X_E^{\top}\beta_E + \rho u_i + \varepsilon_{it}^{E} \end{aligned}$$

in which the likelihoods of coverage and employment are estimated jointly, and the unobserved heterogeneity terms in both equations are allowed to be correlated.

Adding the coverage and employment in previous period(s) allows a causal interpretation of how employment dynamics influences health insurance. Denoting health insurance coverage and employment status at time t by HI_t and E_t, respectively, the model can be written as

$$Pr(HI_t = 1) = X_{HI}^{\top}\beta_{HI} + \sum_{s=1}^{T}\varphi_s^{HI} HI_{t-s} + \sum_{s=0}^{T}\varphi_s^{E} E_{t-s} + u_i + \varepsilon_{it}^{HI} \tag{2.2}$$

$$Pr(E_t = 1) = X_E^{\top}\beta_E + \sum_{s=1}^{T}\xi_s^{HI} HI_{t-s} + \sum_{s=1}^{T}\xi_s^{E} E_{t-s} + \rho u_i + \varepsilon_{it}^{E} \tag{2.3}$$

and the vector of coefficients $\varphi_t^E, \varphi_{t-1}^E ...$ captures the causal relationship between employment status and health insurance coverage when equations (2.2) and (2.3) are jointly estimated in a bivariate probit model.

In my estimation, HI_t is one if an individual is always insured in year t, and E_t is one if an individual is not employed for 12 months in year t (so

E_t denotes the existence of any unemployment spell in a year). The lagged terms on the right hand side, HI_{t-s} and E_{t-s}, are defined in similar manners and denotes health insurance coverage and employment status in year $t-s$. The set of control variables X_{HI} includes age (in categories of 22-29, 30-39, 40-49, 50-59, and 60-64), highest level of education (high school dropout, high school, some college, college, or post-secondary), race (white, black, Hispanic, or other), marital status (married or not), number of kids younger than 18 in the household (in categories of 0, 1, 2, and 3 or more), job tenure (not working, less than 1 year, 1 to 3 years, 3 to 5 years, and 5 years or above) and household earnings in year t (below the Federal Poverty Level, 100%-200% of the FPL, 200%-400% of the FPL, or above 400% of the FPL). The set of X_E is the same as X_{HI} minus job tenure and household income.

2.3 Data

2.3.1 The Panel Study of Income Dynamics

The Panel Study of Income Dynamics (PSID) is a longitudinal survey of representative U.S. individuals and the families that they reside in. When it started in 1968, the PSID had two independent samples: a cross-sectional national sample and a national sample of low-income families. The cross-sectional sample was drawn by the Survey Research Center (SRC), having 2,930 households, and was an equal probability sample of U.S. families in contiguous 48 states. The sample from low-income families, also know as Survey of Economic Opportunity (SEO) sample, had 1,872 households and was conducted by the Bureau of the Census for the Office of Economic Opportunity (Hill 1991). The PSID core sample constitutes these two samples.

Between 1968 and 1996, the PSID interviewed individuals from the households in the core sample. Household splits and merges were both tracked; adults were observed as they aged, and children were followed as they grew into adulthood and formed their own family units. Starting from 1997, PSID became a biennial survey, and the most recent wave of PSID available was conducted in 2009. Many questions regarding health status and health care coverage were added into PSID questionnaires beginning from 1999, which makes the PSID an extremely valuable long panel to study the dynamics of health insurance coverage. Finally, since the composition of the U.S. population has changed dramatically since 1968, the PSID also provides cross sectional sample weights each year to ensure its representativeness.

I estimate how unemployment impacts the dynamics of health insurance coverage using the 1999-2009 waves of the PSID core sample. Only male PSID household heads aged between 22 and 64 are included in the analysis, regardless of employment status and labor force participation. Female household heads are dropped because female wage earners tend to have different incentives to obtain health insurance coverage.[3] People who are older than 65 would be eligible for Medicare automatically and hence are dropped since they would not contribute to the dynamics of health insurance coverage. Only individual records that are older than 65 are dropped; that is, if a person turns 65 between 1999 and 2009, his records before aged 65 are still retained. Also, records that are younger than age 22 are excluded because people younger than 22 are likely to be students; for students, their health insurance coverage may not be a direct outcome of employment due to the

[3] Since the PSID always counts the male as the household head between a married couple, female household heads in the PSID are either not married or married but separated from their husbands. This makes the availability of spousal coverage a less concern. However, pregnant women under a certain income threshold would still be eligible for subsidized public health insurance plans.

provision of student insurance. Households that were part of the Survey of Economic Opportunities subsample are dropped to obtain a random sample since the Survey of Economic Opportunities over-sampled households in lower social-economic status. Overall, 5,262 individuals and 22,729 records are used in the analysis. Numbers of records are 3,481, 3,627, 3,726, 3,833, 3,962, and 4,100 in each biennial wave, starting from 1999 and ending in 2009.

The PSID has two major components: the individual survey and the household survey. Individual characteristics, such as age, years of education and types of health insurance are obtained from the individual survey. Earnings in the previous year, hours worked/unemployed in the previous year, employment history, and number of months covered by health insurances are obtained from the household survey. Years of education is not available in all waves of PSID; in the cases of missing data, information from the most recent wave is used to fill in. Four types of earnings are available in PSID: labor earnings, unemployment benefits, social security benefits, and other transfer earnings. These information are used to calculate household earnings, and all the dollar amounts are deflated to 2010 U.S. dollars using the CPI-W. Descriptive statistics of these variables, weighted by the PSID crosssectional sample weights to ensure the representativeness of the sample, are provided in table 2.1.

2.3.2 Employment History in the PSID

In each wave of the interview, the PSID surveys the employment history of both the head and the wife. It first asks the interviewee whether he or she is working, unemployed, or out of labor force. For those who are working, information about the type of the employer (public, private, or self-employed), industry, occupation, and when the job started are asked. For those who are not working, the starting and end dates of the last job is recorded. Regardless of employment status, the PSID asks information on up to three additional jobs. These additional jobs can be either a previous job that the interviewee worked before the current main job or an "extra job" that occurred simultaneously with one of the main jobs. For these additional jobs, both starting and end dates (if not censoring) are recorded. Based on such information, the employment history of an interviewee can be constructed. Given the biennial nature of the PSID and that PSID surveys the interviewee early in the year, I use information from year t of the PSID to construct the employment history in years $t-1$ and $t-2$. That is, employment history in 1997 and 1998 is constructed based on the 1999 wave of PSID, employment history in 1999 and 2000 is based on the 2001 wave, etc. The employment history is then transformed into monthly basis, that is, a PSID individual is coded to be working in a certain month if he is employed in that month; otherwise, the person is coded to be not working (unemployed). The monthly employment history hence consists of a sequence of binary variables indicating whether an individual is employed in a given month. Unfortunately, the PSID only asks about job search behaviors four weeks prior to the time of survey for those who are not working at the time of survey, and there is no reliable information to distinguish between unemployed and not in the labor force for each month in the employment history.

One of the limitation to the PSID employment history is that only up to four jobs are recorded in each wave. These jobs are recorded chronically, with the most recent one recorded first; however, if an interviewee worked for more than four jobs between the current and previous waves of the PSID, information regarding the earlier jobs will not be available. Since the only feasible way to construct the employment history from the PSID is to count people as employed when they report they are working based on the job information available in the data, people would be counted as “unemployed” when they actually are working for the jobs that are not reported in the PSID. Jobs not reported in the PSID are likely to occur earlier than later in the two-year period of the monthly employment history given how job history is chronically recorded, hence, employment is likely lower in the earlier part of the two-year period. For instance, the 1999 wave of PSID is used to construct the employment history in 1997 and 1998. The jobs not recorded in the PSID are more likely to occur early in 1997 than late in 1998, so it is expected that the monthly employment rate would be lower in early 1997 than in late 1998. Descriptive statistics indeed suggest that, among the final sample included in the analysis, the monthly employment rate on average increases from 72% in the early of the two-year period to 84% in the middle of the period to 88% in the end of the period. The increase is monotonic in time within each of the two-year periods, consistent with the hypothesis that earlier jobs are more likely to be missed in the PSID.

2.3.3 Health Insurance Coverage Information in the PSID

The PSID also asks the question “how many months were you covered by health insurance in each of the past two years?” Consequently, the 1999 wave of the PSID has numbers of months covered in 1997 and 1998, the 2001 wave has 1999 and 2000, etc. The six waves of PSID can be used to construct a panel consisting number of months each year that each PSID household head is covered by health insurance between 1997 and 2008. This is the main health insurance coverage information used in this study. Unfortunately, the PSID does not have enough information on type of health insurance coverage that can be used in the twelve-year panel.[4] Based on the number of months covered, all the yearly records of the PSID male household heads can be split into three categories: always covered (having coverage for 12 months in a year), never covered (having health insurance coverage for zero month), and sometimes covered (neither always nor never covered). Table 2.2 shows the cross-sectional distribution of these three states of coverage in each of the twelve years.

Specific patterns in table 2.2 suggest some extent of heteroscedastic retrospective/measurement errors. Recall that the 1999 wave of PSID asks the number of months covered in 1997 and 1998, the 2001 wave asks about coverage in 1999 and 2000, etc. This means information about 1997 and 1999 coverage may have more retrospective error than information about 1998 and 2000, simply because 1997 and 1999 are further away from the time of survey than 1998 and 2000. Table 2.2 suggests people are more likely to answer "always covered" or "never covered" regarding the coverage in 1997, 1999, etc. than in 1998, 2000, etc. This should not be surprising, since people are less likely to recall the exact number of months covered by health insurance in

[4]The PSID does ask about the type of health insurance coverage at the time of survey. The types include: not insured, employer-sponsored, directly purchase from private insurance market, Medicare, Medi-Gap, Medicaid, Military, CHAMPUS (Civilian Health and Medical Program of the Uninformed Services)/TRICARE/CHAMP-VA (Civilian Health and Medical Program-Veteran's Administration), Indian Health Insurance, state-sponsored, and other government plans. However, type of plan is not surveyed in the retrospective question regarding the coverage in the past two years. It is still possible to, for example, obtain type of insurance in 1999 from the 1999 wave of data and number of months covered in 1999 from the 2001 wave. But such matching mechanism requires a strong assumption that one does not change plan types during the span, and the matching may still be ambiguous when more than one types of health insurance are provided. And hence, I do not use the information on plan types in the analysis.

earlier years and may just report either "never covered" or "always covered" instead, especially if the exact number of months is close to 0 or 12.

Column 1 in table 2.3 shows the relationship between individual characteristics and health insurance coverage. The left hand side variable is a dummy indicating whether an individual is always covered in a given year, and the coefficients in column 1 are from a random effect probit model.[5] All the coefficients have the expected signs. Older people, those who are married, and those who have kids are more likely to be covered than those who are younger or single with no kid. The likelihood of health insurance coverage is positively correlated with the level of education. Minorities are less likely to be covered. Those with job tenure more than 3 years are more likely to have health insurance coverage. However, those whose job tenure is less than 3 years, and especially those who just started a job, are less likely to be covered than those who are not working. This is consistent with the findings in Farber and Levy (2000) that people in new jobs (job tenure less than one year) are less likely to have health insurance coverage, and this might also reflect that some of those who are not working are covered by public health insurance plans. Finally, the higher household earnings is also associated with higher probability of health insurance coverage.

[5]Household earnings are compared to the Federal Poverty Level (calculated based on the family size) and split into four categories: less than the FPL, between 100% and 200% of the FPL, between 200% and 400% of the FPL, and above 400% of the FPL. Such categorization is better than strict income brackets or the logarithm of income because the eligibility for public health insurance programs is determined by the comparison of household income to the Federal Poverty Level. Using 100%, 200%, and 400% of the FPL as thresholds largely complies with the latest eligibility rules following the recent expansions in government-sponsored health insurance programs since 1990s (Cutler and Gruber 1996, Gruber and Simon 2008, LoSasso and Buchmueller 2004).

2.3.4 Dynamics of Health Insurance Coverage in the PSID

The dynamics of health insurance coverage in the PSID is evident and can be shown by estimating how people transit into and out of different states of coverage (always covered in a year, never covered in a year, or sometimes covered in a year).

First, I show how long people stay in the "always covered" status before they transit into "never covered" or "sometimes covered"; in other words, this is an estimate of how soon people drop out from being "always covered." Figure 2.1 shows the Kaplan-Meier survival curve of the "always covered" status, utilizing all observations between 1997 and 2008. Starting with 100% of all those who are always covered in a given year, a Kaplan-Meier survival curve depicts the proportion of those who are initially always covered and stay always covered in each year. It hence shows roughly 70% of those who are initially always covered are still always covered after ten years.[6]

Another way to approach this is to show how soon people can get out of "never covered" status and gain coverage. Figure 2.2 shows such survival curve, and around 20% among those who are initially "never covered" stay in the same status after 10 years. In other words, 80% of those who are "never covered" gain health insurance coverage at some point of time. Given that slightly less than 85% people are always covered and slightly more than

[6] It would be technically less sophisticated to just show the proportion of the sample that falls into each of the three categories based on simple descriptive statistics of the sample. However, a survival curve utilizes the information contributed by the censoring observations while the descriptive statistics do not. For example, if an individual is in the data for five years and is always covered but then disappears, a survival curve would only count this person to be always covered for only five years but censored, while the descriptive statistics would just count this person as "always covered". Even if all the censored (those who do not stay in the data for the full twelve years) are kept, a survival curve still is more informational. For those who are moving into and out of coverage, the survival curve would utilize each yearly spell of "always covered", "sometimes covered", or "never covered" in estimating the likelihood of dropping out of coverage, and the information available from descriptive statistics is very limited.

10% are never covered in each year according to table 2.2, figures 2.1 and 2.2 suggest that over any 10-year span, almost 60% (70% $\times$ 85%) of the sample is always covered and 2% is never covered (20% $\times$ 10%), implying roughly 40% transiting into and out of health insurance coverage.

The survival curves, nevertheless, are nonparametric and do not tell whether any of the individual or job characteristics increases or decreases the likelihoods of losing/ gaining coverage. Columns 2 and 3 in table 2.3 are the estimates from Cox proportional hazard models that show how individual/job characteristics change "the hazards" of losing or gaining health insurance coverage. The numbers in columns 2 and 3 are hazard rates, and a coefficient larger than one implies a greater likelihood of losing/gaining coverage compared to the base group while a coefficient less than one implies a less likelihood.

According to the estimates in column 2 of table 2.3, those who are older, married, and with kids are less likely to drop out from being "always covered" than those who are aged 22-29, single, and without kid. Less educated people are more likely to lose coverage. People with shorter job tenure are more likely to lose coverage– either they lose coverage because they start new jobs following unemployment or because those who are not working are covered by public health insurance plans. Less wealthy households also have greater hazards to loss coverage. These results are very similar to those reported in Fairlie and London (2009a, 2009b).

Column 3 of table 2.3 shows the hazards of gaining coverage. Older people are *less* likely than younger people to gain health insurance once they lose coverage. Married people are more likely to become covered. Conditional on being uninsured, the likelihood of gaining coverage is positively correlated with the level of education. People with short tenure are more likely to gain coverage than those who are not working, presumably through having an employer who offers health benefits or becoming eligible for those benefits. Higher household earnings are again associated with greater hazards to gain coverage. Although some of the coefficients are not significant in column 3 due to a smaller sample size (much less people are not covered to begin with), most of the coefficients still have expected signs.

Overall, an examination of a 12-year panel shows that the health insurance coverage does not stay static. Roughly 40% of the sample transits into and out of coverage, with the hazards of transit differ by individual characteristics. The main theme of this study is to show how much of the dynamics in health insurance coverage is caused by the dynamics in employment. As I argue in the previous sections, coverage and employment are closely correlated because health insurance coverage in the U.S. is a major component of employee benefits and the provision of health insurance may alter labor supply decisions. An evidence to the correlation is that, in my sample, only around 10% of those who are always covered throughout the twelve-year period experience any spell of unemployment, while more than two-thirds of those who transit into and out of coverage experience at least one spell of unemployment.[7] The causal relationship between unemployment and health insurance coverage is examined in the next section. I first present the estimates based on a model that does not control for the correlation between coverage and employment. I then show how the estimates change after such correlation and endogeneity are controlled.

[7]These numbers are calculated by the author.

2.4 Result

2.4.1 A Model without Controlling for the Correlation between Coverage and Unemployment

Table 2.4 tabulates the estimated relationship between unemployment and health insurance coverage based on equation (2.1) using random effect probit models. The same set of control variables as those in table 2.3 are included in all models but are not reported to save space. The coefficients on these control variables are qualitatively similar to those reported in column 1 of table 2.3.

Column 1 of table 2.4 shows the effect of unemployment or not working (for any duration or number of spells in a year) on health insurance coverage in the same year. The coefficient of -0.4711 corresponds to a marginal effect of -0.1456, that is, any spell of unemployment reduces the probability of covered for 12 months in the same year by 14.56%. A lagged term is added in column 2; coefficients of -0.4637 and -0.1157 correspond to marginal effects of -0.1093 and -0.0505, respectively. This implies the spells of unemployment are correlated over time, and the effect of unemployment in year t on the coverage in year t is reduced after unemployment in year $t-1$ is included in the regression. More lagged terms of unemployment are added in the following columns. The numbers suggest that the negative effect of unemployment on coverage lasts for up to three years (column 4), as the coefficients on longer lagged terms in columns 5 through 7 are mostly insignificant. The total combined marginal effects of all lagged terms in column 4 is -0.1625.

2.4.2 The Bivariate Probit Model

Estimation results of equations (2.2) and (2.3), based on bivariate probit models that control for the endogeneity between coverage and unemployment as well as individual heterogeneity, are shown in table 2.5. There are four columns in each panel, and the first columns in panels A and B are from the same set of bivariate probit regression, the second columns in both panel from the same set of regression, etc. Each of panel A and B has its own set of control variables: the control variables in panel A are the same as those in table 2.4, and panel B has the same control variables except job tenure and household earnings. Coefficients on these control variables are not reported to save space. Besides, in order to separate the change in the estimates due to the change in the model (number of lag terms included) and the change in the sample (those who do not stay in the panel long enough are dropped when more lags are included), only the individuals who have more than five observations are used to estimate equations (2.2) and (2.3). The changes in coefficients on the same variable across columns in table 2.5 hence reflect only the change in specifications.

The estimates in table 2.5 show that error terms in the two bivariate probit regression equations are negatively correlated (ρ ranges from -0.1603 to -0.0951), justifying the use of bivariate rather than univariate probit models. Also, the effect of unemployment on health insurance coverage only lasts for one year, as the top half of panel A suggests only coefficients on unemployment in years t and $t-1$ are significant. This indicates that the effect does not last as long as the estimates based on univariate models shown in table 2.4. The interpretation of the coefficients in table 2.5, however, is less straightforward than those in table 2.4. The nature of the bivariate probit setting in equations (2.2) and (2.3) means that, for example, the effect of unemployment on health insurance coverage depends on how unemployment affects the probability of both "covered and unemployed" and "covered and employed". That is, given

$$Pr(HI_t = 1) = Pr(HI = 1, U_t = 1) + Pr(HI_t = 1, U_t = 0), \qquad (2.4)$$

the effect of unemployment at any period on health insurance coverage at the current period, $\frac{\partial Pr(HI_t=1)}{\partial U_{t-k}}$, can be written as

$$\frac{\partial Pr(HI_t = 1)}{\partial U_{t-k}} = \frac{\partial Pr(HI = 1, U_t = 1)}{\partial U_{t-k}} + \frac{\partial Pr(HI_t = 1, U_t = 0)}{\partial U_{t-k}}, \qquad (2.5)$$

where unemployment at k period(s) earlier is denoted as U_{t-k}. Each marginal probability in equation (2.5) depends on the the coefficients in equations (2.2) and (2.3) as well as the correlation between the two errors terms, ρ. The calculated marginal probabilities based on the coefficients in table 2.5 are listed in table 2.6.

Panel A in table 2.6, which contains numbers calculated based on the column 1 in table 2.5, shows how the experience of unemployment influences the joint likelihoods of health insurance coverage and unemployment. That is, for example, the numbers in the column labeled "$P(HI_t = 1, U_t = 1)$" are the marginal changes in probability of being covered and unemployed in year t caused by the spell of unemployment and health insurance coverage at various points of time. In panel A of table 2.6, any spell of unemployment in year t reduces the probability of both covered ($HI_t = 1$) and unemployed ($U_t = 1$) by 0.37 percentage point, evaluated while the values of other variables are held at the mean. This number is calculated based on the first coefficient in the first column of panel A in table 2.5 (-0.1315), the first coefficient in the first column of panel B in the same table (1.4914), and the correlations of the error terms in equations (2.2) and (2.3) that is reported at the bottom of table 2.5 (-0.1603). The same column in panel A of table 2.6 also shows unemployment in year t reduces the likelihood of covered ($HI_t = 1$) and employed ($U_t = 0$) by 1.28 percentage point. To show the relative magnitudes of these effect sizes, the sample mean proportions of the four regimes jointly determined by health insurance coverage and unemployment are listed at the top of the panel. Overall, unemployment in year t reduces the probability of being covered by 1.65 percentage point, or 1.75% of the 94.05% who are covered by health insurance.

On the other hand, the net effect of unemployment in year $t-1$ on coverage is positive, at 1.79 percentage point. For those who experienced unemployment spells in year $t-1$, the likelihood of health insurance coverage decreases by 43.45 percentage points if he is always working in year t but increases by 45.24 percentage points if he is not always working. This result is consistent with previous study that the availability of continuation coverage or retiree health insurance coverage makes it affordable to stay unemployed or out of labor force for an extended period of time or to retire before being eligible for Medicare (Gruber and Madrian 1995, Gruber and Madrian 1996, Gruber and Madrian 1997, Gustman and Steinmeier 1994, Karoly and Rogowski 1994, Madrian 1994a). And hence, for those who have unemployment spells for two years in a row, the combined effect of unemployment on coverage is positive but very small at 0.14 percentage point based on panel A.

Panels B through D in table 2.6 show the short-term effect of unemployment on coverage becomes larger when more lagged terms are included. The effect of unemployment in year t on coverage in the same year is -1.8 percentage point (-0.0040-0.0140) in panel B, -3.04 percentage points in panel

C, and -3.86 percentage points in panel D. On the other hand, the unemployment in year $t-1$ increases the likelihood of coverage by 1.66 percentage in panel B, 2.04 percentage points in panel C, and 2 percentage points in panel D. If an individual has unemployment spells for two consecutive years, the cumulative effect of unemployment on coverage is -0.14 (-1.8+1.66) percentage point in panel B, -1 percentage point in panel C, and -1.86 percentage point in panel D. To the extent that including more lags leads to more precise estimates, the effect of long-term unemployment on coverage is slightly negative.[8] Overall, after the endogeneity between coverage and employment is controlled for, estimates in tables 2.5 and 2.6 suggest that unemployment has a smaller negative effect on the likelihood of health insurance coverage– both in terms of magnitude and duration.

The numbers in table 2.6 have some caveats. First, due to the limitation in information on job search behaviors available in the PSID, the states of "unemployment" and "not in labor force" can not be distinguished. Distinguishing these two states in each month of the job history would require the information on whether an individual has an active job search in each month, which is not available in the PSID. Consequently, an individual would be counted as "unemployed" in a given month if he is not recorded as "working" in that month, regardless whether he is unemployed, not in the labor force, or working for a job not recorded in the PSID. Second, while the PSID allows the reconstruction of monthly job history, it does not provide enough information for a monthly health insurance coverage history. It only has information on the number of months in each of the previous two years that an individual has health insurance coverage. Such limitation constraints my

[8]The marginal probabilities of terms on $t-2$, $t-3$, and $t-4$ are not used because their effects are not statistically significant according to table 2.5.

analysis at yearly rather than monthly level. It also prevents a direct assessment of the relationship between the durations of unemployment and uninsured spells.

2.5 Discussion and Conclusion

In spite of its important policy implications, the dynamics of health insurance coverage and how such dynamics is influenced by employment is not widely studied. This study examines the causal effect of unemployment on health insurance coverage using male household heads aged 22-64 from six waves of Panel Study of Income Dynamics (PSID) that lead to a twelve-year panel data of health insurance coverage and employment history.

The dynamics of health insurance coverage is evident. The nonparametric survival analysis suggests that almost 60% of the sample always stays covered during the twelve-year span, while roughly 2% is never covered. This implies around 40% of the sample moves into and out of health insurance coverage. Descriptive statistics show that those who move into and out of coverage experience more unemployment than the group that is always insured: more than two-thirds of the former group experiences at least one spell of unemployment during the twelve years, but only about 10% of the later group experiences any unemployment. This result means a great deal of dynamics in health insurance coverage is correlated with employment dynamics.

Distinguishing the causal relationship between health insurance coverage and employment, however, is empirically challenging. Health insurance coverage is one of the major component in compensation in the U.S. labor market, and the provision (or the lack) of health insurance has been shown to influence job choice and labor supply decisions. Such endogeneity between the two has to be accounted for in order to obtain a precise estimate of the causal relationship. The estimation is further complexed by the existence of individual heterogeneity that influences both health insurance and coverage. The panel structure of the PSID allows an empirical strategy that yields an unbiased estimate of how unemployment affects health insurance coverage.

A model that does not account for the endogeneity between coverage and employment suggests that the negative effect of unemployment on the likelihood of health insurance coverage lasts for up to three years, with a combined marginal effect of 16%. However, the bivariate probit model that accounts for the correlation structure suggests a smaller effect. When the state dependency of unemployment as well as the endogeneity between health insurance coverage and employment is included in the model, the long term cumulative effect of unemployment on health insurance coverage becomes much smaller: unemployment spells in two consecutive years reduces the likelihood of coverage by less than two percentage points. The comparison of the two sets of results underscore the importance to take the endogeneity into consideration in order to assess the causal relationship between unemployment and health insurance coverage.

This study nevertheless has its own limitations. First, the PSID only records up to four jobs in each wave, and an individual might sometimes be mis-classified as unemployed in the monthly employment history if he works for more than four jobs during the two year that each wave of the PSID covers. Second, the information on job searching behaviors available in the PSID is very limited, which makes it challenging to distinguish between unemployment and not in the labor force. And hence, I am not able to estimate specifically whether people would move form "not in the labor force" to employment in order to obtain health insurance coverage. Third, health insurance plans are all but homogeneous. Plans differ in generosity, employee contribution, and out-of-pocket expenses. Treating health insurance coverage as a dichotomy probably would lead to a simplified view of how health insurance coverage and employment interact with each other. However, with the information available in the PSID, this study still demonstrates a reasonable solution to an important yet empirically challenging research question.

Figure 2.1: Kaplan-Meier Survival Curve of Staying Always Covered

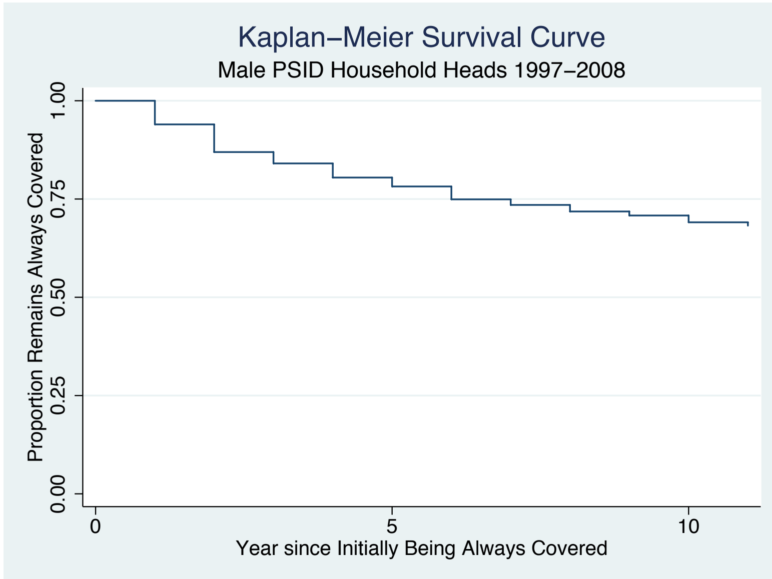

Figure 2.2: Kaplan-Meier Survival Curve of Staying Never Covered

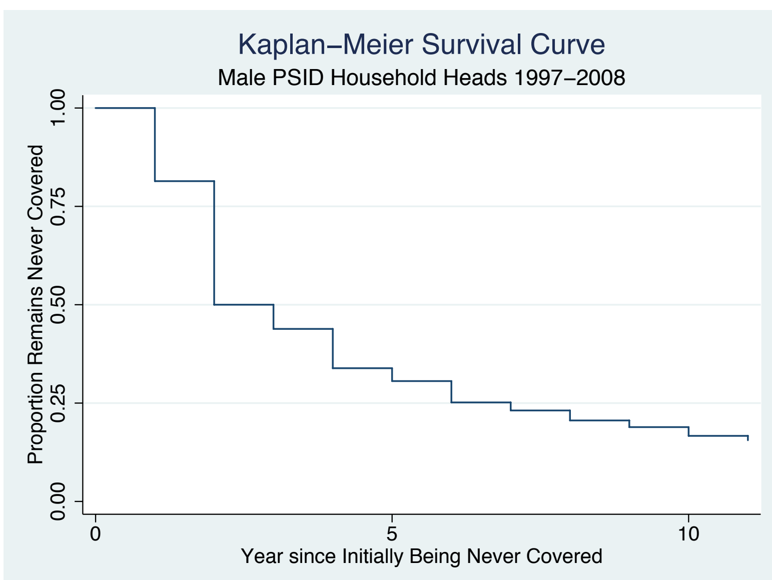

Table 2.1: Descriptive Statistics, Male PSID Household Heads, 1999-2009

PSID Wave	1999	2001	2003	2005	2007	2009
Age	42.75	43.28	43.54	44.14	44.40	44.58
	(10.87)	(10.94)	(11.08)	(11.45)	(11.69)	(11.94)
Years of Education	13.64	13.68	13.72	13.70	13.72	13.71
	(2.28)	(2.21)	(2.21)	(2.18)	(2.17)	(2.14)
Household Earnings in 2010 U.S. Dollars						
1^{st} Quartile	\$19,630	\$21,000	\$19,000	\$20,000	\$21,200	\$22,320
2^{nd} Quartile	\$35,000	\$37,500	\$36,500	\$39,900	\$40,600	\$43,711
3^{rd} Quartile	\$54,300	\$60,000	\$60,000	\$65,000	\$70,000	\$75,000
Number of People	2,966	3,104	3,220	3,287	3,394	3,491

Weighted using the PSID cross sectional sample weight.
Standard deviations in parentheses.

Table 2.2: Cross Sectional Health Insurance Coverage

Year	Always Covered	Never Covered	Sometimes Covered	Number of People
1997	85.91%	11.10%	2.99%	2,947
1998	86.32%	9.97%	3.71%	2,967
1999	86.67%	10.40%	2.93%	3,086
2000	86.07%	9.80%	4.13%	3,102
2001	86.36%	10.06%	3.58%	3,219
2002	85.39%	10.25%	4.36%	3,223
2003	82.40%	14.10%	3.50%	3,267
2004	81.24%	13.43%	5.33%	3,275
2005	83.23%	14.01%	2.76%	3,392
2006	81.48%	12.82%	5.70%	3,404
2007	82.22%	13.90%	3.88%	3,507
2008	81.39%	13.72%	4.89%	3,504

Weighted using the PSID cross sectional sample weight.

Table 2.3: Likelihoods of Being Covered, Dropping out of Coverage, and Gaining Coverage

	(1) Being Covered	**(2) Losing Coverage**	**(3) Gaining Coverage**
Age 30-39	0.3441**	0.6021**	0.8809
	(0.0431)	(0.0530)	(0.0826)
Age 40-49	0.6733**	0.4498**	0.7454**
	(0.0568)	(0.0434)	(0.0799)
Age 50-59	0.8320**	0.4008**	0.7765
	(0.0670)	(0.0434)	(0.1007)
Age 60-64	1.1987**	0.2313**	0.7036
	(0.0948)	(0.0412)	(0.1619)
Married	0.6745**	0.3391**	1.5672**
	(0.0418)	(0.0248)	(0.1377)
Having 1 Kid	0.1265**	0.7999**	1.0553
	(0.0456)	(0.0769)	(0.1110)
Having 2 Kids	0.2272**	0.7379**	1.0546
	(0.0523)	(0.0738)	(0.1194)
Having 3+ Kids	0.1876**	0.9410	1.1087
	(0.0675)	(0.1103)	(0.1381)
High School Dropout	-0.8582**	1.4861**	0.6834**
	(0.0988)	(0.1522)	(0.0745)
Some College	0.4107**	0.8081**	1.3200**
	(0.0732)	(0.0623)	(0.1160)
College	1.1640**	0.5464**	1.8171**
	(0.0909)	(0.0543)	(0.2502)
More than College	1.2941**	0.4587**	2.1848**
	(0.1231)	(0.0649)	(0.4203)
Black	-0.3778**	1.1507	1.0461
	(0.1262)	(0.1454)	(0.1426)
Hispanic	-0.6420	1.3035	0.4511
	(0.4294)	(0.4972)	(0.2628)
Other Race	-0.4808**	1.1909	1.0491
	(0.0834)	(0.1159)	(0.1110)
Tenure 0 to 1 Year	-0.5464**	2.4935**	1.4296**
	(0.0386)	(0.2054)	(0.1398)
Tenure 1 to 3 Years	-0.1148**	1.5257**	1.3201**
	(0.0386)	(0.1442)	(0.1333)
Tenure 3 to 5 Years	0.3248**	0.6028**	1.1131
	(0.0540)	(0.0924)	(0.1637)
Tenure 5+ Years	0.4818**	0.4020**	0.7167**
	(0.0409)	(0.0423)	(0.0890)
Below FPL	-0.3321**	1.0569	0.8158**
	(0.0401)	(0.0927)	(0.0759)
200%-400% FPL	0.3256**	0.5821**	1.2122**
	(0.0376)	(0.0484)	(0.1158)
Above 400% FPL	0.5584**	0.3758**	1.4021**
	(0.0544)	(0.0417)	(0.2021)
Number of Observations	38,893	26,588	3,698
Number of People	4,737	–	–
χ^2	2,037.17**	1,376.92**	186.53**

Coefficients in column 1 are the marginal effects from a random-effect Probit model.
Coefficients in columns 2 and 3 are hazard ratios.
Standard errors in parentheses.
**: $p < 0.05$

Table 2.4: Unemployment and Health Insurance Coverage, Random Effect Probit Models

Dependent Variable: Having Health Insurance Coverage for 12 Months in Year t							
Unemployment in Year:							
t	-0.4711**	-0.4637**	-0.5330**	-0.5558**	-0.6317**	-0.6256**	-0.7065**
	(0.0325)	(0.0366)	(0.0418)	(0.0472)	(0.0548)	(0.0623)	(0.0742)
$t-1$		-0.1157**	-0.1221**	-0.1047**	-0.1409**	-0.1120**	-0.1219
		(0.0340)	(0.0396)	(0.0444)	(0.0515)	(0.0578)	(0.0686)
$t-2$			-0.0978**	-0.9993**	-0.1157**	-0.1218**	-0.1219
			(0.0385)	(0.0442)	(0.0516)	(0.0582)	(0.0692)
$t-3$				-0.1024**	-0.1224**	-0.2060**	-0.2688**
				(0.0430)	(0.0510)	(0.0572)	(0.0685)
$t-4$					-0.0279	-0.0126	0.0042
					(0.0512)	(0.0592)	(0.0713)
$t-5$						-0.1448**	-0.0852
						(0.0566)	(0.0697)
$t-6$							-0.0765
							(0.0697)
Number of Observations	38,893	33,962	29,117	24,970	20,902	17,471	14,116
Number of People	4,737	4,658	4,094	4,016	3,431	3,355	2,825

Control variables included in the regression: age (in categories of 22-29, 30-39, 40-49, 50-59, and 60-64), highest level of education (high school dropout, high school, some college, college, or post-secondary), race (white, black, Hispanic, or other), marital status (married or not), number of kids younger than 18 in the household (in categories of 0, 1, 2, and 3 or more), job tenure (not working, less than 1 year, 1 to 3 years, 3 to 5 years, and 5 years or above) and household income in year t (below the Federal Poverty Level, 100%-200% of the FPL, 200%-400% of the FPL, or above 400% of the FPL).

Coefficients are the estimates from random effect Probit models.

Clustered standard errors in parenthesis.

**: $p < 0.05$

Table 2.5: Unemployment and Health Insurance Coverage, Bivariate Probit Models

Panel A. Dependent Variable: Covered for 12 Months in Year t				
Unemployment in Year:				
t	-0.1315**	-0.1460**	-0.2461**	-0.3057**
	(0.0537)	(0.0573)	(0.0647)	(0.0702)
$t-1$	0.1619**	0.1541**	0.2030**	0.2014**
	(0.0444)	(0.0460)	(0.0523)	(0.0556)
$t-2$		0.0340	0.0001	-0.0100
		(0.0396)	(0.0478)	(0.0507)
$t-3$			0.0447	0.0212
			(0.0403)	(0.0440)
$t-4$				0.0461
				(0.0440)
Health Insurance Coverage in Year:				
$t-1$	2.2129**	1.9848**	2.0769**	1.9485**
	(0.0634)	(0.0405)	(0.0485)	(0.0494)
$t-2$		0.3253**	0.0097	0.0669
		(0.0451)	(0.0666)	(0.0650)
$t-3$			0.5006**	0.2837**
			(0.0484)	(0.0561)
$t-4$				0.3602**
				(0.0504)
Panel B. Dependent Variable: Any Spell of Unemployment in Year t				
Unemployment in Year:				
$t-1$	1.4914**	1.2721**	1.3451**	1.3144**
	(0.0268)	(0.0268)	(0.0280)	(0.0305)
$t-2$		0.4079**	0.3438**	0.2909**
		(0.0296)	(0.0371)	(0.0362)
$t-3$			0.1250**	0.0443
			(0.0316)	(0.0352)
$t-4$				0.4062**
				(0.0316)
Health Insurance Coverage in Year:				
$t-1$	-0.1740**	-0.1450**	-0.1243**	-0.1072**
	(0.0280)	(0.0383)	(0.0409)	(0.0464)
$t-2$		-0.0093	0.0641	0.0447
		(0.0388)	(0.0523)	(0.0564)
$t-3$			-0.0989**	-0.0863
			(0.0446)	(0.0558)
$t-4$				0.0690
				(0.0461)
ρ	-0.1603	-0.1490	-0.1206	-0.0951
Number of Observations	30,327	27,070	23,813	20,556
Number of People	3,257	3,257	3,257	3,257

Coefficients are the estimates from bivariate Probit models.
Coefficients on the controlled variables are not listed to save space.
Clustered standard errors in parenthesis.
**: $p < 0.05$

Table 2.6: Corresponding Marginal Probabilities of the Bivariate Probit Models Estimates

Panel A: Corresponding Marginal Probabilities of Coefficients in Column 1 in Table 2.5				
	$P(HI_t = 1, U_t = 1)$	$P(HI_t = 1, U_t = 0)$	$P(HI_t = 0, U_t = 1)$	$P(HI_t = 0, U_t = 0)$
Sample Mean	0.1462	0.7940	0.0147	0.0449
Unemployment in Year:				
t	-0.0037	-0.0128	0.0037	0.0128
$t-1$	0.4524	-0.4345	0.0222	-0.0401
Health Insurance Coverage in Year:				
$t-1$	0.0899	0.5109	-0.1350	-0.4658
Panel B: Corresponding Marginal Probabilities of Coefficients in Column 2 in Table 2.5				
	$P(HI_t = 1, U_t = 1)$	$P(HI_t = 1, U_t = 0)$	$P(HI_t = 0, U_t = 1)$	$P(HI_t = 0, U_t = 0)$
Sample Mean	0.1485	0.7935	0.0141	0.0437
Unemployment in Year:				
t	-0.0040	-0.0140	0.0040	0.0140
$t-1$	0.3855	-0.3689	0.0181	-0.0348
$t-2$	0.1045	-0.1007	0.0068	-0.0107
Health Insurance Coverage in Year:				
$t-1$	0.0784	0.4418	-0.1159	-0.4043
$t-2$	0.0078	0.0372	-0.0101	-0.0349
Panel C: Corresponding Marginal Probabilities of Coefficients in Column 3 in Table 2.5				
	$P(HI_t = 1, U_t = 1)$	$P(HI_t = 1, U_t = 0)$	$P(HI_t = 0, U_t = 1)$	$P(HI_t = 0, U_t = 0)$
Sample Mean	0.1415	0.8040	0.0118	0.0426
Unemployment in Year:				
t	-0.0060	-0.0244	0.0060	0.0244
$t-1$	0.4051	-0.3847	0.0156	-0.0360
$t-2$	0.0838	-0.0838	0.0059	-0.0059
$t-3$	0.0296	-0.0248	0.0009	-0.0058
Health Insurance Coverage in Year:				
$t-1$	0.0785	0.4668	-0.1093	-0.4360
$t-2$	0.0140	-0.0129	0.0008	-0.0018
$t-3$	-0.0068	0.7989	-0.0174	-0.0556
Panel D: Corresponding Marginal Probabilities of Coefficients in Column 4 in Table 2.5				
	$P(HI_t = 1, U_t = 1)$	$P(HI_t = 1, U_t = 0)$	$P(HI_t = 0, U_t = 1)$	$P(HI_t = 0, U_t = 0)$
Sample Mean	0.1505	0.7955	0.0114	0.0424
Unemployment in Year:				
t	-0.0076	-0.0310	0.0076	0.0310
$t-1$	0.4050	-0.3850	0.0149	-0.0349
$t-2$	0.0722	-0.0733	0.0051	-0.0040
$t-3$	0.0108	-0.0085	0.0002	-0.0025
$t-4$	0.1058	-0.1008	0.0055	-0.0105
Health Insurance Coverage in Year:				
$t-1$	0.0749	0.4259	-0.1022	-0.3985
$t-2$	0.0115	-0.0039	-0.0007	-0.0068
$t-3$	-0.1250	0.0490	-0.0093	-0.0272
$t-4$	0.0243	0.0240	-0.0078	-0.0405

Marginal effects of the control variables are not listed.

CHAPTER 3

WHAT DRIVES THE RACIAL GAP IN EMPLOYER-SPONSORED HEALTH INSURANCE COVERAGE?

3.1 Introduction

The racial disparity in social-economic status and economic outcomes has long commanded the attention of social scientists in the United States. One of the important topics is the racial wage gap, and researchers have consistently concluded that the black-white wage gap is still around 10% even when all the information observable in the data are controlled for (Altonji and Blank 1999, Chay and Lee 2000). On the other hand, despite the fact that most of the working-age people obtain health insurance coverage through their employers and health insurance has become one of the most valuable component of employee benefits,[1] the racial health insurance coverage gap is not as widely studied as the racial wage gap (Altonji and Blank 1999, Dushi and Honig 2005, Fairlie and London 2009a, Reschovsky, Hadley and Nichols 2007, Waidmann, Garrett and Hadley 2004). In this study, I estimate the racial gap in employer-sponsored health insurance coverage, address the causes of the gap, assess how the increases in health insurance premium influence the gap, and propose how my findings set up the stage for further organizational level studies.

In the past decade, there has been some progress in the study of racial

[1] In December 2010, health insurance premium values at 8.4% of the total compensation cost according to the "Employer Costs for Employee Compensation" report released by the Bureau of Labor Statistics.

health insurance coverage gap, especially in the economic literature. Several studies were sponsored by the Economic Research Initiate on Uninsured (ERIU) at the University of Michigan. Crow, Harrington, and McLaughlin (2002) document black-white and Hispanic-white health insurance coverage gaps of 8% and 13%, respectively, based on the 2001 Current Population Survey (CPS). They argue that such gap may be the result of racial disparities along other dimensions such as education attainment, earnings, public insurance coverage, employment sector, and labor force participation. After these variables are controlled for, most studies show that the racial gap shrinks by 50% to 65% but remains significant. Overall, earnings difference is a major determinant of the black-white gap, while nativity and education are important components in explaining the Hispanic-white gap (Crow, Harrington and McLaughlin 2002). There are also some studies that specifically address the health insurance coverage of the Hispanics. For example, nation of origin and English proficiency influence the likelihood of health insurance coverage for non-citizen Hispanics (Reschovsky et al. 2007, Rutledge and McLaughlin 2008). Although Monheit and Vistnes (2000) show that 40% of the 13 percentage points drop in health insurance coverage between 1987 and 1996 for Hispanic males is attributed to changes in worker demography and 60% is attributed to other factors, a large portion of the change in Hispanic-white health insurance coverage gap remains unexplained (Rutledge and McLaughlin 2008).

Given that most of the working-age population rely on employer-sponsored plans as the source of health insurance coverage in the U.S. and the racial gap exists for very different reasons in public and private insurance coverage, the racial gap in employer-sponsored health insurance coverage deserves to be examined separately. Studies show that whites have more access to employer-sponsored coverage than the Hispanics, but the black-white disparity is much smaller (Crow et al. 2002, Dushi and Honig 2005, Fronstin 2007, Quinn 2000, Reschovsky et al. 2007, Rutledge and McLaughlin 2008, Waidmann et al. 2004). When the employer-sponsored health insurance coverage is further decomposed to employer offering, employee eligibility, and employee take-ups, there is less consensus regarding the racial gap in these three components. Relying on the 1996 panel of the Survey of Income and Program Participation (SIPP), Dushi and Honig (2005) find significant minority-white gap in employer offering but not employee enrollment. Reschovsky and his colleagues (2007), on the other hand, find significant Hispanic-white gap in both offering and enrollment using the Community Tracking Study (CTS) Household Survey. They argue that Hispanics may have different demand for health insurance when they face price increases, which partially explains the Hispanic-white coverage gap. The review by Crow, Harrington, and McLaughlin (2002) also suggests findings in the racial gap in employer offering and employee enrollment of health insurance coverage are somewhat inconsistent, depending on the data used– some find the black-white gap in offering to be significant but some do not, some find the racial gap in employee enrollment to be substantial but some conclude the gap is rather small.

From a dynamic perspective, it is not well studied why the racial health insurance coverage gap changed over time. The literature suggests that a good portion of changes in racial gap remains unexplained (Monheit and Vistnes 2000, Reschovsky et al. 2007). Although Fairlie and London (2009a, 2009b) conclude that blacks and Hispanics are less likely to earn and more likely to lose employer-sponsored health insurance coverage using data from 1996 to 2004 waves of the CPS, the fact that racial health insurance coverage gap has been fluctuating implies a more complicated causal model to explain the racial gap in addition to the racial disparity in transition into and out of coverage. Figure 3.1 shows the time trends of racial gaps as well as overall employer-sponsored health insurance coverage between 1993 and 2009 based on the March CPS. The share of the population with health insurance coverage is shown on the left vertical axis of the figure, and both the black-white and Hispanic-white gaps in coverage are shown in the right vertical axis. The axes cover the same range despite the magnitudes of the numbers are different, so the changes in each trend represent the same level of change in coverage or racial gaps. Had the racial disparity in transition into and out of insurance been the only reason that explains the racial gap, the black-white and Hispanic-white gap would not have narrowed at all. More interestingly, the time trends seem to suggest that the black-white gap decreases when overall health insurance coverage increases and vice versa, and the Hispanic-white gap follows a similar pattern although to a lesser extent.

Another perspective of health insurance coverage dynamics is how the market factors, namely the recent hike in health insurance premiums, affects the coverage. Economic theories that predict the relationship between premium cost and coverage differ on how the "price" of health insurance is defined. One theory (Chernew, Cutler and Keenan 2005) defines the full amount of premium as the price of health insurance coverage. If the premium cost goes up because the "value" (including the cost of health care services without coverage, the quality of care, and the generosity of coverage) goes up, the quantity of demand for health insurance would increase. However, if the premium cost goes up because of a higher level of moral hazard or adverse selection, the quantity of demand would go down. The overall impact of premium cost increase on coverage is hence theoretically ambiguous, and this is exacerbated by the lack of a reliable measure on the level of moral hazard (Chernew et al. 2005). The other theory, which is more dominant in health economics textbooks, asserts that the health insurance premium consists two components: the expected payoff of medical care benefits and the "load"– such as the administrative cost, insurance company's profit, and the risk premium of the insurance company (Chernew and Hirth 2004, Feldstein 1999, Kronick and Gilmer 1999, Phelps 2003). From this perspective, the "price" of health insurance is essentially the load, which can be seen as the risk premium that a consumer is willing to pay to insure against uncertain medical care expenditures. Price theory would suggest the quantity of demand for health insurance to decrease when the load goes up, everything else being equal. On the other hand, the health care expenditure hike in the past two decades not only shifts the mean of the expenditure distribution but also skews the whole distribution further to the right (French and Jones 2004). Such change in the expenditure would on average increase the expected level of benefit from health insurance (which increases health premium cost but does not change the demand for health insurance because the expected payoff still equals the premium cost) as well as the risk premium that people are willing to pay (which increases the demand for health insurance). Although the overall effect in this framework still is not clear, proxies that are correlated with different components of the insurance premium can be used to separately identify change in demand for health insurance coverage caused by increase in different component of the premium cost. Besides, note that both theories implicitly assume that health insurance coverage is affordable and people can decide whether they want to purchase the coverage or not. If the premium cost becomes too high, then the theoretical implication is conclusive: people would drop out of coverage because they cannot afford the premium. Given that the average health insurance premium outgrows wages by almost 10% each year in the past decade, the affordability is becoming an important determinant of health insurance coverage. Finally, regardless of the theory used, Reschovsky and his colleagues (2007) do find Hispanics may have different demand for health insurance, suggesting some extent of racial disparity in the effect of premium increase on coverage.

Despite the progress in the literature, three related issues remain unresolved. First, while the racial gap remains significant in employer-sponsored health insurance plans, whether the gap concentrates in employer offering, employee take-up, or both is not conclusive based on the previous literature. Second, although some studies use the Oaxaca-Blinder decomposition (Blinder 1973, Oaxaca 1973) to address how the discrepancy in the distributions of individual characteristics across racial groups affect health insurance coverage, we still do not know whether the compositional effect (the gap attributed to the discrepancy in the distribution of individual characteristics, in other words, the "explained" racial gap) is consistent across both employer offering and employee enrollment decisions and how it changes over time. Third, although the effect of rising health insurance premium cost in the past two decades on overall coverage may be theoretically ambiguous, we do not know whether the rising price impacts all racial groups equally and whether the racial gap is enlarged or shrunk due to recent hikes in the health insurance premium.

Using the Contingent and Alternative Employment Arrangement Supplements (February 1995, 1997, 1999, 2001, and 2005) to the Current Population Survey, I find that the trend in the employer-sponsored health insurance coverage is dominated by the changes in employer offering, as eligibility remains somewhat constant and employee take-up has declined since 1995. A sizable Hispanic-white gap also presents in employer offering of health insurance; however, conditional on offering, there are no racial gap in eligibility and take-up. A non-linear variance decomposition shows that around 20% of the racial coverage or offering gap can be explained by the discrepancy in education across racial groups, and roughly another one-third of the gap can be attributed to difference in job characteristics including job type (full- versus part-time and whether the tenure is longer than one year), industry, and occupation. Finally, depending on the proxy used for health insurance premium cost, the recent cost has no or small negative effects on insurance coverage; more specifically, the employer offering somewhat increases and employee take-up significantly decreases following the price increase. Nonetheless, Hispanics face a lower rate of employer offering when the health insurance premium rises.

The rest of the article is structured as the following. The framework of the study, including the related literature and the econometric skills used, is introduced in next section. The data used in this study, the Contingent and Alternative Employment Arrangement Supplements to the Current Population Survey as well as various proxies for health insurance premium cost, are introduced in section 3. The exclusive restrictions and sample construction process are also mentioned. Section 4 presents the results, including the racial gap in employer-sponsored health insurance coverage, the decomposition of the racial gap, and the effect of recent health insurance premium hike on coverage and the racial gap. The final section concludes and proposes further research directions.

3.2 Framework

This section discusses the analytic framework and econometric methods regarding how the employer-sponsored health insurance coverage and the racial gap can be decomposed as well as how other factors may affect the level of coverage. First, it is straightforward that coverage can be written as the products of a sequence of conditional probabilities: employer offering, employee eligibility, and employee take-up. I then introduce a decomposition method proposed by Fairlie (2005), which is an extension of the Oaxaca-Blinder decomposition (Blinder 1973, Oaxaca 1973) when the dependent variable is binary. Finally, following the recent literature in health economics regarding the decline of employer-sponsored health benefits (Cooper and Schone 1997, Farber and Levy 2000, Gruber and Washington 2005, Hadley and Reschovsky 2002, Nichols, Blumberg, Cooper and Vistnes 2001), I estimate how the increasing health care cost changed the trend of insurance coverage and the racial gap in the past decade.

3.2.1 Components of Employer-Sponsored Health Insurance Coverage

To be covered under employer-sponsored health insurance plans, an employee needs to (1) have an employer who provides a health insurance coverage plan, (2) be eligible for the health insurance benefit plan provided by the employer, and (3) enroll in the plan. Following Farber and Levy (2000), employer-sponsored health insurance coverage for group i can be decomposed as:

$$P(C)_i = P(O)_i \cdot P(E|O = 1)_i \cdot P(T|E = 1)_i, \tag{3.1}$$

where $P(\cdot)$ denotes probability and C, O, E, and T in the equation stand for coverage, offering, eligibility, and take-up, respectively.

Additionally, the difference in coverage between groups i and j can be written as

$$\begin{aligned}
P(C)_j - P(C)_i &= P(O)_j \cdot P(E|O=1)_j \cdot P(T|E=1)_j - \\
&\quad P(O)_i \cdot P(E|O=1)_i \cdot P(T|E=1)_i \\
&= P(O)_i(1+\Delta O) \cdot P(E|O=1)_i(1+\Delta E) \cdot P(T|E=1)_i(1+\Delta T) - \\
&\quad P(O)_i \cdot P(E|O=1)_i \cdot P(T|E=1)_i \\
&= P(O)_i \cdot P(E|O=1)_i \cdot P(T|E=1)_i \cdot \\
&\quad \Big[(1+\Delta O)(1+\Delta E)(1+\Delta T) - 1\Big]
\end{aligned} \tag{3.2}$$

in which ΔO, ΔE, and ΔT represent the difference in offering, eligibility, and take-up between groups i and j, where groups i and j can be different racial groups at the same time, the same racial group at different time, or a mix of both. Rewrite $P(C)_j$ as $P(C)_i(1 + \Delta C)$ and divide both sides of equation (3.2) by $P(C)_i$, which is also $P(O)_i \cdot P(E|O = 1)_i \cdot P(T|E = 1)_i$, the change in coverage can be expressed as:

$$\Delta C = (1 + \Delta O)(1 + \Delta E)(1 + \Delta T) - 1, \tag{3.3}$$

which simply means the change in coverage can be decomposed into the contributions of changes in offering, eligibility, and take-up. And hence, equation (3.3) can be used to estimate how the changes in offering, eligibility, and take-up influence the difference in overall coverage between two racial groups at the same time, for the same racial group over time, or between racial groups over time.

Since the take-up of employer-sponsored health insurance is conditional on being eligible, which in turn is conditional on being offered, estimating separate models for offering, eligibility, and take-up will lead to biased estimates of standard errors. A sequential Logit model is therefore used, and coefficients on the year of observation dummy and the interaction between year of observation and racial group dummies can be used to assess the significance of changes in offering, eligibility, and take-up of both within and between ethnic groups over time. I use the Stata module `SEQLOGIT` (Buis 2010) to estimate sequential Logit models. Note that it is also possible that the error terms in these three sequential regressions are endogenous and correlated across stages. To model the error structure, however, requires a fairly strong parametric assumption about the covariance matrix of the error terms, such as a multi-variate normal distribution (Ferguson 2008, Upchurch, Lillard and Panis 2002). Such model then can be fitted into a multivariate probit regression with simulated maximum likelihood estimation (Cappellari and Jenkins 2003, Cappellari and Jenkins 2006). The null hypothesis that error terms are independent with each other is indeed rejected using the February CPS, but the multivariate model does not yield qualitatively different estimates on the coefficients of interest compared to the sequential Logit models. In the result section, I rely on the estimates obtained in the sequential Logit models, which require fewer parametric assumptions.

However, equations (3.1) and (3.3) only decompose the outcome (dependent variable) and do not utilize individual and job characteristics (independent variables) that also contribute to the coverage gap. Following the wage inequality literature that decomposes wage differentials into the "compositional effect" (the wage differential attributed to different compositions across two groups) and the "wage structure effect" (the wage differential attributed to different coefficients on independent variables across two groups)

(Fortin et al. 2010) relying on counterfactual distributions, a similar question that can be asked is "What would the coverage for black people have been had the blacks had the same distributions of both individual and job characteristics of the white population?" The extensions of the Blinder-Oaxaca decomposition introduced in Blinder (1973), Oaxaca (1973), Fairlie (2005), and Fortin, Lemieux, and Firpo (2010) can be used to address this question.

3.2.2 Decomposing Coverage, Offering, Eligibility, and Take-up

Based on the Oaxaca-Blinder decomposition (Blinder 1973, Oaxaca 1973), the changes in health insurance coverage can be decomposed into the impacts of changes in the composition of individual characteristics (compositional effect) and of changes in returns to these characteristics ("health insurance coverage" effect)[2]. Assume that

$$\bar{C} = \sum_{i=1}^{N} \frac{F(X_i \hat{\beta})}{N}, \tag{3.4}$$

where $\bar{C}$ is the mean coverage of the population, N is the size of the population, and X is the set of independent variables in a regression model $F(\cdot)$ that determines coverage. A more general form of the decomposition can be written as:

$$\bar{C}^W - \bar{C}^B = \left[\sum_{i=1}^{N^W} \frac{F(X_i^W \hat{\beta}^W)}{N^W} - \sum_{i=1}^{N^B} \frac{F(X_i^B \hat{\beta}^W)}{N^B} \right] + \left[\sum_{i=1}^{N^B} \frac{F(X_i^B \hat{\beta}^W)}{N^B} - \sum_{i=1}^{N^B} \frac{F(X_i^B \hat{\beta}^B)}{N^B} \right], \tag{3.5}$$

as W and B denote whites and blacks, respectively. If $F(\cdot)$ is a linear function, equation (3.5) reduces to the standard Oaxaca-Blinder decomposition,

[2] This term is called the "wage structure effect" in Fortin, Lemieux, and Firpo (2010) because the explained variable in their context is employee wages. Calling it "health insurance coverage effect" instead should fit this study better and avoid confusion.

which in turn can be written as:

$$\bar{C}^W - \bar{C}^B = \left[(\bar{X}^W - \bar{X}^B)\hat{\beta}^W\right] + \left[\bar{X}^B(\hat{\beta}^W - \hat{\beta}^B)\right], \tag{3.6}$$

in which $\bar{X}^W$ and $\bar{X}^B$ are the vectors of the mean values of the independent variables for whites and blacks, and $\hat{\beta}^W$ and $\hat{\beta}^B$ are the vectors of coefficients for each racial group. The first term on the right hand side of equation (3.6) is the compositional effect, and the second term is the health insurance coverage effect. These effects are easily calculated by estimating two regressions for whites and blacks separately and obtaining vectors of mean independent variables and coefficients from both regressions.

In addition to the "aggregate decomposition" of the coverage into the compositional and health insurance coverage effects, the Oaxaca-Blinder decomposition can also be used to obtain the "detailed decomposition" (Fortin et al. 2010) to estimate the contribution of the variation in each variable to the compositional effect and the total variation in health insurance coverage. Theoretically, the Oaxaca-Blinder decomposition can be directly applied in a linear probability model if the dependent variable is dichotomous. However, Fairlie (2005) shows that applying the Oaxaca-Blinder decomposition on linear probability models may yield less accurate results, especially when the distribution of an independent variable is highly skewed. A modified decomposition that can be applied on Logit and Probit models is proposed in Fairlie (2005) to estimate the detail decomposition of the compositional effect. Note that, assuming linear additivity, the compositional effect in equation (3.5) can be written as:

$$\sum_{j=1}^{J}\left[\frac{1}{N}\sum_{i=1}^{N}\Big(F(X_{ji}^W\hat{\beta}_j^* + X_{-ji}^W\hat{\beta}_{-j}^*) - F(X_{ji}^B\hat{\beta}_j^* + X_{-ji}^W\hat{\beta}_{-j}^*)\Big)\right], \tag{3.7}$$

where J is the number of variables in the array of independent variables, X_j denotes the j^{th} independent variable in the array, X_{-j} is the array of all independent variables except X_j, and $\hat{\beta}^*_j$ and $\hat{\beta}^*_{-j}$ are the estimates obtained from the pooled (whites and blacks) sample on respective set of independent variables. In other words, the contribution of the independent variable X_j to the black-white coverage gap is

$$\hat{D}_j = \frac{1}{N}\sum_{i=1}^{N}\Big(F(X^W_{ji}\hat{\beta}^*_j + X^W_{-ji}\hat{\beta}^*_{-j}) - F(X^B_{ji}\hat{\beta}^*_j + X^W_{-ji}\hat{\beta}^*_{-j})\Big), \tag{3.8}$$

and the variance of $\hat{D}_j$ can be approximated as

$$Var(\hat{D}_j) = \left[\frac{\delta\hat{D}_j}{\delta\hat{\beta}^*}\right]^{\top} Var(\hat{\beta}^*)\left[\frac{\delta\hat{D}_j}{\delta\hat{\beta}^*}\right], \tag{3.9}$$

using the Delta method (Oaxaca and Ransom 1994, Fairlie 2005). In this study, I use the `FAIRLIE` procedure in Stata written by Jann (2008) to calculate the decomposition. Although the above elaboration focuses on the coverage gap, the gaps in offering, eligibility conditional on offering, and take-up conditional on eligibility can be calculated in similar manners.

In addition to comparing the racial gap cross-sectionally, the same method can be used to calculate the change in coverage (and offering, eligibility, etc.) over time for the same racial group or across racial groups. Empirically, this can be done by choosing a counterfactual distribution that resembles the composition of the same ethnic group in a different year and applying exactly the same algorithm. Hence, for example, using the white population in 1995 as the "base group", the following two basic questions can be answered:

1. What would the mean coverage for black people have been in 1995 had the black population had the same composition (including individual and job characteristics) as the white population in 1995?

2. What would the mean coverage for white people have been in 2005 had the composition remained the same for white people since 1995?

Similar questions can be asked based upon various counterfactual distributions using different time and different racial gaps (black-white versus Hispanic-white). Ultimately, one can assess how the compositional effect operates across racial groups cross-sectionally, within the same race over time, or whether the effect itself changes over time.

3.2.3 Impacts of Rising Health Insurance Premium Cost on Coverage

Depending on how the price of health insurance is defined, there are two widely-recognized economic models that predict how health insurance coverage would change following an increase in the premium cost. The first of them treats the full amount of premium as the price of health insurance (Chernew et al. 2005). This model predicts that the demand for health insurance coverage when the premium cost increases would go up if higher premium is associated with a rising magnitude of potential losses (hence the "value" of health insurance is rising). However, the demand for coverage would go down if higher premium is associated with a rising level of moral hazard or adverse selection. The demand would also go down if the "benefit" of being uninsured, namely in the form of uncompensated care, increases– this would occur when the premium goes up but the amount of care that one can receive through uncompensated care stays constant. Consequently, the effect of rising cost on health insurance coverage becomes an empirical question in this model, especially as no reliable measure of moral hazard is available (Chernew et al. 2005).

The other model that is more dominant in health economics textbooks suggest that the health insurance premium cost can be divided into two components: the expected payoff of medical care benefits as well as the load or loading fee, which includes administrative cost, profit of the insurance company, and insurer's risk premium (Feldstein 1999, Phelps 2003). This dominant paradigm hence defines the price of health insurance as the load, which reflects the difference between premium cost and expected payoff. If the load equals to zero, the health insurance is essentially free to the consumers (Phelps 2003). Following this vein, the price or the load of health insurance is the risk premium that a person is willing to give up in exchange for insuring against uncertain medical expenses.

Price theory would suggest that, everything else being equal, the quantity of demand for a certain good should decrease when the price goes up, and there have been numerous studies addressing how the health insurance coverage respond to health insurance premium hikes or subsidies (Blumberg, Nichols and Banthin 2001, Chernew, Frick and McLaughlin 1997, Cutler 2003, Gruber and Washington 2005, Hadley and Reschovsky 2002, Nichols et al. 2001). Overall, scholars have found the demand elasticity for health insurance to be negative but rather small.[3] Nevertheless, the mechanism behind the effect of premium increase on health insurance coverage is rather complicated with a theoretically ambiguous implication. The demand for health insurance would only decrease if the increase in premium is due to the increase in the load while everything else is unchanged. A different literature, on the other hand, finds the distribution of medical care expenditures has not only shifted further to the right but also become more skewed (Chandra and Skinner 2011, French and Jones 2004). If the expected payoff of medical care

[3]Note that Cutler (2003) points out it only takes an elasticity of -0.06 to explain the recent decline in coverage.

benefits (and hence health insurance premium cost) increases in the same magnitude as the mean shift in the medical expenditure distribution, the demand for health insurance should not change because the actuarial value of the health insurance plan is still the same. However, the change in the skewness of the medical care expenditure distribution would actually increase the risk premium that an average consumer is willing to pay for health insurance benefits. In the end, the recent increase in medical expenditure may cause the demand for health insurance to increase to the extent that the load in health insurance premium cost does not outgrow the risk premium that an average consumer is willing to pay. These combined make the effect of insurance premium hike on coverage theoretically ambiguous since the risk premium cannot be imputed. Nevertheless, this very concept to decompose the health insurance premium still underlies the necessity to distinguish between expected payoff and load components of premium cost in order to address how health insurance coverage responds to premium changes.

Regardless of the definition of price, one implicit assumption embedded in these two theories is that the health insurance coverage is always affordable and rational agents can determine whether to buy health insurance coverage or not based on their own utility-maximizing decisions. However, according to the premium information in the Kaiser Employer Health Benefits Annual Survey (2010), the health insurance premium has been outgrowing wage by almost 10% annually in this past decade and is quickly becoming a big financial burden for average workers. Indeed, the average health insurance premium for a family of four was $12,000 in 2007, marking the first time that health insurance premium for a family eclipses minimum wage.[4] If the

[4]The full-time equivalent federal minimum wage is $10,712 in 2007 before it was adjusted upward to $14,500 in 2009.

health insurance premium is too high to be affordable, then the relationship between premium cost and coverage is clear: people will drop out of coverage no matter how "valuable" the coverage is or how skewed the distribution health care expenditures become. In other words, although the economic theories generate theoretically ambiguous predictions and make the question more empirical-oriented, the reasons that lead to a positive relationship between premium cost and coverage may become secondary to the extent that the health insurance premium keeps rising at the current speed. That is, if health insurance coverage becomes increasing unaffordable, the relationship between premium cost and coverage would be overwhelmingly negative.

An additional challenge to estimate the impact of premium price change on coverage is that data on health insurance premium are seldom available.[5] In this study, I rely on three different sources of information: the operational wage index information obtained from the Center of Medicare and Medicaid Services (CMS), the medical care component of Consumer Price Index (CPI), and the mean health insurance premium for employer-sponsored plans at state level from the MEPS-IC, as proxies for the health insurance premium cost at individual level. More detailed explanations regarding these data are presented in the data section of this paper. Briefly speaking, the operational wage index is a component of the Prospective Payment System (PPS) that is used by the Center of Medicare and Medicaid Services to determine the amount of Medicare reimbursement based on various factors that affect the

[5]To overcome such limitation, studies that deal with the relationship between employee cost and coverage have used various employer surveys, such as the Health Insurance Association of America (Chernew et al. 2005), the Kaiser Family Foundation/Health Research and Educational Trust Survey of employer-sponsored health benefits (Cutler 2003, Chernew et al. 2005), Bureau of Labor Statistics Employee Benefit Survey (Gruber and McKnight 2003), the KPMG Peat Marwick data (Gruber and McKnight 2003), or the insurance component of the Medical Expenditure Panel Survey (MEPS-IC) (Blumberg et al. 2001) to obtain the premium cost.

health care cost at each hospital. I use the operational wage index in each year at MSA level and match the information with the CPS as a proxy for health insurance premium.[6]

The medical care component of the CPI, on the other hand, provides a more direct assess of the health insurance cost. The shortcoming of this data is its availability: unlike the CMS wage index, medical care CPI is only available in 27 metropolitans.[7] Besides, although the 1995 CPS provides an unique identifier for the state of residence, its publicly available release does not provide information regarding which MSA an interviewee resides in due to confidentiality concerns. Instead it only provides a variable indicating whether an interviewee resides in a MSA. Since some states (California, Florida, Illinois, Missouri, Ohio, Pennsylvania, Wisconsin, and Texas) have more than one metropolitan, I am not able to uniquely identify the metropolitan that a person lives in based on the publicly available information in the 1995 CPS. This results in the 1995 CPS being dropped completely when the CPI data is matched with the CPS. Roughly one-third of the sample in the remaining waves resides in MSAs where the BLS provides metropolitan-level CPI data. Nevertheless, given its direct assess of the price of other medical care consumptions and services, the medical care CPI can still be used as a proxy for health insurance premiums.

Finally, the state-level aggregates of health insurance premium data from the MEPS-IC capture the variation of health insurance premium across states and over time. Based on the MEPS-IC data, the Agency for Healthcare Re-

[6]Wage index information are obtained from the PPS historical impact files, which can be downloaded at the CMS website `https://www.cms.gov/AcuteInpatientPPS/03_wageindex.asp`.

[7]Including Anchorage, Atlanta, Baltimore/Washington, D.C., Boston, Chicago, Cincinnati, Cleveland, Dallas, Denver, Detroit, Honolulu, Houston, Kansas City, Los Angeles, Miami, Milwaukee, Minneapolis, New York, Philadelphia, Phoenix, Pittsburgh, Portland, San Diego, San Francisco, St. Louis, Seattle, and Tampa.

search and Quality (AHRQ) publishes mean health insurance premium by plan types at the state level starting from 1996.[8] Although the premium information at individual level would be ideal, such data are not available at a nationally representative scale[9], and sample selection problem might occur even if this ideal data set does exist since we seldom are able to know about the health insurance premium cost among those who are not insured. Furthermore, even for those who we can observe the health insurance premium, an ordinary least square model still might produce a biased estimate if health insurance premium is correlated with unobservables; for instance, those who are in higher-paying jobs may also bear higher premium cost due to a more generous health insurance package. Nevertheless, provided that the health insurance premiums for all types of employer-sponsored plans within a state in a given year are all highly correlated, the state-aggregate of total health insurance premium provides a reasonable assessment of how the geographic and intertemporal variations in health insurance premium affect individual coverage.

While the state-aggregate of health insurance premium from the MEPS-IC directly captures the variation in premium, note that the CMS wage index and the medical care CPI represents different component of the health insurance premium under the dichotomy of expected payoff versus load discussed earlier. Each data point in the CMS wage index is a weighted mean wage of hospital workers in a local area. Since the wages of the hospital workers is generally not correlated with the load, the CMS wage index should be more

[8]Hence, the 1995 wave of the CPS is again dropped when the MEPS-IC is matched to the CPS.

[9]Matching MEPS-IC to the household component of the MEPS will generate an individual level data that contains types of coverage and amount of premium. However, the individuals in this merged data set are no longer a random sample due to the non-responding bias in the MEPS-IC.

correlated with the expected payoff component of health insurance premium than with the load. Oppositely, the medical care CPI reflects the overall cost of health care and should be correlated with both the expected payoff and the load components of health insurance premium cost.[10] As a result, the medical care CPI is more likely than the CMS wage index to capture a negative effect of premium cost increase on insurance coverage. Such conceptual difference across the three proxies of individual-level health insurance premium is evident in my data: the correlation between the CMS wage index and the MEPS-IC health insurance premium is 0.61, the correlation between the medical care CPI and the MEPS-IC premium is 0.33, but the correlation between the CMS wage index and the medical care CPI is only 0.14.

Regardless of the proxy used, the full model can be written as:

$$Pr(Coverage_{i,t}) = \alpha \cdot Premium_{i,t} + X_{i,t}^{\top}\beta_{i,t} + S_j + T_t + S_j \times T_t + \varepsilon_{i,t} \quad (3.10)$$

in which $Coverage_{i,t}$ is individual i's employer-sponsored health insurance coverage in year t, $Premium_{i,t}$ is the operational wage index at MSA level, the medical care CPI, or the state-level health insurance premium in the respective years; X is an array of race dummies, individual characteristics, and a set of detail industry and occupation indicators. S_j and T_t are state and year fixed effects, and $S_j \times T_t$ represents the set of state-year interactions. The reason to include the state and year interactions is that private insurance might have been "crowded-out" by the expansion of public insurance programs such as Medicaid and SCHIP, as enrollment in private

[10]Berndt and his colleagues (2001) point out that the CPI is only an index of price and is not related to the quantity of consumption. For example, if the unit price of health care services remains constant but people simply consume more units of health care on average, such increasing expenditure in health care will not be captured by the CPI. Fortunately, this does not seem to be the case according to the recent studies on health care expenditure (Chandra and Skinner 2011, French and Jones 2004), as the technological advancement being a major driving force of unit price increase.

health insurance coverage declined when free or subsidized public health insurance programs became available in the past two decades (Cutler and Gruber 1996, Dubay and Kenney 1997, Yazici and Kaestner 2000, Card and Shore-Sheppard 2004, LoSasso and Buchmueller 2004, Ham and Shore-Sheppard 2005, Hudson, Selden and Banthin 2005, Gruber and Simon 2008). Although the individuals included in this study (employed male aged 18-64) are unlikely to be direct beneficiaries of the public insurance expansion (which mostly targets at pregnant women and children under age 18 below a certain income level), it still is possible that an adult working male drops the employer-sponsored family (and hence his own) coverage because his spouse or kids become eligible for Medicaid and SCHIP, especially when the coverage becomes more expensive. Because individuals with lower household income are both more sensitive to premium increase and more likely to benefit from public insurance expansion, the estimated effect of premium hike on coverage will be biased if the potential crowd-out effect is not properly controlled. Unfortunately, because the February CPS only collects wage information from those who report as being paid hourly and is a survey based on households rather than health insurance units, I am not able to impute each individual's Medicaid eligibility using the total income at the health insurance unit level. This prevents me from following the prevailing "imputed IV" strategy in the crowd-out literature that uses the imputed women's and children's Medicaid/SCHIP eligibility as an instrument for actual enrollment in Medicaid and/or SCHIP (Currie and Gruber 1996a, Currie and Gruber 1996b, Cutler and Gruber 1996, Gruber and Simon 2008). Since the crowd-out is essentially identified through the variation of eligibility rules and level of generosity over states and over time in this imputed IV strategy, I use the set of state-year interactions to capture the effects of public health insurance expansion on employer-sponsored coverage without estimating the magnitude of crowd-out directly.[11]

Alternatively, employer offering, employee eligibility and employee take-up decisions can be estimated simultaneously using sequential Logit models to account for the correlations across stages:

$$\begin{aligned} Pr(O_{it}) &= \alpha^{O} \cdot Premium_{it} + X_{i,t}^{\top}\beta_{it}^{O} + S_j + T_t + S_j \times T_t + \varepsilon_{it}^{O} \\ Pr(E_{it}) &= \alpha^{E} \cdot Premium_{it} + X_{i,t}^{\top}\beta_{it}^{E} + S_j + T_t + S_j \times T_t + \varepsilon_{it}^{E} \qquad (3.11) \\ Pr(T_{it}) &= \alpha^{T} \cdot Premium_{it} + X_{i,t}^{\top}\beta_{it}^{T} + S_j + T_t + S_j \times T_t + \varepsilon_{it}^{T} \end{aligned}$$

where O, E, and T are dummy variables for employer offering, employee eligibility, and employee take-up decisions.

3.3 Data

3.3.1 The Current Population Survey

I use data from the Current Population Survey (CPS) to estimate the coverage gap among racial groups. The Contingent and Alternative Employment Arrangement Supplements (February 1995, 1997, 1999, 2001, and 2005) to the CPS ask questions about the offering of employer-sponsored health insurance and whether an employee is eligible for and enrolls in the plan if being offered. The first two waves of the data are also used in Farber and Levy (2000) to assess how the recent decline in employer-sponsored health insurance is explained by the changing composition of job types. Only people who are employed have their health insurance coverage information collected in these supplements, so those who are not employed at the time of data

[11] The state-year interactions are not included in the model when state-aggregated health insurance premium is on the right hand side of the regression model.

collection are missed out in this study. This should not be a problem as the focus of this study is employer-sponsored health insurance coverage, which by definition is provided to those who are employed and their immediate family members. An exception to this is some of the unemployed who have employer-sponsored health benefit from the former employer through COBRA. Nevertheless, given that only about 10 percent of the workforce would experience a qualifying event that potentially triggers COBRA in a given year and some 20% among them would actually elect COBRA coverage, it implies only 2% of the workforce have coverage through COBRA (Madrian 1998). Furthermore, since COBRA coverage lasts up to 18 months but the median unemployment spell is around 1.5 to 2.5 months between 1996 and 2003 according to the Census Bureau reports (2003, 2006), it is reasonable to assume that only a small portion of COBRA takers, whom by themselves constitute a very small portion of people who are covered by employer-sponsored health benefits, is missing from the February CPS.

Only male wage earners aged between 20 and 64 are included in the sample. Those who are older than 65 are covered by Medicare, and those who are younger than 19 are likely to be covered by Medicaid or SCHIP if they are not covered by their parents' plans. People who are not employed at the time of the survey are excluded because their health insurance coverage information is not available in the data. People who are self-employed are excluded as well because the distinction between employer-sponsored health insurance plans and individual directly-purchase health insurance plans is less clear for these people. Finally, since male and female wage earners have different incentives to obtain employer-sponsored health benefits[12], I only include male wage

[12] All members in the same health insurance unit can be covered under one plan. And hence, if one of the family members already has a job that provides health insurance coverage to all the members, there is essentially no need for other members to find jobs

earners.

The descriptive statistics of selected variables are tabulated in table 3.1. Except for age, all other variables are dummies that take values of 0 or 1. And hence, for example, 77.20% of the male wage earners are non-Hispanic whites in 1995. The race variables are mutually exclusive, including White Non-Hispanics, Black Non-Hispanics, Hispanics, and Other. The CPS uses a separate question to ask whether one is a Hispanic in addition to the question asking about race, and a person is categorized as Hispanic as long as he identifies himself as a Hispanic, regardless of his race. The "White, Non-Hispanic" group includes people who identify themselves as white and not Hispanic, the "Black, Non-Hispanic" group include people who self-identified black and non-Hispanic, and the "Other" group include people who identify themselves as races other than white and black (mostly American Indians or Asian) and not Hispanic. In the 1995 through 2001 waves of data, people are only allowed to select one racial group. However, in the 2005 wave of data, people are allowed to choose more than one group. To assign race categories in the 2005 data, the following decision rule is used: (1) people who identify themselves as Hispanic in the separate question are counted as Hispanics regardless of their choice(s) of race; for those who are not Hispanics, (2) only people who select "white" only are counted as White Non-Hispanics, (3) people who select "black" only or both "white" and "black" are counted as Black Non-Hispanics, and (4) the rest are counted as Other. Such mutually exclusive categorization is consistent with Farber and Levy (2000) and Fron-

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with health insurance or to enroll in the coverage even if it is provided. To the extent that men have higher labor participation rates than women and women are more likely to have part-time jobs not eligible for health insurance, between a married couple it is more likely for the husband than the wife to enroll in employer-sponsored health insurance plans. Besides, pregnant women under a certain income threshold would be eligible for subsidized public health insurance plans, which is another disincentive for women to pay for health insurance out of their own pockets.

stin (2007) as well as literature in black-white wage gap such as Neal and Johnson (1996), Oettinger (1996), and Chay and Lee (2000). I also tried an alternative categorization that include both black non-Hispanics and black Hispanics in the Black group (while black Hispanics are still included in the Hispanics group, so this alternative definition is not mutually exclusive), but the results are not sensitive to the changes, as the black-white coverage gap only increases by less than half of a percentage point.

Besides race, individual characteristics such as age, marital status, and years of education are directly available from the February CPS. Years of education is then transformed into the highest level of education attained: high school dropout (less than 12 years of education), high school graduate (12 years), some college (more than 12 but less than 16 years), and college degree and beyond (16 years or more). Since wages and employee benefits (and hence health insurance provision) tend to be highly correlated with types of job, industry, and occupation, job characteristics also need to be controlled. However, in 2002, the CPS changed from 3-digit industry and occupation codes used in the 1990 census to 4-digit codes used in the 2002 census. While this is not a problem in cross-sectional studies, such change poses a significant challenge to longitudinal studies because there is no one-to-one match from 3-digit to 4-digit industry and occupation codes. Fortunately, the basic monthly CPS coded industry and occupation information using both 3-digit and 4-digit systems between 2000 and 2002. I hence merge the February CPS in 2001 (which only has industry and occupation information in 4-digit codes) with the basic monthly CPS to obtain both the 3-digit and 4-digit industry and occupation codes for individuals in February CPS based on the method proposed in Madrian and Lefgren (2000). Around 0.07% of the individuals in the 2001 wave of February CPS cannot be matched with the monthly CPS and hence are dropped from my sample. This allows me to compare 1995, 1997, 1999, and 2001 waves of data based on the 3-digit industry and occupation codes and 2001 and 2005 waves of data based on the 4-digit codes. I also group the industries and occupations by the CPS detailed industry and occupation categories, which consist of 51 industry and 45 occupation groups for the 3-digit codes and 51 industry and 22 occupation groups for the 4-digit codes. In addition to industry and occupation, a couple of variables describing job types, which are shown to have significant impacts on employer-sponsored health insurance benefit (Farber and Levy 2000), are also included: whether the job is full time or part time, and whether the job has been held for more than one year.

Most importantly, the February CPS has detailed questions on the source of health insurance coverage. As mentioned earlier, only those who are employed at time of survey are asked of these questions. The first question asks the interviewee about the source of health insurance coverage, which can be employer-sponsored plans, individual direct-purchased plans, Medicare, Medicaid and other public plans, or uninsured. If the answer to the first question is "employer-sponsored plans", the following question is who the provider is– either the interviewee's own employer or spouse's employer. Finally, all wage earners are asked whether their employers sponsor any health insurance plans, whether they would be eligible for the plans, and whether they take up the plans. If the interviewee has an employer who offers health benefit but either is not eligible or does not enroll in the plan, the reason of ineligibility or not taking up is also collected in the data. Based on these detailed information, a data set of whether a wage earner has a health insurance plan sponsored by own employer as well as employer offering decision, employee eligibility, and take-up decision can be constructed along with the individual characteristics and job type variables introduced above.

3.3.2 The Center of Medicare and Medicaid Services Wage Index

As mentioned in an earlier section, given the lack of health insurance premium information in the Current Population Survey, I use the operating wage index data from the Center of Medicare and Medicaid Services (CMS) and other measures as proxies for health premium cost. The operating wage index information is an important component of the Prospective Payment System, which is used by the CMS to calculate the amount of reimbursement to the hospital for inpatient Medicare discharges.

In order to construct the wage index, the CMS groups hospitals by geographic locations. Hospitals located in urban areas are grouped by Metropolitan Statistical Area (MSA); if a hospital is located in a rural area and does not belong to an MSA, it is grouped with all other hospitals in the same state that are also in rural areas. Each of the MSAs and state-level aggregates of rural areas represents a "local labor market". Based on hospital-level surveys conducted by CMS, the average hourly wage (AHW, which is simply the total wage cost divided by total hours worked) of hospital workers can be calculated for each local labor market. Each local labor market's AHW is then divided by the national AHW, becoming the operating wage index. And hence, the operating wage index is the weighed price of hospital workers at a local labor market compared to the national level, and it is applied to the labor-related portion of the base rate for Medicare reimbursement. For each Medicare discharge case, the base rate is then adjusted by the relative cost of each MS-DRG (Medicare Severity Diagnosis Related Group, which groups patients with similar problems that are expected to consume similar amount of hospital resources), the length of stay, and other hospital-specific adjustments (such as whether the hospital provides services to a disproportionate share of Medicaid patients or uncompensated care, whether it is a teaching hospital, whether it is a sole community hospital, etc.), to calculate the Medicare reimbursement to a hospital.[13]

The operating wage index is a good proxy for health insurance premium for several reasons. First, Cutler (2003) notices that most of the variation in health insurance premium cost originates from the changes in health care cost. According to the estimation by the CMS, the “labor share”, namely the local wage rates and fringe benefits and are captured in the operating wage index, constitutes to roughly 68% of the base rate of Medicare reimbursements. Even though the non-labor share of the base rate (such as land cost) may be significantly different from one geographic area to another, it tends to remain relatively constant over time within a given market and does not contribute to the changes in health care costs over time. Second, although private insurance companies may not calculate the payment to hospitals exactly the same as the Medicare does, the reimbursement made by private insurance companies and the Medicare should be highly correlated given that the health care services are provided by the same group of physicians and nurses within a labor market. And hence, it is conceivable that most of the changes in health insurance premiums, which reflect the changes in health care costs over time, are driven by the changes in labor-related costs at local labor markets.

Because the CMS wage index is only a price index with the national average

[13]For more information on how the operating wage index and other factors are used to calculate the Medicare reimbursement to the hospital, see a brief report by the Medicare Panel Advisory Commission (2010), available at `http://www.medpac.gov/documents/MedPAC_Payment_Basics_10_hospital.pdf`

hourly wage normalized to 1 in each year, the wage index by itself does not capture the growth of wages in hospital sectors over time. The Federal Register keeps the records of nominal average hourly wages in each local area that are used to calculate the index. I hence obtain the nominal wages from the Federal Register, deflate them based on the Social Security National Average Index[14] to reflect the wage in hospital sectors and hence price of health care relative to wage income. Doing so enables the series of this "deflated" wage index to include variations of health care cost both across geographic areas and over time. Besides, one of the necessary conditions for the CMS wage index to be a good proxy for health premium cost is that it has to grow faster than the wages of all workers, since the health insurance premium outgrows wage by almost 10% each year during the time that my study covers. Although the CMS wage index does not grow as fast as the health insurance premium itself does, a simple t-test suggests that CMS wage index on average outgrows the overall wages for all workers[15], with a t-statistic of 28.14.

Matching the wage index to the CPS is, however, not straightforward for two reasons. First, due to the nature how the wage index is constructed, there is some lagged time between when wage survey is conducted and when the wage index made from that specific survey becomes in effect for reimbursement calculation. Typically the lag is four years, that is, wage index in year t is based on wage surveyed in year $t-4$. Second, the CPS in year t reports the health insurance in year $t-1$, and health insurance in year $t-1$ depends on the health insurance premium in year $t-1$, which in turn depends on health care cost in year $t-2$ or even earlier years. This poses a challenge

[14] Available at http://www.ssa.gov/oact/cola/AWI.html

[15] Wage information is obtained through the CPS Outgoing Rotation Groups and clustered at MSA level.

to using wage index as a proxy for health insurance premium– there is not necessary a "correct" match of the wage index to the CPS. Nevertheless, in this study I match the wage index in year $t + 2$ (which is based on wage surveys in year $t - 2$) to the CPS in year t (which reflects the decision to enroll in health insurance in year $t - 1$ as a response to the health insurance premium in year $t - 1$, which is based on health care cost in year $t - 2$). And hence, the CMS wage index in 1997 is matched to the February CPS in 1995, the wage index in 1999 is matched to the CPS in 1997, etc. For each of these wage index and CPS pairs, wage index is then matched to the CPS individuals by geographic area (MSA for urban areas and state-aggregates for rural areas).

3.3.3 Medical Care Component of the Consumer Price Index

The CPI has eight major categories: food and beverages, housing, apparel, transportation, medical care, recreation, education and communication, and other goods and services. The ratio of its medical care component divided by the components excluding medical care hence represents the relative price of health care commodities and services compared to other consumptions. This provides an alternative assess of health insurance premium cost to the CMS wage index.

The medical care component of the CPI is further split into two categories: medical care commodities (MCC, including medical drugs and medical equipment supplies) and medical care services (MCS, including professional services, hospital and related services, and health insurance). Although the health insurance premium cost accounts for almost 50% of the medical care CPI,[16] the Bureau of Labor Statistics does not collect information on health

[16] For more technical details on how the medical care CPI is constructed, including the

insurance premium directly. Instead, the health insurance cost is imputed indirectly based on the changes in the prices of medical care items covered by insurance policies and the changes of administrative costs. The first item reflects the insurer's reimbursements/payments for medical treatments, and the second item is calculated using the changes in insurance companies' retained earnings ratios. The weights furthermore exclude employer-paid health insurance cost as well as tax-funded health insurance programs such as Medicaid and Medicare Part A in order to more precisely reflect out-of-pocket expenditures for health care services. Unfortunately, the CPI does not release a separate index for health insurance premiums, and even the series of MCC and MCS components are not separately released until very recent years. This limitation forces me to use the medical care CPI as a whole rather than its subcomponent that is arguably more correlated with the health insurance premiums.

To incorporate the medical care CPI in my analyses, I first calculate the ratio of medical care CPI to the CPI excluding the medical care component. This gives the relative price of medical care consumptions and services to other goods. Between 1984 and 2010, the relative price of medical care consumptions and services increased significantly, with increments ranging from 55% (Kansas City) to 143% (Boston). The medical care CPI series is only available in 27 metropolitans, but the BLS did not collect such information from a few of them until late 1990s. To account for the difference in the base year across metropolitans, the relative price of medical care to non-medical care goods is re-normalized to 1 for each metropolitan in 2005. This series is then merged to the February CPS, and the 1995 CPS has to be dropped

definitions of refined items in each category and their relative weights, see "Measuring Price Change for Medical Care in the CPI" on the Bureau of Labor Statistics website, `http://www.bls.gov/cpi/cpifact4.htm` completely because MSA information is not available due to confidentiality concerns.[17] Roughly one-third of the CPS sample in the remaining waves lives in the metropolitans covered by the CPI series. Similar to the timing issue in the CMS wage indices, the relative price in year $t-2$ is merged to the CPS in year t to reflect the causal relationship between health care cost and health insurance coverage. As a robustness test, I also match the moving average of the relative price in years $t-4$, $t-3$, $t-2$ with year t of the CPS. Using the relative price in one year or the three-year moving average yields similar estimates.

3.3.4 The Medical Expenditure Panel Survey (MEPS)

The insurance/employer component of the MEPS (MEPS-IC) collects from employers the information regarding the types of employer-sponsored health insurance plans, the mean amount of premium per enrolled employee, and the mean amount of employee contribution to the total premium. The sample is drawn at establishment level from the U.S. Census Bureaus Business Register (for private sector establishments) or from the Census of Governments (for state and local government units). Overall, around 35,000 to 40,000 establishments and government units are surveyed each year.

The MEPS-IC started in 1996. Due to the budget limitation in the first few years, it only representatively sampled establishments from the 30 most populous states in the U.S. plus 10 of the 20 less populous states;[18] hence,

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[17]The 1995 CPS provides an unique identifier for the state of residence, but its publicly available release does not provide information regarding which MSA an interview resides in due to confidentiality concerns. Instead it only provides a variable indicating whether an interviewee resides in a MSA. Since some states have more than one metropolitan, I am not able to uniquely identify the metropolitan that a person lives in based on the publicly available information in the 1995 CPS.

[18]Establishments from the remaining states were still sampled, but the sample size was not enough to generate reliable estimates.

state-level estimates were only available for 40 states each year in the MEPS-IC until 2002. Starting from 2003, the MEPS-IC improved the sample size and design so that it now covers all 50 states plus the District of Columbia and can even produce reliable descriptive statistics of premium information at the MSA level.

Although the MEPS-IC data are not available without an approval from the U.S. Census Research Data Center, the Agency for Healthcare Research and Quality (AHRQ) releases the mean health insurance premium at state level by type of plans and the size of employer based on MEPS-IC each year. I match the state-level total premium in 1996, 1998, 2000, and 2004 to the corresponding waves of the CPS[19] to perform the final assessment of the relationship between health insurance premium and employer-sponsored health insurance coverage. The AHRQ releases premium information for three types of plans: single coverage, employee-plus-one, and family coverage. Since insurance premium costs for different types of plans are highly correlated and my identification strategy is based on the variation of health insurance premium over time as well as across state rather than the actual amount of premium for each CPS individual, I only rely on the family coverage premium as the proxy for the health insurance premium cost. For the states that the state-level estimate of premium is not available due to sample size constraints in earlier waves of the MEPS[20], the AHRQ would lump them together and release the mean premium in "all state not separately listed". My estimates reported in the following section are not sensitive to the in-

[19]This corresponds to 1997, 1999, 2001, and 2005 waves of the February CPS, which respectively collect health insurance coverage information in 1996, 1998, 2000, and 2004. The 1995 wave of the CPS is dropped because a corresponding wave of MEPS is not available.

[20]States that do not have state-level premium data in all four waves are Alaska, Delaware, Hawaii, Idaho, Maine, Mississippi, Montana, North Dakota, New Hampshire, Nevada, Rhode Island, South Dakota, Vermont, Wyoming, plus the District of Columbia.

clusion of these higher-level aggregates. Finally, the premium information by the AHRQ only includes private sector establishments. Excluding public employees from my CPS data do not lead to significantly different results, presumably due to the high correlation of premiums in private and public sectors.

3.4 Result

I present how the overall health insurance coverage (regardless of sources), own employer-sponsored health coverage, and their respective racial gaps in each wave of the February CPS before I decompose the own employer-sponsored coverage into employer offering, employee eligibility, and employee take-up. These are followed by the main results of this study: how the racial gap can be explained by individual and job characteristics and how the racial gap is affected by the recent increase in health insurance premium cost.

3.4.1 Overall Health Insurance Coverage

Table 3.2 shows the proportion of the sample that has health insurance coverage between 1995 and 2005 based on the February CPS. The proportion of the sample covered by health insurance (regardless of the source of coverage) increased from 85.4% in the 1995 CPS to 86.5% in 2001 but then decreased to 83.3% in 2005.[21] A breakdown by race shows the same pattern that the coverage increased between 1995 and 2001 waves of the CPS but decreased in the 2005 wave. In all years, non-Hispanic white workers are

[21] Note that the CPS actually asks about health insurance coverage in the previous year. And hence, the 1995 CPS reflects the health insurance coverage in 1994, the 1997 CPS reflects the coverage in 1996, etc. To avoid confusions, I still label each wave by the year that the data were collected, although the data reflect the health insurance coverage status a year earlier.

more likely to have coverage than non-Hispanic black workers, who in turn are more likely to be covered than Hispanic workers. The next two columns in table 3.2 show the black-white and Hispanic-white coverage gaps. The back-white and the Hispanic-white gaps move in the same direction except in 2005, when the black-white gap shrank but the Hispanic-white gap enlarged. Besides, the gaps are somewhat consistent (not statistically different over time) but again except in 2005. The last two columns in table 3.2 shows the "regression-adjusted" difference, obtained through the coefficients on dummies of blacks and Hispanics from separate OLS regressions for each year that have a dummy of coverage on the left-hand side and control for age (entered as 45 age dummy variables rather than a single linear term since the relationship between health insurance coverage and age is not likely to be linear), gender, marital status (single, married, widowed, divorced, and separated), the highest level of education (high school dropout, high school, some college, and college degree or higher), state of residence (50 dummy variables for 50 states plus the District of Columbia), industry and occupation (detailed CPS industry and occupation category dummy variables), job types (full-time versus part-time), and whether the employee has held the job for more than 1 year. The adjust coverage gaps are only 40% to 50% as large as the raw gap yet are statistically significant, suggesting that some 50% to 60% variation in the racial coverage gap can be explained away by individual characteristics and job types. This is consistent with the wage and health insurance coverage gap literature (Altonji and Blank 1999, Crow et al. 2002, Monheit and Vistnes 2000, Reschovsky et al. 2007) that individual and job characteristics explain a non-trivial portion of male black-white wage or insurance coverage gaps.

The proportion of the sample that is covered by own employer-sponsored health insurance plans is shown in table 3.3. Numbers are, albeit slightly higher, close to those in a recent EBRI report that uses a similar source of data (Fronstin 2007). Since information about health insurance coverage is only available for employed people in the February CPS, a comparison of numbers in table 3.3 and the time trends in figure 3.1 (which is based on the March CPS using all adults) suggest that simply conditioning on employment significantly increases the rate of employer-sponsored health insurance coverage for all races and decreases the racial coverage gap to some extent. Similar to table 3.2, the adjusted racial gaps are much smaller than the raw gaps but remain at least marginally significant except the black-white gap in 2005. Furthermore, the black-white gap seems to be more volatile than the Hispanic-white gap, as the Hispanic-white gaps across the five periods are not significantly different from one another. The adjusted black-white gap hovers between 45% to 75% of the raw gap (again, except for in 2005), but the adjusted Hispanic-white gap stays at around 30% of the raw gap over time.

To summarize, tables 3.2 and 3.3 reveal similar information. First, overall coverage went up from 1995 to 2001 and contracted between 2001 and 2005. Second, both black-white and Hispanic-white gaps are non-trivial, even after individual and job characteristics are controlled for. The following subsections further explore these observations. To begin with, I decompose the employer-sponsored health insurance coverage into employer offering, employee eligibility, and employee take-up decisions. Such decomposition allows me to estimate the racial gap in each stage and how each stage contributes to the changes and racial gaps in coverage. I then use a simple variance decomposition method to assess the main reasons that affects racial gaps in employer-sponsored health insurance offering, eligibility, take-up, and coverage. Finally, I answer the question how the racial gap in health insurance coverage is affected by the changes in the cost of health insurance premium.

3.4.2 Components of Employer-Sponsored Health Insurance Coverage

In this section, I decompose employer-sponsored health insurance coverage into three components: offering, eligibility, and take-up. Based on equation (3.1), tables 3.4, 3.5, and 3.6 show the employer offering, employee eligibility, and take-up rates of employer-sponsored health insurance plans as well as racial gaps using the same format as tables 3.2 and 3.3. The eligibility rate is conditional on offering, and the take-up rate is conditional on being eligible.

Similar to the patterns of health insurance coverage regardless of sources and the coverage of own employer-sponsored health insurance, table 3.4 shows the offering rate increased from 1995 to 2001 but decreased in 2005. Both black-white and Hispanic-white gaps in offering rate are of similar magnitudes to the coverage gaps in table 3.3. The regression-adjusted black-white offering gap is only statistically significant for three out of five waves and is also highly volatile, again similar to the pattern in table 3.3. The adjusted Hispanic-white offering gap, on the other hand, is significant in all waves and remains at a relatively consistent level. The similar patterns of racial gaps in table 3.3 and 3.4 suggest that employer offering may be an important determinant in the racial gaps in employer-sponsored health insurance coverage.

Conditional on offering, the eligibility also increased from 1995 to 2001 but contracted in 2005 (Table 3.5). However, there is basically no difference among white, black, and Hispanic workers in terms of eligibility for employer-sponsored health insurance, as the adjusted racial gaps stay around zero in spite of some fluctuations. Conditional on being eligible, take-up rates follow a decreasing trend for whites and Hispanics but less so for blacks (Table 3.6). The regression adjusted difference suggests that, except for the whites and blacks in 1995, employees of all racial groups are equally likely to take up the health insurance plan if they are eligible. Given that there is no significant racial gaps in employee eligibility and take-up decisions, results in tables 3.5 and 3.6 affirm that most of the coverage gap is caused by the difference in employer-offering.

Next, I decompose the changes in offering, eligibility, and take up of employer-sponsored health insurance over time based on equation (3.3). Given that most of the trends (including coverage, offering, and eligibility) increased between 1995 and 2001 but contracted between 2001 and 2005, I perform the decomposition in two parts (1995-2001 and 2001-2005) in order to assess the factors contributing to the rise and decline of employment-sponsored health insurance. The results are in table 3.7. Panels A and C in table 3.7 are the raw changes in coverage, offering, eligibility, and take-up over time according to numbers in tables 3.3 through 3.6. Panels B and D are the results based on the decompositions implied by equation (3.3), that is, the changes in coverage is decomposed to the contributions of changes in offering, eligibility, and take-up. For example, the first column in panel B implies that out of 1.3 percentage points increase in coverage for whites between 1995 and 2001, 1.7 percentage points is contributed by the increase in offering, 1.0 percentage point is contributed by the increase in eligibility, while changes in take-up actually contributes to 1.2 percentage points *decrease* in coverage.[22]

[22]Note that the contributions of employer-offering, employee eligibility and employee take up may not necessarily add up to the total change in coverage. This is because the full expansion of equation (3.3) also includes "interaction" terms (using the terminology in Farber and Levy (2000)) among changes in offering, eligibility and take-up. These "interaction" terms are $\Delta O \Delta E$, $\Delta E \Delta T$, $\Delta O \Delta T$, and $\Delta O \Delta E \Delta T$ in the full expansion of equation (3.3).

A few patterns emerge from panels B and D of table 3.7. The change in employer offering is the most important contribution to the change in coverage for whites and Hispanics between 1995 and 2005. The huge decrease in offering for the Hispanics in 2001-2005 may be the reason why the (unadjusted) Hispanic-white coverage gap increased during that period of time, as shown in figure 3.1 and tables 3.2 and 3.3. On the other hand, change in take-up rates is the most important determinant of change in coverage for blacks. Because whites plus Hispanics account for around 90% of the sample, the last column that includes the pooled sample has a result very similar to that of whites and Hispanics.

3.4.3 Changes of Racial Gaps in Offering, Eligibility, and Take-up over Time

Table 3.7 shows how the racial differences in employer offering, eligibility, and take up affect respective coverage over time. Consistent with an earlier study (Cooper and Schone 1997), the take-up ratio has been decreasing since the mid-1990s. It is also clear that rather than take-up rates, it is changes in employer offering that determines whether the overall coverage trends upwards or downwards. However, the numbers in table 3.7 are not adjusted by the difference in individual and job characteristics, and neither do they illustrate a clear picture of the racial gap in each of the components.

Equation (3.2) provides a channel to further explore the racial gap in coverage over time. Specifically, including the interaction terms between race and time in the regression of coverage (offering, eligibility, etc.) equation enables the identification of racial gaps in employer offering, eligibility, take-up, and their aggregated effect on overall coverage over time. However, due to the sequential nature of the components leading to employer-sponsored health insurance coverage, I model offering, eligibility, and take-up decisions in a single sequential Logit model instead of separate OLS regressions. Table 3.8 shows the coefficients on the interaction terms between the dummy variable indicating the minority group and the dummy variable indicating the later year in the given period; hence a significant coefficient suggests the racial (black-white or Hispanic-white) gap (in offering, eligibility, or take-up) changes over time. All numbers in panel A are from the same sequential Logit regression and all numbers in panel B from another. For example, the first number in panel A of table 3.8 suggests the black-white gap in employer offering increased by 5.1% between 1995 and 2001, as a coefficient of -0.056 represents a log-odds ratio of 0.949. Interestingly, none of the coefficients on the Hispanic-white gap is significant, suggesting a stable Hispanic-white gap in offering, eligibility, and take-up over time. For the black-white gap, both of the coefficients on offering are significant, and the 1995-2001 coefficient on take-up is also significant. These are consistent with the numbers in table 3.7 that employer offering for blacks follows a different trend from employer offering to whites and Hispanics. Overall, the black-white gap is less stable than the Hispanic-white gap in employer-sponsored health insurance coverage.

3.4.4 Decomposition of Coverage and Offering

In this subsection, I use the non-linear decomposition method for Logit regressions proposed in Fairlie (2005) to further explore the dynamics of employer-sponsored health insurance across racial groups and over time. Since there are no significant racial gaps in eligibility conditional on offering and take-up conditional on eligibility, only coverage and offering are addressed in this subsection.

Tables 3.9 and 3.10 show the decomposition of cross-sectional racial gaps in coverage and eligibility, respectively. The first three rows in panel A of table 3.9 are the row coverage rates for whites, blacks, and the raw racial gap in each year. These are followed by a detailed decomposition using the same set of control variables used to calculate the adjusted racial gaps in tables 3.2 through 3.6. More specifically, these variables are split into six categories: age (45 age dummy variables), marital status (single, married, widowed, divorced, and separated), education (high school dropout, high school graduate, some college, and college graduate or higher), state of residence (50 dummy variables for 50 states plus the District of Columbia), job types (whether the job is full-time and whether job tenure is more than one year), and industry and occupation (dummy variables for detailed CPS industry and occupation categories). And hence, for example, age contributes to 0.006 (with a standard error of 0.001) of the 0.060 black-white coverage gap (10.13%) in the year of 1995. All the control variables combined contributes to 0.022 of the 0.060 raw gap (36.67%). In other words, the racial gap would decrease by 0.022 (36.67%) had the blacks have the same distributions of those control variables as the white sub-population. The rest of panel A in table 3.9 shows the decomposition of black-white coverage gap over time. The numbers suggest a non-trivial portion of the black-white coverage gap can be explained by the discrepancy of individual characteristics between blacks and whites, especially education and job type.

Panel B in table 3.9 shows the decomposition of the Hispanic-white coverage gap. Overall, the discrepancy in individual characteristics explains a slightly higher proportion of the Hispanic-white coverage gap than black-white coverage gap. Unlike in panel A, it is the distribution of industry and occupation rather than types of job that explains a bigger portion of the gap. Discrepancy in education is also an important factor that contributes to the Hispanic-white coverage gap, with an effect size three to four times larger than its effect on the black-white gap.[23] The results in both panels of table 3.9 suggest the importance of education and general human capital, which is also related to the finding that industry, occupation, or types of job are important reasons for the sizable black-white and Hispanic-white coverage gaps. Overall, education, job type, industry and occupation consistently contribute to around 90% of the compositional effect of the racial coverage gap (the difference in coverage attributed to discrepancies in the distributions of independent variables) in all years.

Similar to table 3.9, the decomposition of employer offering is shown in table 3.10. The overall pattern is similar to the decomposition of coverage gap that the discrepancy in education, job type, industry and occupation explains a significant part of the offering gap and contributes to the major portion of the compositional effect. Blacks and Hispanics are less likely to work in the industry, occupation, and job type that come with employer-sponsored health insurance offering– presumably jobs with lower wages and less generous employee benefits. Table 3.7 already shows that it is employer offering that affects the trend in employer-sponsored health insurance coverage. Tables 3.9 and 3.10 confirm this and further suggest that the discrepancy in employer offering among racial groups differs in the level of education and the jobs that people have, emphasizing the role of education (general human capital), job types, industry, and occupation (job characteristics and quality) in determining health insurance offering and hence coverage.

[23] In percentage terms, the effect size of education is slightly smaller in the Hispanic-white gap than in the black-white gap, mostly because the Hispanic-white gap is larger.

Among the sets of the independent variables used to produce tables 3.9 and 3.10, education probably is the most important. Education is highly correlated with earnings and can also be seen as a mediating variable that influences a person's job type, industry, and occupation. Earnings are directly related to whether an employee can afford health insurance coverage, and job type, industry, and occupation all are correlated with the likelihood of health insurance offering. And hence, an alternative specification is to only include demographics information and education to estimate the total "direct" effect of education on the racial gap in health insurance coverage. The results are not reported here to save the space, but the explanatory power of education in tables 3.9 and 3.10 are generally only 50% to 60% as large as the numbers in respective alternative specifications not reported here. This means 40% to 50% of the direct effect of education is absorbed through job types, industry, and occupation. And hence, the importance of education and hence general human capital is to some extent understated when job characteristics are included in the decomposition models. Additionally, as it has been shown in some recently studies that focus on non-pecuniary returns to education (McMahon 2009, Oreopoulos and Salvanes 2009), ignoring health insurance benefit may understate the returns to education.

I next decompose the changes in coverage and offering over time for the same racial group. Although the within-race coverage change does not by itself explain the racial gap, this set of analysis helps to pin down whether there are factors other than individual and characteristics that affect the racial gap in health insurance coverage and offering. If the discrepancies in individual and job characteristics explain the within-race change in coverage/offering over time as well as they explain the cross-sectional racial gaps, I can more confidently conclude that discrepancies individual and job characteristics are the main reason that contributes to the difference of health insurance coverage across the population. Also, this would imply the racial gap in health insurance coverage is mainly mediated by the gap in individual and job characteristics, suggesting that policies that intend to improve the health insurance coverage of the minorities should focus more on these mediating factors. However, if the individual and job characteristics do not explain the within-race coverage variation as well as between-race variation, it means either these characteristics have interactions with races or there are some other factors not captured in the CPS data that affect the racial coverage gap, suggesting more extensive firm-level studies on the provision of health insurance benefits or compensation practices in general. The results of longitudinal, same-race decompositions are shown in table 3.11.

There are two reasons why the time trend of coverage and offering should be split in 2001. First, as tables 3.2 and 3.3 show, both the overall (regardless of sources) and employer-sponsored health insurance coverage increased between 1995 and 2001 but decreased between 2001 and 2005. And hence, rather than decomposing the changes over the 10-year span, decomposing the increase (1995-2001) and decrease (2001-2005) allows me to capture the reasons for both the increase and the decrease, especially if they are driven by different forces. Second, due to the changes in the way industry and occupation are coded in the CPS in the early 2000s as well as the lack of the one-to-one match between different coding mechanisms, there is no natural way to appropriately control for detail industries and occupations if the data span across the two coding schemes. This makes the split inevitable in order to control for detail industry and occupation categories.

The interpretation of the results in table 3.11, however, is less straightforward than tables 3.2 and 3.3. To begin with, many factors in table 3.11

have “negative” explanatory power, where the compositional effect goes in a direction opposite to the observed change in coverage or offering over time. For example, the result in the first column of panel A suggests the coverage would have decreased by 1.4% rather than increasing by 1.2% had the white people had the same distributions of industry and occupation in 1995 and 2001, which is basically -116.40% of the observed change. The decompositions for black people seem to be more vulnerable to this, as not only individual factors but also the collective effects of all factors have contributions of negative numbers, absolute values larger than 100%, or both. These numbers make the results hard to interpret. Fairlie and London (2009a) have the same problem in their results as well and conclude that it is the common problem when the observed change is small and the two groups being compared are largely homogeneous.

Regardless of the challenge to interpret some of the numbers, a common theme of the results in table 3.11 is that the compositional effect is a less significant driving force of the change in health insurance coverage and offering over time for the same racial group, given the compositional effects are smaller in table 3.11 than in tables 3.9 and 3.10 when the racial gaps are decomposed. More importantly, note that the coefficients for the same factor and racial group tend to have the same sign in panels A and B. For instance, the coverage for whites would have increased by 0.002 in 2001 and 0.003 in 2005, respectively, if the age distribution had remained the same. However, since the coverage of the white people increased in 1995-2001 but decreased in 2001-2005, the effects of age on actual changes in coverage are oppositely signed in both periods. Since the composition (including demography and job characteristics) for the same racial group is not likely to change significantly within 10 years, the fact that the coefficients tend to have the same sign in panels A and B indicates that the compositional effect may not be the main reason behind the rise and fall of health insurance coverage and offering between 1995 and 2005. In the following, I switch the focus to how the rapid increase in health insurance premium cost affects of health insurance coverage over time.

3.4.5 The Effect of Rising Health Insurance Premium Cost

As the previous subsection shows, individual and job characteristics explain the increase in employer-sponsored health insurance coverage in late 1990s fairly well but fail to account for the decline of coverage between 2001 and 2005. Table 3.7 also suggests that it is the decline of offering, rather than the decline of take-up, that overturned the trend of employer-sponsored health insurance coverage between 1995 and 2005. Namely, employer offering increased in late 1990s but declined after 2001, and the trend of coverage followed the same pattern despite that employee take-up has decreased since 1995. Not only did the decline in offering occur simultaneously with the decline in coverage, it was also accompanied by a widening racial coverage gap, especially the Hispanic-white one. All these observations combined suggest that there are factors other than individual and job characteristics influencing employer offering in early 2000s, and minority groups may have been more adversely impacted according to the widening racial gap.

One of the commonly accepted reasons that explain the change in health insurance coverage in the past two decades is the rise in health insurance premium cost (Blumberg et al. 2001, Chernew et al. 1997, Cutler 2003, Gruber and Washington 2005, Hadley and Reschovsky 2002, Nichols et al. 2001). Despite the mixed empirical evidence, economists tend to believe that employees bear the full cost of health insurance premiums, and the mixed findings are caused by endogeneity in the data rather than imperfect theory (Blumberg 1999, Gruber 1994, Gruber 2000, Levy and Feldman 2002, Rosen 1986, Sheiner 1999).[24] When the premium cost goes up, it is reflected in the total compensation package either as a lower take-home pay or a slower wage increase.

However, the earlier discussion suggests that the health insurance premium can go up for two reasons. Provided that the benefit package unchanged, the premium can go up if the "load", including the administrative cost and the profit of insurance companies, increases. On the other hand, the premium can also go up with the level of expected benefit, which is especially the case when the medical care cost raises. The former corresponds to a decrease in quantity of demand for health insurance, but the later may lead to an increase demand of health insurance if people are willing to pay more risk premium to insure against unexpected high amount of medical care expenses. These two effects combined make the sign of demand elasticity for health insurance ambiguous. Although the theory would suggest a downward sloping demand curve, this only applies to the "load" part of the premium. And hence, in addition to the challenge that premium information is seldom available in a nationally representative data, it is also important to find appropriate proxies for different components (expected benefit versus the load) of the health insurance premium.

In this study, I use three different proxies for health premium cost: the CMS wage index, the medical care component of the CPI, and the state-level

[24] Another reason why the wage-health insurance trade-off is hard to observe is the firm's limited ability to discriminate wages on the basis of the value of health insurance and compensate employees on a person-to-person basis depending on individual decision to enroll in health insurance plans (Cutler 2003, Gruber 1994, Gruber and Madrian 2004, Sheiner 1999).

health insurance premium from MEPS-IC. Cutler (2003) argues that most of the variation in health premium is driven by the changes in health care cost, and including state dummy variables in the regression equation makes the CMS wage index an appropriate proxy for the expected benefit part of health premium cost because these dummy variables capture geographical or cross sectional variation of medical care sector wages not related to health care (and hence health insurance premium) cost. The medical care CPI, on the other hand, represents the overall cost in health care spending and proxies for both the benefit and the load component of the premium cost. And hence, I would expect the effect of changes in medical care CPI to be more negative than changes in the CMS wage index on health insurance coverage based on the theory articulated above. At last, the MEPS-IC premium information at the state level is expected to capture the overall impact of the premium increase on coverage since it is by its nature correlated with both components of the premium.

Formally, the model that accounts for the health care cost is estimated as equation (3.10) with standard errors clustered by both years of observation and states. The term $Premium_{i,t}$ is either the CMS wage index, the medical care CPI, or the MEPS-IC state-level premium. The CMS wage index is entered into the equation as the logarithm of real hourly wages of hospital labor at MSA level in 2009 U.S. Dollars (adjusted based on the Social Security National Average Wage Index). More specifically, since the CPS in year t asks about the health insurance coverage in year $t-1$, CMS wage index for fiscal year $t-2$ is matched to year t of the CPS as the coverage in year $t-1$ is influenced by the premium in year $t-1$ and hence the health care cost in year $t-2$. The medical care CPI enters the regression in the same time lag as the CMS wage index, and the CPI is also normalized that one unit change represents one percentage point change in the relative price of health care expenses to other goods and services. The MEPS-IC premium in year $t-1$, which reflects the health insurance premium in that same year, is directly matched to the year t of the CPS.

The estimation results of equation (3.10) are shown in table 3.12. All the coefficients are estimates using linear probability models with standard errors clustered by years of observation and states of residence. Columns 1 through 3 use the CMS wage index as the proxy for health insurance premium, columns 4 through 6 use the medical care CPI, and columns 7 and 8 use the MEPS-IC state level premium. The left hand sides of all regressions in table 3.12 are own employer-sponsored health insurance coverage, and I do not differentiate offering, eligibility, and take-up in this table. Columns 1, 4, and 7 are the baseline model, several interaction terms are added in columns 2, 5, and 8, and the set of state and year interactions is added in columns 3 and 6 to capture potential crowd-out effect. No state-year interactions are used for the model where the MEPS-IC premium is on the right hand side because this measure by itself only varies by state and year.

The numbers in columns 1 through 3 suggest that the increase in the CMS wage index is not significantly associated with change in the level of coverage. However, Hispanics are more likely to lose coverage with the increase in the CMS wage index. Given that the coefficient on the Hispanic dummy in specifications 2 is 0.239, the negative coefficient on the interaction term between Hispanics and wage index means that Hispanics would be adversely impacted by the high health care cost if the real hourly wage in hospital sector increases by around 2.5% or higher.

Columns 4 through 6 use the medical care CPI as the proxy for health insurance premium. The sample size becomes much smaller because the

CPI is only available for those reside in 27 metropolitans (corresponding to roughly one-third of the CPS sample). Also, the whole 1995 wave of the February CPS is dropped as the matching between CPI and CPS is not feasible due to the lack of detail geographical information in the 1995 wave of CPS. Because the CPI captures both the expected benefit and load components of health insurance premium, the number in column 4 based on this restricted sample is more negative than the number in column 1 and is indeed significant.[25] Adding interactions in columns 5 and 6 yields similar results. Similar to columns 2 and 3, Hispanics are more likely to lose coverage when medical care services become more expensive. More interestingly, the effect of change in CPI on insurance coverage also differs by education level– less educated people are more likely to lose insurance when price increases.

Columns 7 and 8 in table 3.12 show the results when MEPS-IC state-level health insurance is used as the proxy for individual-level premium. The sample size is smaller than that in columns 1 through 3 because MEPS did not start until 1996, which causes the entire 1995 wave of the February CPS to be dropped. The numbers suggest that rising health insurance premium do not significantly decrease coverage, although the Hispanics are still more adversely affected than non-Hispanics. Similar to columns 5 and 6, people with more years of schooling are also less vulnerable to rising health insurance premiums. The estimates are similar if I exclude the ten less populous states which the MEPS under-sampled due to budget limitation in earlier waves (only the mean health insurance premium in these ten states, rather than the mean for each of the states, is available).

To summarize the results in table 3.12, the Hispanics are much more vul-

[25]Note that the coefficient still captures both positive and negative changes in demand caused by health insurance premium hike, so the “pure” effect of the increase in loading fee on coverage may be more negative.

nerable to coverage loss when the premium goes up, regardless of the proxies used. Besides, only the medical care CPI is negatively associated with health insurance coverage; such result is consistent with the dichotomy of expected benefit versus load, given the three proxies capture different components of the health insurance premium. However, the estimates in table 3.12 have coverage as the dependent variable and do not further differentiate each component of coverage. In order to account for the correlations of error terms across each stage to determine employer-sponsored health insurance coverage (employer offering, employee eligibility, and employee take-up), I next estimate equation (3.11) using sequential logistic models to assess the effects of rising health insurance premium on offering, eligibility, and take-up simultaneously. As in table 3.12, the CMS wage index, the medical care CPI, and the MEPS-IC state-level premium are separately used as proxies for the health care cost. The results are in table 3.13.

Table 3.13 is composed of estimates from six regressions. There are two regressions in each panel, one with no interactions (regression 1 in each panel) and one with interactions of race and the proxy for premium, race and the proxy, and the set of state-year interactions (regression 2). As in table 3.8, a sequential logistic model estimates the effects of variables of interest simultaneously for employer offering, employee eligibility and employee take-up. Panel A has the CMS wage index as the proxy for health insurance premium, panel B has the medical care CPI, and panel C has the MEPS-IC state-level health insurance premium.

The baseline estimates in regression 1 of panel A show that the change in wage index does not significantly affect offering, eligibility, and take-up. After all the interactions are entered, the estimates in regression 2 suggest a marginal increase in employer offering when premium cost increases. The effects on eligibility and take-up are essentially zero. While numbers in table 3.12 suggest the Hispanics are more likely to lose coverage when price increases, regression 2 of panel A in table 3.13 shows that most of the effect concentrates in employer offering. Indeed, both Hispanics and blacks are more likely to have employers not offering health insurance.[26] Although the coefficient on the interaction of Hispanics and wage index is more negative than the one on the interaction of blacks and wage index, these two coefficients are not statistically different. In regression 1 of panel B in table 3.13, increase in medical care CPI leads to a lower level of employee take-up of employer-sponsored health insurance plans. The coefficient of -0.016 is associated with a log-odds ratio of 0.983, yielding an elasticity of -0.017. These numbers also help to explain the pattern in figure 3.1 that racial gap grows when overall coverage declines– the negative coefficients on the interaction term of blacks/Hispanics and wage index/CPI in table 3.13 imply that minorities suffer from both lower offering and lower take up while whites only face lower take-up when price increases. Adding interactions to the regression 2 of panel B in table 3.13 does not change the results qualitatively, and Hispanics still are more adversely affected in employer offering when price increases. Finally, in panel C, an increase in state-level health insurance premium leads to lower employee take-up overall and lower offering to Hispanic workers; the estimates in panel C are qualitatively similar to those in panel

[26] There are several possible explanations to this. For example, the group health insurance premium tends to be higher for smaller firms, whose owners/employers arguably also have the most elastic demands for health insurance (Hadley and Reschovsky 2002). If the minorities are more likely to be hired by small firms, the observed coverage and offering outcome will be consistent with what are presented here. Another possible explanation is the Hispanics are less able to afford the health insurance premium when it goes up, and an employer may hence drop the offering if most of her employees do not plan to enroll in the insurance plan. And hence, employer's decision to drop offering may simultaneously reflect both employer's and employees' demand for health insurance. Unfortunately, the data in the CPS do not allow me to differentiate the demands of the employer and of the employees.

B.

To summarize, the dominant theory in health economics suggests there are two components in health insurance premium: the expected benefit and the load. An increase in the load decreases the quantity of demand for health insurance coverage, but higher expected benefit accompanied by a more skewed medical care expense distribution may increase the demand for coverage. Although the premium information is seldom available in nationally representative data (let alone separate identifications of these two components of premium cost), I use the CPS wage index, medical care CPI, and MEPS-IC state-level health insurance premium to proxy for the individual premium. The MEPS-IC premium information directly captures the overall relationship between premium increase and insurance coverage, while the CMS wage index and medical care CPI proxy for separate components of the health insurance premium. Because the medical care CPI is more correlated with the loading fee than the CMS wage index, a more negative effect on coverage is expected from the increase in CPI than in the wage index. The estimates in tables 3.12 and 3.13 are largely consistent with the prediction. Furthermore, the effect of the wage index concentrates on employer offering, and the medical care CPI mostly affects employee take-up of insurance plans. Minorities, especially the Hispanics, are adversely affected due to lower rate of employer offering when insurance premium cost increases.

3.5 Discussion and Conclusion

Since the end of the World War II, employer-sponsored health insurance plans have become the major source of private health insurance coverage for the American people. Even with the enactment of Medicare and Medicaid in 1965 and the recent expansions of public health insurance programs to cover low-income pregnant women and children, more than 50% of the U.S. population still obtain health insurance coverage through the their own or their immediate family member's employment. Nevertheless, just like the racial wage gap, there is a persistent health insurance coverage gap between the whites and the minorities, and the gap seems to have broadened slightly as the number of uninsured people went up in the past decade.

This study examines how the racial gap in employer-sponsored health insurance coverage changed between 1995 and 2005. Using the Contingent and Alternative Employment Arrangement Supplement to the Current Population Survey, which allows me to further decompose the employer-sponsored health insurance coverage into employer offering, employee eligibility, and employee take-up of the plan, I estimate how the trend and the racial gap in each component of the coverage changed during the 10-year span. Based on the variance decomposition method proposed in Fairlie (2005), I further assess how much of the change in the time trend and the racial gap coverage can be attributed to the difference of individual and job characteristics over time or across racial groups. Finally, the impact of rising health insurance premium cost is also addressed.

Major findings in this study include: (1) The racial gap in employer-sponsored health insurance coverage concentrates mostly on the gap in employer offering; (2) Around half of the racial gap in coverage and offering can be explained away by the racial discrepancies in individual and job characteristics; and (3) Depending on the proxy used, rising health insurance cost has no or small negative employer-sponsored health insurance coverage for employed males at age 18-64, but the Hispanics are especially hit with lower employer offering when price increases.

Despite the findings above, the scope of this study is limited by the data available in the Current Population Survey. Granted, there is no single source of data that is ideal for this study, and some trade-offs have to be made. For example, it is plausible that the health insurance coverage is correlated with wages, which in turn is correlated with an individual's labor market experience, so it would be good to have information on wages and labor market experiences. Nevertheless, although the March CPS covers a longer span of time and has more detailed information on wages and labor market experiences, it does not allow the decomposition of employer-sponsored health insurance coverage into offering, eligibility, and take-up. Also, one can argue that the CPS asks about the health insurance coverage in the previous year and hence may be contaminated with retrospective errors (Short 2004), so data that ask about the current coverage such as the Survey of Income and Program Participation (SIPP) or Health Retirement Study (HRS) may be more suitable. However, SIPP is not nationally representative (10 states are excluded from the survey), and the HRS simply does not have people younger than 50 years old. At last, health insurance coverage is more complicated than a dichotomy of insured versus uninsured, as health insurance plans differ in generosity and the level of contribution. Such information is not available in the CPS. The Medicare Expenditure Panel Survey (MEPS) does provide such information after its household and insurance components are matched, but not only does the match lead to a sample that is not nationally representative[27], it also has a smaller sample size and covers a shorter span of time. Consequently, albeit far from being perfect, the February CPS does seem to be one of the reasonable choices to initiate the study on racial

[27] The household and insurance components have different sample designs. When both have non-random attrition problems (namely non-responding), there is no natural way to construct the sample weight for those who can be matched across the two components.

gap in employer-sponsored health insurance coverage and set up for further research.

Several implications emerge from these results. First, although the anecdotal evidences suggest that high premium cost causes working people to drop health insurance and become uninsured, it is actually employer offering decisions that drive the trend in employer-sponsored health insurance coverage. And hence, in addition to making health insurance more affordable, policies intended to increase coverage should address employer offering as well. Second, while the black-white wage gap often stays at around 10% even after detail individual and job characteristics are controlled (Altonji and Blank 1999), the black-white gap in employer-sponsored health insurance coverage is either very small or insignificant after the same set of variables are controlled for. Such parity in health insurance coverage implies that focusing on the wage gap only would overstate the total compensation gap between blacks and whites by roughly 10%.[28] Finally, there is strong evidence in table 3.13 that Hispanics tend to work for employers who drop health insurance offering when the health insurance premium cost increases. Such racial disparity not only creates potential fairness issue in the workplace but also prompts a more thorough examination of firm compensation practices to identify the exact cause.

The theme of this study is to shed light on factors that explain the racial

[28]For instance, in 2010, the median income for the U.S. household is $43,000 and the median health insurance premium for family coverage is $12,000, yielding median compensation of $55,000. Based on a simple back-of-envelop calculation, a black-white wage gap of 10% and coverage gap of 6% (the largest adjusted black-white gap in table 3.3 divided by the respective level of coverage for whites) means on average blacks get $4,300 less in wages and $720 less in health benefits, leading to a compensation gap of only 9.1% even though the magnitude of the gap becomes larger. Consequently, focusing on the wage gap only overstates the total compensation gap by around 10%. With the recent trend that the cost of health benefits outgrow wages by five times, the racial compensation gap will shrink even faster and the racial wage gap is becoming a more misleading indicator of the total compensation gap.

gap in employer-sponsored health insurance coverage and to lay down the foundation upon which more studies in human resource management and employee benefits can build. The findings in this research provide insights regarding how employer offering decisions dominate the trend in coverage, the importance of education and job characteristics in explaining the racial gap, and how the Hispanics become more disadvantaged when health insurance premium cost increases. These results have implications on several literatures, including health insurance, racial compensation gap, human resource management, and employee benefits. Furthermore, these results also identify the directions to explore the more fundamental causes of the racial gap and raise some research issues that require different data sets with more information on wages and health insurance premiums. Embedded in the implications and further research questions are the call to a deeper understanding of the mechanism how employers decide to offer health insurance coverage, how employees decide to enroll in the plan, and how the mechanism differs across races. Such understanding underscores the importance of organizational-level studies regarding the employer's decision to provide health insurance benefits under the cost pressure, the design of health insurance plans, and the role of health insurance in the compensation package.

Figure 3.1: Trends of Employer-Sponsored Health Insurance Coverage and Racial Gaps over Time

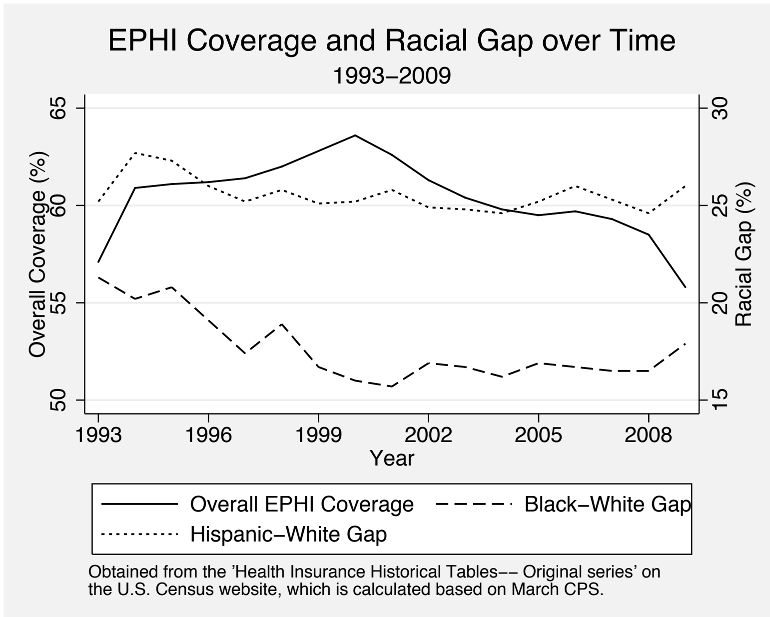

Table 3.1: Male Wage Earners Aged 20-64 in 1995-2005 Waves of the CPS Contingent and Alternative Employment Arrangement Supplement

Variables	**1995**	**1997**	**1999**	**2001**	**2005**
Age	38.07	38.54	39.05	39.19	39.97
White	0.772	0.754	0.742	0.732	0.697
Black	0.099	0.096	0.099	0.098	0.091
Hispanic	0.097	0.108	0.115	0.122	0.151
Other Races	0.030	0.041	0.042	0.046	0.059
Single	0.229	0.235	0.237	0.250	0.246
Married	0.669	0.659	0.652	0.646	0.648
Widowed	0.005	0.004	0.005	0.004	0.005
Divorced	0.078	0.081	0.087	0.080	0.083
Separated	0.017	0.019	0.017	0.017	0.016
HS Dropout	0.108	0.111	0.103	0.100	0.104
High School	0.323	0.323	0.312	0.301	0.309
Some College	0.287	0.284	0.285	0.288	0.278
College Degree	0.279	0.280	0.298	0.310	0.307
Full-Time Jobs	0.936	0.941	0.944	0.944	0.934
Sample Size	21,569	19,300	19,414	14,394	15,957

Descriptive statistics are weighted using the CPS weights.

Table 3.2: Fractions of the Male Wage Earners Aged 20-64 Covered by Any Health Insurance, 1995-2005

CPS Year	**All Workers**	**White Workers**	**Black Workers**	**Hispanic Workers**	**White-Black Difference**	**White-Hispanic Difference**	**White-Black Adjusted**	**White-Hispanic Adjusted**
1995	0.854	0.889	0.801	0.643	0.088	0.246	0.050	0.111
					(0.008)	(0.008)	(0.008)	(0.008)
1997	0.857	0.894	0.821	0.653	0.073	0.241	0.041	0.121
					(0.009)	(0.008)	(0.008)	(0.008)
1999	0.860	0.899	0.821	0.650	0.078	0.249	0.040	0.120
					(0.009)	(0.008)	(0.008)	(0.008)
2001	0.865	0.907	0.820	0.656	0.087	0.251	0.040	0.124
					(0.010)	(0.008)	(0.009)	(0.009)
2005	0.833	0.884	0.817	0.604	0.067	0.280	0.018	0.123
					(0.011)	(0.008)	(0.010)	(0.009)

The sample includes all male wage earners aged 20-64 who are not self-employed.

Descriptive statistics are weighted using the CPS supplement sample weights.

Standard errors in parentheses.

The adjusted differences in the last 2 columns are coefficients on dummies of black and Hispanic from separate OLS regressions for each year in which age (entered as 45 dummy variables rather than a linear term), marital status, education, type of job (full time versus part time), job tenure (more than one year versus less than one year), state of residence, and detailed CPS industry and occupation categories are controlled.

Table 3.3: Fractions of the Male Wage Earners Aged 20-64 Covered by Own Employer-Sponsored Health Insurance, 1995-2005

CPS Year	**All Workers**	**White Workers**	**Black Workers**	**Hispanic Workers**	**White-Black Difference**	**White-Hispanic Difference**	**White-Black Adjusted**	**White-Hispanic Adjusted**
1995	0.740	0.766	0.706	0.570	0.060	0.196	0.045	0.060
					(0.011)	(0.011)	(0.010)	(0.011)
1997	0.743	0.770	0.740	0.579	0.030	0.191	0.017	0.060
					(0.012)	(0.011)	(0.011)	(0.011)
1999	0.752	0.785	0.727	0.576	0.058	0.209	0.032	0.071
					(0.011)	(0.010)	(0.010)	(0.010)
2001	0.749	0.779	0.725	0.581	0.054	0.198	0.025	0.084
					(0.013)	(0.011)	(0.012)	(0.012)
2005	0.709	0.751	0.721	0.511	0.030	0.240	-0.004	0.078
					(0.014)	(0.011)	(0.013)	(0.011)

The sample includes all male wage earners aged 20-64 who are not self-employed.
Descriptive statistics are weighted using the CPS supplement sample weights.
Standard errors in parentheses.
The adjusted differences in the last 2 columns are coefficients on dummies of black and Hispanic from separate OLS regressions for each year in which age (entered as 45 dummy variables rather than a linear term), marital status, education, type of job (full time versus part time), job tenure (more than one year versus less than one year), state of residence, and detailed CPS industry and occupation categories are controlled.

Table 3.4: Offering of Employer-Sponsored Health Insurance to Employees Aged 20-64, 1995-2005

CPS Year	**All Workers**	**White Workers**	**Black Workers**	**Hispanic Workers**	**White-Black Difference**	**White-Hispanic Difference**	**White-Black Adjusted**	**White-Hispanic Adjusted**
1995	0.864	0.889	0.861	0.681	0.028	0.208	0.014	0.095
					(0.008)	(0.008)	(0.007)	(0.008)
1997	0.869	0.895	0.856	0.716	0.039	0.179	0.021	0.075
					(0.009)	(0.008)	(0.008)	(0.008)
1999	0.876	0.903	0.873	0.718	0.030	0.185	0.010	0.074
					(0.008)	(0.007)	(0.008)	(0.008)
2001	0.878	0.910	0.854	0.706	0.056	0.204	0.029	0.096
					(0.010)	(0.008)	(0.009)	(0.009)
2005	0.849	0.891	0.869	0.640	0.022	0.251	-0.007	0.112
					(0.010)	(0.008)	(0.010)	(0.009)

The sample includes all male wage earners aged 20-64 who are not self-employed.

Descriptive statistics are weighted using the CPS supplement sample weights.

Standard errors in parentheses.

The adjusted differences in the last 2 columns are coefficients on dummies of black and Hispanic from separate OLS regressions for each year in which age (entered as 45 dummy variables rather than a linear term), marital status, education, type of job (full time versus part time), job tenure (more than one year versus less than one year), state of residence, and detailed CPS industry and occupation categories are controlled.

Table 3.5: Eligibility of Employer-Sponsored Health Insurance Conditional on Offering, 1995-2005

CPS Year	**All Workers**	**White Workers**	**Black Workers**	**Hispanic Workers**	**White-Black Difference**	**White-Hispanic Difference**	**White-Black Adjusted**	**White-Hispanic Adjusted**
1995	0.947	0.949	0.940	0.938	0.009	0.011	-0.005	0.007
					(0.006)	(0.006)	(0.006)	(0.006)
1997	0.949	0.950	0.957	0.937	-0.007	0.013	-0.014	-0.009
					(0.006)	(0.006)	(0.006)	(0.006)
1999	0.952	0.958	0.931	0.925	0.027	0.033	0.013	0.012
					(0.006)	(0.005)	(0.005)	(0.006)
2001	0.957	0.958	0.948	0.955	0.010	0.003	0.000	-0.007
					(0.006)	(0.006)	(0.006)	(0.006)
2005	0.954	0.956	0.944	0.949	0.012	0.007	0.005	-0.009
					(0.007)	(0.006)	(0.006)	(0.006)

The sample includes all male wage earners aged 20-64 who are not self-employed.
Descriptive statistics are weighted using the CPS supplement sample weights.
Standard errors in parentheses.
The adjusted differences in the last 2 columns are coefficients on dummies of black and Hispanic from separate OLS regressions for each year in which age (entered as 45 dummy variables rather than a linear term), marital status, education, type of job (full time versus part time), job tenure (more than one year versus less than one year), state of residence, and detailed CPS industry and occupation categories are controlled.

Table 3.6: Take-up of Employer-Sponsored Health Insurance Conditional on Eligibility, 1995-2005

CPS Year	**All Workers**	**White Workers**	**Black Workers**	**Hispanic Workers**	**White-Black Difference**	**White-Hispanic Difference**	**White-Black Adjusted**	**White-Hispanic Adjusted**
1995	0.903	0.908	0.872	0.891	0.036	0.017	0.039	-0.015
					(0.008)	(0.009)	(0.008)	(0.010)
1997	0.900	0.905	0.903	0.863	0.002	0.042	0.012	0.009
					(0.009)	(0.009)	(0.009)	(0.010)
1999	0.901	0.907	0.894	0.867	0.013	0.040	0.011	0.002
					(0.009)	(0.008)	(0.009)	(0.009)
2001	0.891	0.894	0.894	0.862	0.000	0.032	0.003	0.016
					(0.010)	(0.010)	(0.011)	(0.011)
2005	0.875	0.881	0.878	0.840	0.003	0.041	-0.006	-0.000
					(0.011)	(0.010)	(0.011)	(0.011)

The sample includes all male wage earners aged 20-64 who are not self-employed.
Descriptive statistics are weighted using the CPS supplement sample weights.
Standard errors in parentheses.
The adjusted differences in the last 2 columns are coefficients on dummies of black and Hispanic from separate OLS regressions for each year in which age (entered as 45 dummy variables rather than a linear term), marital status, education, type of job (full time versus part time), job tenure (more than one year versus less than one year), state of residence, and detailed CPS industry and occupation categories are controlled.

Table 3.7: Decomposition of Changes in Employer-Sponsored Health Insurance Coverage, 1995-2005

Panel A: Overall Changes, 1995-2001				
	White	**Black**	**Hispanic**	**All Races**
Coverage	0.013	0.019	0.011	0.009
Offering	0.020	-0.007	0.025	0.014
Eligibility	0.013	0.008	0.017	0.010
Take-Up	-0.014	0.022	-0.029	-0.012
Panel B: Decomposition of Overall Changes, 1995-2001				
	White	**Black**	**Hispanic**	**All Races**
Coverage	0.013	0.019	0.011	0.009
Offering	0.017	-0.006	0.021	0.012
Eligibility	0.010	0.006	0.010	0.008
Take-Up	-0.012	0.018	-0.019	-0.010
Panel C: Overall Changes, 2001-2005				
	White	**Black**	**Hispanic**	**All Races**
Coverage	-0.028	-0.003	-0.069	-0.040
Offering	-0.018	0.015	-0.066	-0.029
Eligibility	-0.002	-0.004	-0.006	-0.003
Take-Up	-0.013	-0.016	-0.022	-0.016
Panel D: Decomposition of Overall Changes, 2001-2005				
	White	**Black**	**Hispanic**	**All Races**
Coverage	-0.028	-0.003	-0.069	-0.040
Offering	-0.015	0.013	-0.054	-0.025
Eligibility	-0.002	-0.003	-0.004	-0.002
Take-Up	-0.011	-0.013	-0.015	-0.013

Table 3.8: Sequential Logit Estimates of Racial Gaps in Employer Offering, Eligibility, and Take-up over Time

Panel A: 1995-2001			
	Offering	**Eligible**	**Take-up**
Black-White Gap	-0.056	0.025	0.087
	(0.021)	(0.040)	(0.028)
Hispanic-White Gap	-0.021	-0.003	-0.030
	(0.017)	(0.038)	(0.025)
Panel B: 2001-2005			
	Offering	**Eligible**	**Take-up**
Black-White Gap	0.103	-0.080	-0.005
	(0.038)	(0.064)	(0.042)
Hispanic-White Gap	-0.008	0.008	0.002
	(0.024)	(0.059)	(0.034)

Control variables are the same as in the last two columns of tables 2-6.

Table 3.9: Non-Linear Variance Decomposition of Racial Gaps in Employer-Sponsored Health Insurance Coverage

Panel A: White-Black Coverage Gap					
	1995	**1997**	**1999**	**2001**	**2005**
White Coverage Rate	0.766	0.770	0.785	0.779	0.751
Black Coverage Rate	0.706	0.740	0.727	0.725	0.721
White-Black Gap	0.060	0.030	0.058	0.054	0.030
Contributed by Racial Differences in:					
Age	0.006	0.004	0.002	0.006	0.002
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
	10.13%	14.33%	4.33%	12.63%	9.93%
Marital Status	-0.003	-0.000	-0.001	-0.003	-0.002
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
	-4.98%	-2.33%	-2.42%	-5.49%	-9.58%
Education	0.008	0.012	0.012	0.012	0.011
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
	14.11%	42.00%	21.83%	23.26%	39.72%
Residence	0.002	-0.001	-0.000	-0.005	0.001
	(0.002)	(0.000)	(0.003)	(0.003)	(0.003)
	4.81%	-4.00%	-0.69%	-10.07%	5.47%
Job Type	0.005	0.005	0.008	0.013	0.005
	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)
	8.63%	16.67%	13.86%	24.54%	18.49%
Ind. and Occ.	0.002	0.007	-0.002	-0.004	-0.000
	(0.002)	(0.002)	(0.002)	(0.000)	(0.000)
	3.98%	25.33%	4.15%	-7.32%	-0.68%
All Variables	0.022	0.027	0.018	0.020	0.018
	36.67%	91.33%	32.40%	54.32%	64.04%
Panel B: White-Hispanic Coverage Gap					
	1995	**1997**	**1999**	**2001**	**2005**
White Coverage Rate	0.766	0.770	0.785	0.779	0.751
Hispanic Coverage Rate	0.570	0.579	0.576	0.581	0.511
White-Hispanic Gap	0.196	0.191	0.209	0.198	0.240
Contributed by Racial Differences in:					
Age	0.014	0.009	0.010	0.007	0.011
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
	7.31%	5.02%	4.92%	3.72%	4.66%
Marital Status	0.000	0.000	0.001	0.002	0.000
	(0.000)	(0.001)	(0.001)	(0.001)	(0.000)
	0.10%	0.31%	0.62%	1.00%	0.25%
Education	0.032	0.035	0.042	0.047	0.045
	(0.005)	(0.005)	(0.005)	(0.007)	(0.007)
	16.67%	18.53%	20.19%	23.75%	18.87%
Residence	-0.000	0.014	0.011	-0.001	0.001
	(0.005)	(0.005)	(0.005)	(0.006)	(0.005)
	-0.35%	7.64%	5.50%	-0.85%	5.83%
Job Type	0.025	0.020	0.010	0.013	0.021
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
	13.10%	10.89%	5.16%	6.84%	8.83%
Ind. and Occ.	0.059	0.062	0.054	0.043	0.069
	(0.000)	(0.004)	(0.005)	(0.005)	(0.005)
	30.43%	32.82%	26.22%	21.84%	29.08%
All Variables	0.132	0.144	0.131	0.112	0.149
	67.42%	75.39%	62.77%	56.56%	62.45%

Table 3.10: Non-Linear Variance Decomposition of Racial Gaps in Offering of Employer-Sponsored Health Insurance

Panel A: White-Black Offering Gap					
	1995	**1997**	**1999**	**2001**	**2005**
White Offering Rate	0.889	0.895	0.903	0.910	0.891
Black Offering Rate	0.861	0.856	0.873	0.854	0.869
White-Black Gap	0.028	0.039	0.030	0.056	0.022
Contributed by Racial Differences in:					
Age	0.004	0.001	0.003	0.003	0.003
	(0.001)	(0.000)	(0.001)	(0.001)	(0.001)
	15.35%	4.33%	13.04%	5.75%	17.80%
Marital Status	0.002	0.006	0.002	0.007	0.004
	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)
	9.28%	17.34%	9.36%	12.94%	21.46%
Education	0.007	0.011	0.008	0.015	0.006
	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)
	25.00%	29.08%	27.42%	27.69%	30.59%
Residence	0.009	0.004	0.004	0.007	0.002
	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)
	33.57%	10.91%	14.04%	13.35%	10.04%
Job Type	0.007	0.005	0.007	0.007	0.006
	(0.000)	(0.001)	(0.001)	(0.001)	(0.000)
	25.00%	13.77%	24.94%	14.25%	27.39%
Ind. and Occ.	-0.009	-0.007	-0.013	-0.005	-0.002
	(0.002)	(0.002)	(0.002)	(0.000)	(0.002)
	-34.28%	-18.62%	-46.15%	-9.74%	-10.95%
All Variables	0.021	0.022	0.013	0.035	0.021
	75.00%	57.39%	43.47%	64.07%	97.26%
Panel B: White-Hispanic Offering Gap					
	1995	**1997**	**1999**	**2001**	**2005**
White Offering Rate	0.889	0.895	0.903	0.910	0.891
Hispanic Offering Rate	0.681	0.716	0.718	0.706	0.640
White-Hispanic Gap	0.208	0.179	0.185	0.204	0.251
Contributed by Racial Differences in:					
Age	0.005	0.000	0.005	0.003	0.005
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
	2.55%	0.16%	2.69%	1.86%	2.34%
Marital Status	0.000	0.000	0.000	0.001	0.000
	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)
	0.09%	0.50%	0.48%	0.88%	0.19%
Education	0.025	0.031	0.037	0.047	0.027
	(0.004)	(0.005)	(0.005)	(0.006)	(0.006)
	12.15%	17.48%	20.01%	23.28%	11.11%
Residence	0.012	0.020	0.009	0.020	0.013
	(0.005)	(0.005)	(0.004)	(0.006)	(0.005)
	5.83%	11.67%	5.33%	10.24%	5.17%
Job Type	0.016	0.011	0.006	0.009	0.009
	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)
	8.05%	6.59%	3.66%	4.85%	3.86%
Ind. and Occ.	0.049	0.051	0.040	0.040	0.071
	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)
	23.68%	28.65%	21.68%	20.04%	28.27%
All Variables	0.109	0.116	0.099	0.124	0.128
	52.40%	65.13%	53.88%	61.22%	51.05%

Table 3.11: Non-Linear Variance Decomposition of Racial Gaps in Coverage and Offering of Employer-Sponsored Health Insurance over Time

Panel A: 1995-2001						
	White Coverage	**Black Coverage**	**Hispanic Coverage**	**White Offering**	**Black Offering**	**Hispanic Offering**
1995	0.766	0.706	0.570	0.889	0.861	0.681
2001	0.779	0.725	0.581	0.910	0.854	0.706
Change over Time	0.012	0.019	0.011	0.021	-0.007	0.025
Contributed by Changes in:						
Age	0.002	0.024	0.002	0.003	0.018	0.005
	(0.001)	(0.006)	(0.004)	(0.001)	(0.006)	(0.005)
	20.31%	127.29%	25.00%	15.94%	-287.69%	21.13%
Marital Status	0.002	0.004	-0.002	0.001	-0.000	-0.002
	(0.000)	(0.002)	(0.001)	(0.000)	(0.003)	(0.001)
	15.62%	23.28%	-20.68%	4.83%	9.23%	-11.78%
Education	0.004	0.008	0.002	0.006	-0.000	0.003
	(0.001)	(0.004)	(0.001)	(0.001)	(0.002)	(0.002)
	37.50%	42.85%	19.82%	29.95%	10.76%	12.60%
Residence	0.001	0.002	0.010	0.002	0.001	0.007
	(0.000)	(0.005)	(0.003)	(0.000)	(0.008)	(0.003)
	9.37%	11.64%	88.79%	14.00%	-29.23%	31.70%
Job Type	0.008	-0.013	0.007	0.004	-0.019	0.004
	(0.001)	(0.004)	(0.003)	(0.001)	(0.004)	(0.004)
	67.18%	-72.48%	67.24%	23.67%	301.53%	19.10%
Ind. and Occ.	-0.014	-0.008	-0.013	-0.015	0.049	-0.015
	(0.001)	(0.008)	(0.006)	(0.000)	(0.001)	(0.002)
	-116.40%	-42.85%	-118.96%	-72.46%	-766.15%	-63.82%
All Variables	0.004	0.017	0.007	0.003	0.050	0.002
	35.93%	91.00%	60.34%	17.39%	-770.76%	10.16%
Panel B: 2001-2005						
	White Coverage	**Black Coverage**	**Hispanic Coverage**	**White Offering**	**Black Offering**	**Hispanic Offering**
2001	0.779	0.725	0.581	0.910	0.854	0.706
2005	0.751	0.721	0.511	0.891	0.869	0.640
Change over Time	-0.028	-0.004	-0.070	-0.019	0.015	-0.066
Contributed by Changes in:						
Age	0.003	0.016	0.004	0.004	0.020	0.007
	(0.000)	(0.007)	(0.004)	(0.000)	(0.011)	(0.004)
	-13.24%	-496.96%	-7.00%	-24.04%	131.16%	-11.46%
Marital Status	0.000	-0.000	-0.000	0.000	0.003	-0.000
	(0.000)	(0.002)	(0.001)	(0.000)	(0.004)	(0.001)
	-1.74%	15.15%	0.71%	-2.18%	20.12%	0.30%
Education	0.001	-0.001	-0.001	0.001	0.000	-0.001
	(0.000)	(0.002)	(0.001)	(0.000)	(0.003)	(0.001)
	-4.87%	48.48%	2.57%	-4.91%	0.64%	1.83%
Residence	0.000	0.002	-0.000	0.002	0.014	0.004
	(0.000)	(0.005)	(0.003)	(0.000)	(0.007)	(0.003)
	-2.43%	-71.72%	0.04%	-12.56%	92.20%	-6.42%
Job Type	0.006	0.016	-0.000	0.003	0.001	0.002
	(0.001)	(0.001)	(0.002)	(0.000)	(0.006)	(0.002)
	-22.99%	-493.75%	0.11%	-20.21%	11.03%	-3.21%
Ind. and Occ.	-0.017	-0.029	-0.018	-0.015	-0.013	-0.024
	(0.010)	(0.009)	(0.002)	(0.001)	(0.016)	(0.006)
	61.67%	881.54%	25.71%	85.79%	-87.66%	36.85%
All Variables	-0.004	0.003	-0.016	-0.003	0.025	-0.011
	14.98%	-90.75%	23.00%	20.21%	167.53%	18.04%

Table 3.12: Impacts of Health Insurance Premium Cost on Employer-Sponsored Health Insurance Coverage and the Racial Gap, Linear Probability Models

	Linear Probability Models							
	CMS Wage Index			**Medical Care CPI**			**MEPS Premium**	
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
CMS Wage Index	0.011	0.015	0.037					
	(0.018)	(0.022)	(0.022)					
Black X Index		-0.010	-0.007					
		(0.027)	(0.028)					
Hispanic X Index		-0.110**	-0.104**					
		(0.032)	(0.032)					
No H.S. X Index		-0.034	-0.032					
		(0.034)	(0.034)					
Some Coll. X Index		0.018	0.017					
		(0.019)	(0.019)					
College X Index		0.029	0.032					
		(0.021)	(0.021)					
Medical Care CPI				-0.002**	-0.003**	-0.003**		
				(0.001)	(0.001)	(0.001)		
Black X CPI					0.000	-0.000		
					(0.001)	(0.001)		
Hispanic X CPI					-0.002*	-0.002*		
					(0.001)	(0.001)		
No H.S. X CPI					-0.002*	-0.002*		
					(0.001)	(0.001)		
Some Coll. X CPI					0.002*	0.002*		
					(0.001)	(0.001)		
College X CPI					0.002**	0.002**		
					(0.001)	(0.001)		
MEPS Premium							-0.042	-0.069
							(0.042)	(0.044)
Black X Premium								0.039
								(0.036)
Hispanic X Prem.								-0.068**
								(0.034)
No H.S. X Premium								-0.050
								(0.050)
Some Coll. X Prem.								0.061**
								(0.024)
College X Premium								0.063**
								(0.024)
Black	-0.024**	0.007	-0.004	-0.030**	-0.030	-0.027	-0.018**	-0.380
	(0.005)	(0.081)	(0.085)	(0.011)	(0.096)	(0.096)	(0.006)	(0.331)
Hispanic	-0.100**	0.239**	0.221**	-0.105**	0.126	0.138	-0.102**	0.522
	(0.006)	(0.098)	(0.100)	(0.008)	(0.129)	(0.132)	(0.007)	(0.318)
State X Year	N	N	Y	N	N	Y	N	N
N	86,302	86,302	86,302	21,940	21,940	21,940	65,379	65,379
R^2	0.1862	0.1865	0.1888	0.1892	0.1904	0.1938	0.1821	0.1824

The sample includes all male wage earners aged 20-64 who are not self-employed.
Dependent variable is a dummy indicating whether one has own employer-sponsored health insurance coverage.
Standard errors are in parentheses and clustered by states and years of observation.
Control variables include age (entered as 45 dummy variables rather than a linear term), race, marital status, education, job characteristics (full time versus part time, more than one year versus less than one year, covered by collective bargaining contracts versus not covered, and public versus private sector), state of residence, and year of observation.

Table 3.13: Impacts of Health Insurance Premium Cost on Employer-Sponsored Health Insurance Coverage and the Racial Gap, Sequential Logit Models

	Panel A: CMS Wage Index					
	Regression 1: No Interactions			Regression 2: With Interactions		
	Offering	**Eligible**	**Take-up**	**Offering**	**Eligible**	**Take-up**
CMS Wage Index	0.168	-0.051	0.021	0.730*	0.038	0.025
	(0.244)	(0.184)	(0.168)	(0.374)	(0.259)	(0.250)
Black	-0.092**	-0.104	-0.179**	1.975**	-1.759	-1.616
	(0.044)	(0.078)	(0.049)	(0.628)	(1.175)	(0.924)
Hispanic	-0.728**	-0.020	-0.096	1.914**	0.354	0.812
	(0.045)	(0.067)	(0.053)	(0.674)	(1.212)	(0.741)
Black X Wage Index				-0.695**	0.545	0.478
				(0.210)	(0.385)	(0.304)
Hispanic X Wage Index				-0.867**	-0.119	-0.296
				(0.219)	(0.388)	(0.242)
	Panel B: Medical Care CPI					
	Regression 1: No Interactions			Regression 2: With Interactions		
	Offering	**Eligible**	**Take-up**	**Offering**	**Eligible**	**Take-up**
Medical Care CPI	-0.010	-0.021	-0.016**	-0.012	-0.021	-0.021**
	(0.007)	(0.015)	(0.006)	(0.008)	(0.018)	(0.009)
Black	-0.220**	-0.010	-0.172*	0.098	1.196	-0.709
	(0.087)	(0.144)	(0.088)	(0.925)	(1.475)	(0.868)
Hispanic	-0.792**	0.152	-0.149*	1.084	-0.472	0.629
	(0.066)	(0.118)	(0.083)	(0.852)	(1.859)	(1.063)
Black X Medical Care CPI				-0.003	-0.013	0.006
				(0.010)	(0.016)	(0.009)
Hispanic X Medical Care CPI				-0.021**	0.007	0.005
				(0.009)	(0.021)	(0.012)
	Panel C: MEPS Health Insurance Premium					
	Regression 1: No Interactions			Regression 2: With Interactions		
	Offering	**Eligible**	**Take-up**	**Offering**	**Eligible**	**Take-up**
MEPS Insurance Premium	0.083	0.248	-0.803**	0.205	0.232	-1.260**
	(0.386)	(0.668)	(0.377)	(0.405)	(0.703)	(0.394)
Black	-0.110**	-0.120	-0.091	-2.257	8.164	-4.502
	(0.055)	(0.089)	(0.056)	(2.536)	(4.274)	(2.627)
Hispanic	-0.734**	-0.003	-0.135**	6.424**	-1.394	-3.307
	(0.053)	(0.076)	(0.060)	(2.010)	(4.202)	(3.132)
Black X MEPS Insurance Premium				0.235	-0.909	0.483
				(0.278)	(0.468)	(0.287)
Hispanic X MEPS Insurance Premium				-0.783**	0.152	0.347
				(0.217)	(0.463)	(0.344)

The sample includes all male wage earners aged 20-64 who are not self-employed.
Dependent variable is a dummy indicating whether one has own employer-sponsored health insurance coverage.
Standard errors are in parentheses and clustered by states and years of observation.
Control variables include age (entered as 45 dummy variables rather than a linear term), race, marital status, education, job characteristics (full time versus part time, more than one year versus less than one year, covered by collective bargaining contracts versus not covered, and public versus private sector), state of residence, and year of observation. State-year interactions are also included but only in panels A and B.

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